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Yet Another Refutation of the More Guns, Less Crime Hypothesis – With Some Help From Moody and Marvell

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Abstract

Armed with the weight of a single new regression for each of seven crime categories, Carlisle Moody and Thomas Marvell (2008) conclude their remarkable paper, "The Debate on Shall-Issue Laws," stating that they are "confident" that "the evidence, such as it is, seems to support the hypothesis that the shall-issue law is generally beneficial with respect to its overall long run effect on crime" (292). The paper is remarkable because the evidence Moody and Marvell present thoroughly undermines (yet again) the conclusion that RTC laws "generally" have any beneficial effect on crime.

Moody and Marvell essentially make four points, which simultaneously grow in the level of both their ambition and error. First, the authors present a somewhat tendentious summary of the previous research evaluating the more guns, less crime hypothesis. Moody and Marvell affectionately summarize the shrinking and aging portion of the literature that purports to find that "right-to-carry" (RTC) laws allowing citizens to carry concealed handguns reduce crime. Moody and Marvell are less thorough and generous in their summary of the ever-expanding literature questioning the more guns, less crime hypothesis. Second, they provide an incomplete and somewhat inaccurately categorized list of studies that is apparently designed to suggest numerical superiority supporting their own views on the impact of RTC laws. With an appropriate quality-adjustment, how-

ever, most of the "supportive" studies on their list would be deemed to have little or no current value.

Third, Moody and Marvell turn their gaze to Ayres and Donohue (2003a), which in 119 pages arrayed an enormous amount of information raising doubts about the more guns, less crime hypothesis. Moody and Marvell ignore virtually all of this discussion and instead challenge a single table, which showed that using state-specific regression estimates from 1977-1997 county data implied that RTC laws increased crime costs over the period up to five years following adoption. Of course, if the challenge succeeded, we might be inclined to ask whether Moody and Marvell were now ready to contend with the rest of that article. Moody and Marvell have a tall mountain to climb if they are to reach their destination, but they have failed even in their first step.

The basis of their challenge is that Ayres and Donohue's estimated five-year post-passage linear trend should be extrapolated further. Such extrapolation is a perilous enterprise. In the single table Moody and Marvell criticize, we were estimating post-passage trends for 25 jurisdictions when we only had 1-3 years of post-passage data for 12 of these 25 states.[3] Accordingly, we felt we were already pushing the boundaries of what the data allowed by extrapolating out to five years.

Indeed, given the fact that the falling crime rates of the 1990s came to an end by the year 2000, the idea that we should engage in extended extrapolations of a linear trend beyond 1997 makes little sense. This is particularly true since the presence of more recent crime data now enables researchers to extend the data forward well past 2000 when the crime trend flattened out and even turned up, particularly in many jurisdictions adopting RTC laws (see Figures 1-3, below). Making use of more recent data is clearly preferable to engaging in the risky enterprise of trying to extrapolate so far beyond the data that was previously employed. Therefore, the discussion of the Ayres-Donohue crime calculations on 1997 data would seem to be beside the point since county data extending three years past that date now has been used by Ayres and Donohue (2003b), Moody and Marvell (2009), and the National Research Council (2004), and even more data is now available to extend the 1977-2000 dataset, thereby reducing the need for extrapolation.

Fourth, armed with the 1977-2000 county dataset, Moody and Marvell seem to conclude that the correct path to establishing the impact of RTC laws is to follow the estimation approach of the Ayres and Donohue table just discussed, which used a state-specific hybrid model with state trends on county da-

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3 Table 7 of Ayres and Donohue (2003a, 1242) shows that in our *state* dataset, which extended through 1999, we had at least *three* years of post-passage data for all RTC states. Our *county* data in that paper extended only through 1997, however, thereby limiting us to *one* year of post-passage data for the three states that adopted RTC laws in 1996, *two* years of data for the five adopters in 1995, and *three* years for the four adopters in 1994.

ta.[4] Moody and Marvell ignore the concerns Ayres and Donohue (2003a, 1259) expressed about the use of county data, and proceed with their own specification that contains two suggested improvements – adding a measure designed to capture the criminogenic influence of crack and adding a lagged dependent variable. Based on the state-specific estimates they generate from this new model, Moody and Marvell conclude that RTC laws are beneficial because one state – Florida – outweighs the overall harmful effects estimated for the other 23 jurisdictions. We discuss below, however, how their own estimates powerfully undercut their suggestion that RTC laws are generally beneficial. Moody and Marvell then labor to refute their own findings by once again unwisely extrapolating linear trends beyond the period of their data.

I. Moody and Marvell's Inaccurate and Incomplete Literature Review

A. The Original Sin: Lott and Mustard's Data Ended in 1992

Moody and Marvell begin with a reference to the initial 1997 Lott and Mustard paper, which originated the claim that RTC laws reduce crime. In that paper, which spawned a robust literature, Lott and Mustard did a creditable panel data analysis of RTC laws for the period 1977-1992. It has now become clear, however, that the timing of the Lott and Mustard's study was fatal to their mission. Crime rose sharply in the late 1980s and the early years of the 1990s. Lott and Mustard's data period caught the sharp run up in crime but not the huge decline that started just when their data series ended.

Trying to estimate the impact of an intervention affecting crime during a massive, unexplained, geographically disparate crime boom is a daunting task if the intervention is not randomly assigned. RTC laws were *not* randomly adopted: RTC law adoptions were clearly influenced by the patterns of crime that were unfolding around the country during that turbulent time. Indeed, the latest published study examining the impact of RTC laws on murder – which does not appear on Moody and Marvell's list of studies refuting the more guns, less crime hypothesis -- found that "in the five-year period before law passage, the 25 states passing shall-issue laws between 1981 and 1996 had an increasing trend on average in murder rates relative to the U.S. murder rate. [R]ather than a random sample of slopes, we have selection for positive slopes. State governments tended to pass shall-issue laws when murder rates were relatively increasing" (Grambsch 2008: 292-93).

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4 A hybrid model estimates both a change in levels of crime (like the dummy model) as well as a shift in the trend in crime (like the trend model).

B. State v. County Data and the Lott, Plassmann, and Whitley Coding Errors Fiasco

In their discussion of the contribution to the RTC literature of Ayres and Donohue (2003a), Moody and Marvell focus only on that paper's state-specific county data analysis. In so doing, they neglect the 53 pages of analysis of state data for the period 1977-1999, and fail to acknowledge, let alone address, the Ayres and Donohue claim that errors in the county data raise concerns about whether it is advisable to prefer regression estimates on state crime data.[5] Similarly, Moody and Marvell have not addressed a litany of other cautions about model specification and data reliability that were discussed at length in that paper. Instead, Moody and Marvell tackle only one part of a much longer paper, which they describe as "the method of Ayres and Donohue."

Thus, Moody and Marvell (274) tell us that they "offer a fresh statistical analysis based on the method of Ayres and Donohue, but our investigation improves the method and extends the data through 2000 (Ayres and Donohue's data was through 1997)." But their literature review neglects to mention that we did analyze the very county data set that they employ (hence with data through 2000) in Ayres and Donohue (2003b). Moody and Marvell also fail to recount the history of the flawed county data set and its impact on the work of Lott and Plassmann and Whitley (2003). John Lott originally created this 1977-2000 county data set for his 2003 book, *The Bias Against Guns*.[6] Lott then joined with Plassmann and Whitley to respond to Ayres and Donohue (2003a), once again relying on this same Lott-created county data set. As we detailed in Ayres and Donohue (2003b), the Lott, Plassmann, and Whitley reply used this expanded county data in an attempt to show that RTC laws reduced crime. We uncovered numerous coding errors in their data set. Correcting these errors reversed the conclusions of the Lott, Plassmann, and Whitley reply and revealed that there was *no* evidence that RTC laws reduced crime.

5 Missing data is a much bigger concern in the county data than in the state data. A leading researcher on the quality of the state and county crime data, Michael Maltz, deems the county data to be severely flawed, especially if one extends the county data across the break in the series that occurred in 1994 when new imputation procedures were adopted for county crime data (Maltz 2006; Ayres and Donohue 2003b: 1392). These concerns are particularly problematic since the states with the worst data are more likely to have RTC laws:

> County-level crime data cannot be used with any degree of confidence…The crime rates of a great many counties have been underestimated, due to the exclusion of large fractions of their populations from contributing to the crime counts. Moreover, counties in those states with the most coverage gaps have laws permitting the carrying of concealed weapons. How these short-comings can be compensated for is still an open question, one that we are attempting to answer in our ongoing study of different methods of imputation. It is clear, however, that in their current condition, county-level UCR crime statistics cannot be used for evaluating the effects of changes in policy (Maltz and Targonski 2002: 316-317).

6 Lott (2003; Appendix Figures 1.3-1.8, 237-239) presents estimates of the effects of right-to-carry gun legislation on violent and property crimes, analyzing effects for the years 1977 to 2000.

Specifically, Table 3B of Ayres and Donohue (2003b) re-ran on corrected data the identical regressions that Lott, Plassmann, and Whitley had originally presented, which were subsequently published in Plassmann and Whitley (2003). When we corrected the coding errors in this Lott data set, *all* of the results presented in the attack against us by Plassmann and Whitley (2003) vanished. Table 3B of our 2003 paper illustrates that, across nine crime categories, whether one used a dummy model, a spline model, or a hybrid combination of the two (both a dummy and a spline), there was not a single statistically significant coefficient that would be suggestive of a *decrease* in crime. Instead, Table 3B demonstrated that the only statistically significant results from the county data suggested RTC laws *increased* property crime, auto theft, and larceny.

Amazingly, Moody and Marvell cite Plassmann and Whitley (2003) in their list of articles finding that "Shall-Issue Reduces Crime," even though everyone now recognizes that all of the regressions used in that paper were using seriously flawed data (Mooney 2003, 2005).[7]

As we stated in the last paragraph of our comment documenting the serious coding errors in Plassmann and Whitley's paper (previously Lott, Plassmann, and Whitley): "These serious data errors infect *every* regression presented in the PW response. Consequently, researchers and policymakers should not rely on any of the new regressions that PW present in their response" (Ayres and Donohue 2003b: 1394). Apparently, that message did not come through to Moody and Marvell, who make no reference to these problems as they blithely cite Plassmann and Whitley's paper as supporting the more guns, less crime hypothesis.

C. The NRC Report Mischaracterized

In the summer of 2001, a committee of 16 experts began a two-year evaluation of the literature and data concerning the impact of RTC laws, which culminated in a report for the National Research Council (2004). In referencing this report, Moody and Marvell make the very misleading statement: "the Committee did some independent analyses that indicated that shall-issue laws reduce murder." Like Moody and Marvell, one member of that committee – James Q. Wilson – decided he would ignore the lessons of the vast literature on the impact of RTC laws and just focus narrowly on a few isolated regression results to draw a conclusion that appears absurd in the context of the broader knowledge that has been revealed by the extensive literature. Wilson, a non-econometrician who reviewed the evidence in an oddly wooden and unsophisticated way (while saying he wished he knew more econometrics), found that one could not conclude anything about the impact of the RTC law on any crime, except that RTC laws reduced murder. The

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7 After we pointed out that our corrections of Lott's errors had rendered statistically insignificant all of the Lott, Plassmann, and Whitley results, Lott removed his name from the reply to Ayres and Donohue (2003a), which then was published – with the errors uncorrected – as Plassmann and Whitley (2003).

other 15 members of the Committee – which included an array of top econometricians, including John Bates Clark award winner Steve Levitt, Joel Waldfogel of Wharton, Joel Horowitz of Northwestern – unanimously responded to Wilson's puzzling and unconvincing statement by noting that "The scientific evidence does not support his conclusion" (2004, 275).

D. More Counting of Useless Studies

Moody and Marvell's exercise of counting the number of studies supporting Lott also seems to have missed the finding, emphasized in Ayres and Donohue (2003) and the NRC report (see Tables 6-5 and 6-6), that estimates based on data only through 1992 are of questionable value since adding additional years of data invariably weakens Lott's initial findings. In fact, 7 of the 10 references that Moody and Marvell cite as "referred journal articles and books" that support the more guns, less crime hypothesis were based on the exact same Lott and Mustard dataset ending in 1992, just prior to the great American crime drop of the 1990s.[8] Relatively little is left of the literature supporting the more guns, less crime hypothesis if you subtract out the studies examining RTC laws that analyze data only through 1992.

Each year that one extends the Lott and Mustard data set predictably weakens their claim. This is not surprising, given the fact that the Lott and Mustard apparent finding of a beneficial effect of RTC laws was caused not by having more armed citizens out on the streets but rather because other forces not well controlled in the RTC literature had caused crime to soar in the states that did not adopt RTC laws. Figures 1-3 document the pronounced crime increase, later reversed, that occurred in the late 1980s in the states that did not adopt these laws. Of course, numerous explanations have been given for the sharp crime drops in the 1990s, which were particularly great in the non-RTC states – perhaps mean reversion due to the end of the crack boom, the legalization of abortion, better policing, a tipping effect from the decades long growth in incarceration, the superheated economy, or some combination of these. Notice that none of these listed factors are adequately captured in any of the studies that Moody and Marvell reference. If any of these omitted factors played an important role in reducing crime and are correlated with RTC law adoption, then one must be very cautious in interpreting regression estimates of the impact of RTC laws. States with a major crack problem were, on the whole, less willing to roll the dice by adopting a RTC law in the late 1980s or early 1990s.

E. Moody and Marvell Overlook Some Important Studies – and Lessons

8 Lott and Mustard (1997), Bartley and Cohen (1998), Bronars and Lott (1998), Benson and Mast (2001), Olsen and Maltz (2001), Plassmann and Tideman (2001), and Lott (1998).

Moody and Marvell fail to cite Donohue (2004), which illustrated that model sensitivity plagues virtually all of the studies that they list as supportive of the more guns, less crime hypothesis. Using state level panel data for the period from 1977 – 1999, Donohue (2004) underscored how sensitive the estimated effects of RTC laws were to minor alterations in the Lott and Mustard specification. This lack of robustness creates the concern that a researcher will erroneously fixate on a specification that generates an appealing estimate. To address this fear, Donohue supplemented his analysis of a modified version of the Lott and Mustard model by examining the crime models previously employed by three other research papers that analyzed interventions *other* than RTC laws. An array of different specifications were estimated for each of the four different crime models, and the striking fact was that for a number of the crime categories, the estimates were all over the map – some positive, some negative, some significant, some not significant. Interestingly, though, the vast bulk of the estimated effects for the different permutations on the four basic crime models were suggestive of crime *increases* caused by RTC laws for seven of the nine FBI Index I crime categories. (For murder the estimates were more mixed, and for rape RTC laws were associated – albeit not necessarily causally – with crime declines.)

Even for robbery – the crime for which one would most suspect that RTC laws would cause crime to fall if the Lott and Mustard thesis were true – Donohue (2004; Figure 4, 646) presented a very discouraging picture for the more guns, less crime hypothesis. Forty of the 48 different assessments depicted therein suggested RTC laws *increased* robbery. Of course, it is true that one could pick one of the eight specifications that was more supportive of the Lott and Mustard thesis. One had better have a very good reason, though, for preferring an outlier estimate against the weight of the evidence to the contrary.[9]

Nor do Moody and Marvell reference Grambsch (2008), which argues that the endogenous adoption of RTC laws during periods of spiking murder rates that will ultimately regress to the mean has biased the estimates of the impact of these laws in the direction of finding a benefit. Grambsch summarizes her finding as follows: "We find that controlling for regression to the mean changes the sign of the estimated intervention effect on murder rates slopes from negative to positive, has strong impact on statistical significance, and *gives no support to the hypothesis that shall-issue laws have beneficial effects in reducing murder rates*" (289; italics supplied).

II. Moody and Marvell Add a New Specification While Analyzing an Old Data Set

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9 Strnad (2007) has an interesting discussion of Bayesian techniques that can be used to choose among different specifications used in estimating the impact of RTC laws.

Moody and Marvell attempt to shed new light on the impact of RTC laws by using a specification that builds on the basic structure of the initial Lott and Mustard specification and then mimics one of the numerous approaches offered in Ayres and Donohue (2003a) – a hybrid, state-specific model which included a control for state prison population – while adding two new variables. Once again, they are estimating this tweaked model on our corrected version of Lott's original county data set, which has previously been analyzed by Ayres and Donohue (2003b) and the panel of the National Research Council (2004).[10]

So what are the two new variables? Moody and Marvell add a control designed to capture the criminogenic influence of crack on crime and they introduce a lagged dependent variable into their model.[11] Once they obtain their state-specific estimates of the impact of RTC laws, Moody and Marvell follow Ayres and Donohue in providing a dollar estimate – by state and cumulated for the entire country – of the impact of RTC laws on crime. Given their conclusion that RTC laws are "generally beneficial with respect to [their] overall long run effect on crime" (291), one would assume that generally RTC laws would lower the costs of crime. But here is where the first surprise comes in their paper.

A. Moody and Marvell's Overall Conclusions Depend Entirely on an Florida's Right to Carry Law Generating an Enormous Drop in Crime

What do Moody and Marvell mean when they say RTC laws are "generally" beneficial in the long run? They state that "Fourteen states experienced cumulative benefits while ten states experienced cumulative costs" (290). Unfortunately, they have the numbers backwards: fourteen of the 24 states are shown in Moody and Marvell's Table 10 to have cumulative *costs* – that is, according to their own estimates, RTC laws lead to higher crime costs for the majority of states!

But perhaps Moody and Marvell use the term "generally" to mean that the cumulative estimated effect of RTC laws across all states is crime-reducing, which they claim is supported by their Table 10. Specifically, Moody and Marvell's analysis indicates that the overall impact of RTC laws on crime through 2000 has been to lower crime by $28 billion nationally. But the same table reveals that Florida's RTC laws *alone* experienced a crime cost reduction through 2000 of almost $31 billion. In other words, across the 24 states that they analyze, they attribute a benefit of almost $31 billion to the Florida RTC law and estimate an overall *harmful*

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10 While there are differing opinions concerning the proper identification of the date of passage of RTC laws, Moody and Marvell use the coding of Ayres and Donohue. (See Appendix Table 1, column A in Ayres and Donohue (2003a) for the coding that Moody and Marvell employ in their current paper.)

11 Moody and Marvell note that they originally added an execution rate variable to see if the death penalty had any impact on murders. They tell us that the execution rate variable was statistically insignificant and therefore was dropped. In other words, Moody and Marvell appear to have bolstered the empirical evidence *against* the claim that the death penalty is a deterrent to murder. See Donohue and Wolfers (2005).

effect of about $3 billion of RTC laws across the other 23 jurisdictions. So much for "generally beneficial."

Now let us pause to reflect on this finding for a moment. If you had an intervention that had a net harmful effect in 23 out of 24 jurisdictions, while at the same time you estimated a massive benefit from the same intervention in only one state, would you assert that the intervention was "generally beneficial"? Indeed, if you were attentive to the findings in Ayres and Donohue (2003a, 2003b), Donohue (2004), Strnad (2007), and the report of the National Research Council (2004), about the poor fit and lack of robustness in the estimates of the impact of RTC laws, one would likely look at data showing a huge benefit in Florida coupled with a net harmful effect in the combined remaining 23 states and at least wonder whether the estimate of the impact of the 1987 Florida RTC law was spuriously picking up the impact of some other factor that caused crime to drop in Florida some time after 1987. Indeed, as Donohue (2003) noted, the Mariel boat lift in 1980 depositing countless undesirables from Cuba on the shores of Florida led to a massive increase in crime in Florida in the early 1980s. Thus, much or all of the crime reduction in Florida that Moody and Marvell attribute to the adoption of Florida's RTC law may well be simply owing to a regression to the mean effect – consistent with a general finding of Grambsch (2008).

We have already alluded to the general problems with the quality of county data and noted that these problems tend to be more severe with states that have adopted RTC laws. Importantly, Florida's crime data quality has degraded dramatically since it adopted its RTC law in 1987. As a result, Moody and Marvell's data set is missing crime data for 23 out of 67 counties in 1996 and they have *no* Florida crime data entered for 1988 and 1997, and they erroneously excluded all murder and rape data for 1998 including that of Florida. Their murder totals for 1999 and 2000, moreover, are substantially below the actual statewide totals, as reported in the FBI's Uniform Crime Reports. Indeed, the number of murders in Florida fell much less during the late 1990s than Moody and Marvell's data suggest, as Figure 1 illustrates. The serious problems with Florida's county crime data after the state passed its 1987 RTC law underscores once again the concerns about county data in general, and about the anomalous finding from the Moody and Marvell regression that makes Florida look like such a big success story.

Figure 1: Total Florida Homicides – Aggregate of Moody/Marvell Florida County Data vs. Statewide Figures for Florida Reported by the FBI
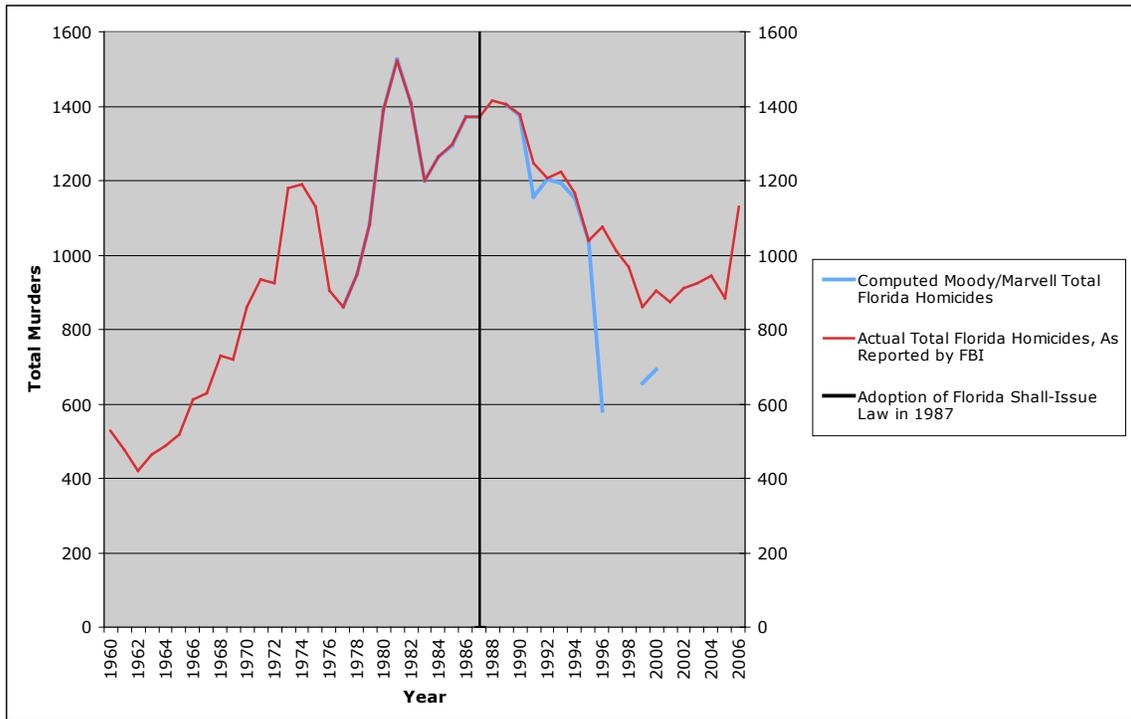
Source: FBI Uniform Crime Reports, via the Bureau of Justice Statistics.

Certainly, Moody and Marvell have not offered even a hint of a suggestion as to why RTC laws should be generally harmful across 23 states, yet be so massively beneficial in Florida. But another surprise awaits our readers.

B. Moody and Marvell Ignore Kovandzic and Marvell's Assessment of the Florida RTC Law as Providing No Net Benefits

Having identified Florida as an extreme outlier that completely drives their conclusion that RTC laws have "generally" reduced crime, Moody and Marvell would be expected to reflect on whether any studies had actually looked at the impact of Florida's RTC law on crime. Happily, Marvell himself was one of the coauthors of a major paper doing exactly that – Kovandzic and Marvell (2003). Indeed, Donohue (2003) published a comment on Kovandzic and Marvell that appeared simultaneously with that paper in the journal *Criminology and Public Policy*. Donohue (2003) offers the following observation while quoting from the *original*

version of the Kovandzic and Marvell paper on which the journal editors asked Donohue to comment:

> Kovandzic and Marvell collected county data on crime and concealed handgun permits across Florida from 1980 to 2000 and concluded, "we find no credible statistical evidence that increases in permit rate growth (and presumably more lawful gun carrying) leads to substantial reduction in violent crime, especially homicide. *Similar to Ayres and Donohue (2003), we find that our best, albeit admittedly imperfect, statistical evidence indicates that increases in permit rate growth may actually lead to slight increases in crime.*" (Donohue 2003, 399; italics supplied)

In other words, the initial conclusion of the Kovandzic and Marvell paper endorsed the view that the best evidence on the impact of the RTC law in Florida was that it slightly *increased* violent crime.

Interestingly, the final version of the Kovandzic and Marvell paper retreated from the claim that the best evidence showed that RTC laws *increased* crime, but it did conclude that Florida's RTC law did *not* decrease crime. In the words of Kovandzic and Marvell: "we find no credible statistical evidence that permit rate growth (and presumably more lawful gun carrying) leads to substantial reductions in violent crime, especially homicide" (2003, 387). Kovandzic and Marvell then went on to offer their explanations for why the Florida RTC law did *not* reduce crime:

> The fact that permit rate growth had no deterrent effect on violence rates is likely due to one of three reasons. First, few people wanted to obtain concealed carry permits. Despite millions of Floridians being eligible for permits, apparently only a handful of people were willing to go through the hassle of applying for one. By mid-year 2000, some twelve years after the law was in effect, there were only 248,049 valid concealed weapons permits in Florida, representing 2.1 percent of the Florida resident population (see Table 1). Second, the law did not actually affect rates of gun carrying among prospective victims – people already carrying merely legitimated what they were doing by getting permits. Third, it may be that gun carrying actually did increase (though perhaps only to a slight degree, among those few who would carry guns only if they could get a permit), but the crime-increasing effects of a few violent people (who nevertheless had no criminal convictions) getting permits balanced out the crime-decreasing effects of many nonviolent people getting permits. **Such an explanation implies that rates of lawfully**

> *permitted gun carrying may have no net effect on crime rates for the same basic reasons that gun ownership levels in general have no net effect, guns among criminals may increase violence while guns among noncriminals decrease it, with the two opposite-sign effects canceling each other out …*" (Kovandzic and Marvell 2003, 387-389; bold and italics supplied)

C. If The Florida RTC Law Was Feckless, Then Moody and Marvell's State by State Estimates Refute the More Guns, Less Crime Hypothesis

What are we then to take from the Moody and Marvell paper? Given their finding that, for 23 of the 24 jurisdictions they examined, RTC laws had an overall crime-increasing effect, one would assume that this would be taken as evidence against the more guns, less crime hypothesis. If one could establish that the one state that seemed to have an unusually good crime experience after the adoption of the RTC laws had really benefited from the passage of the law, then one might alter this generally pessimistic assessment concerning RTC laws. But if Kovandzic and Marvell are correct that Florida's RTC law conferred no benefit, then the estimated $31 billion benefit that Moody and Marvell claim to have found in their latest state-specific analysis is simply a mirage. Remarkably, despite their claims to the contrary, Moody and Marvell have inadvertently presented yet more evidence that RTC laws do *not* reduce crime.

Moody and Marvell are aware of the divergence between their evidence and their more guns, less crime conclusion, as they note: "Since the net effect across all states is $28 billion, the other states have experienced a net *increase* in crime amounting to a cost of $2.8 billion" (291; italics supplied). Moody and Marvell state:

> even without Florida, there is a long run net benefit of $183 million per year, which is significantly different from zero. If the ethically proper discount rate is reasonably low, then the only relevant result is the ongoing long-run effect, which is less crime. Therefore, even excluding Florida, the state which has apparently benefited most from a right-to-carry law, the overall long run impact of these laws is lower crime. (291)

But Moody and Marvell have already told us that, with Florida excluded, the "cumulative effect over all the years the law has been in existence in each state, up to the year 2000" is a crime *increase* of $2.8 billion. If we take their $183 million figure as the annual "long run net benefit," it will be some time before we see any positive result from the RTC laws in those 23 jurisdictions. Even using a zero discount rate, this would mean that the long term trend that Moody and Marvell have

estimated through the year 2000 would have to continue for over 15 years – that is, beyond the year 2015. Add in a positive discount rate, and this number would only grow.

In other words, the Moody and Marvell analysis is obviously flawed. Of course, Moody and Marvell realize that, even if RTC laws were beneficial, they could not cause crime to fall forever, since ultimately a new, lower-crime equilibrium would be reached. At that point, the decline relative to the state of the world in which the RTC law had not been adopted would end. As the data on U.S. murder rates in Figure 2 clearly demonstrates, crime did *not* keep falling after 2000 but instead flattened out and then started rising. Similar patterns can be seen in Figures 3 and 4 for robbery and aggravated assault.[12] In fact, this is one reason that every year that the data has been extended has only served to weaken the claims that Lott and Mustard made in their initial paper with data ending in 1992. Extending Moody and Marvell's data set to capture additional years would presumably only weaken their case still further. Indeed, earlier in their paper when Moody and Marvell were trying to attack Ayres and Donohue for only projecting their trend out for 5 years, they recognized the folly of long-term extrapolation: "We grant that an analysis should not allow an 'eternity' of the trend effect [to] determine the results. Rather, the analysis should extend for some appropriate finite span. [W]e think that a ten-year span is appropriate…." (273). While projecting ten years out from 2000 seems imprudent to us, it would still leave Moody and Marvell with the conclusion that RTC laws generally *increase* crime (for 23 of 24 states).

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12 As Figure 3 indicates, only one group of states has experienced a continuing drop in aggravated assault after 2000 – the states that never adopted RTC laws.

Figure 2: Aggregate Murder Rates in States With and Without Shall-Issue Laws, 1977-2006
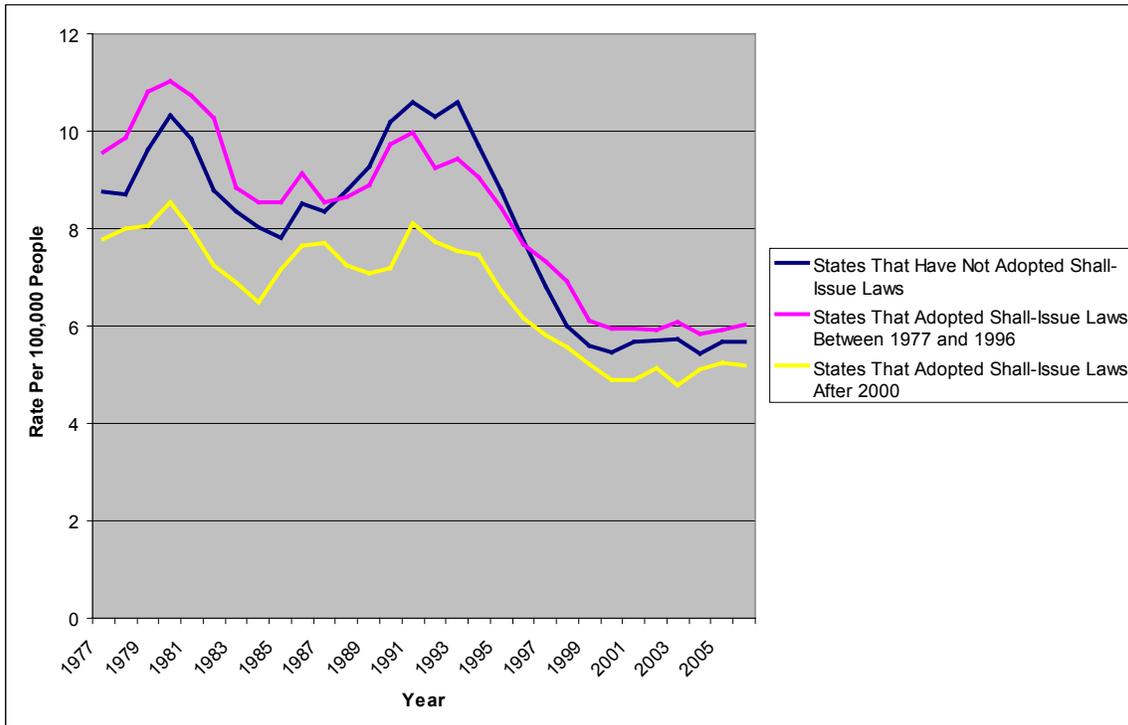
Source: FBI Uniform Crime Reports, via the Bureau of Justice Statistics.

Notes: States that have not adopted shall-issue laws: Alabama, California, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Iowa, Kansas*, Maryland, Massachusetts, Nebraska*, New Jersey, New York, Rhode Island, and Wisconsin.

States that adopted shall-issue laws between 1977 and 1996: Alaska (1994), Arizona (1994), Arkansas (1995), Florida (1987), Georgia (1989), Idaho (1990), Indiana (1980), Kentucky (1994), Louisiana (1996), Maine (1985), Mississippi (1990), Montana (1991), Nevada (1995), North Carolina (1995), North Dakota (1985), Oklahoma (1995), Oregon (1989), Pennsylvania (1989), South Carolina (1996), South Dakota (1986), Tennessee (1994), Texas (1995), Utah (1995), Virginia (1988), West Virginia (1989), and Wyoming (1994).

States that adopted shall-issue laws after 2000: Colorado (2003), Michigan (2001), Minnesota (2003), Missouri (2003), New Mexico (2003), and Ohio (2004).

From 1997 to 2000, no state adopted a shall-issue law, and hence the third group consists of the six states that passed such laws after 2000. In addition, Kansas and Nebraska adopted shall-issue laws in 2006, with the Kansas law taking effect on July 1, 2006, and the Nebraska law taking effect on January 1, 2007.
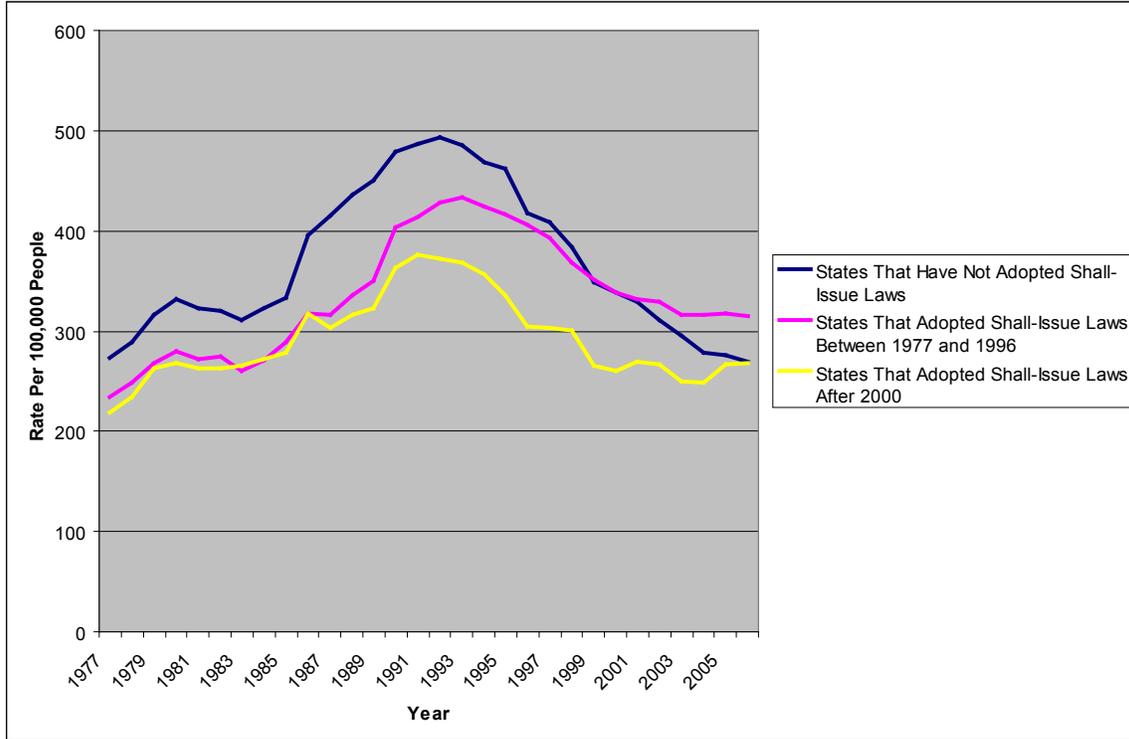
Figure 3: Aggregate Robbery Rates in States With and Without Shall-Issue Laws, 1977-2006
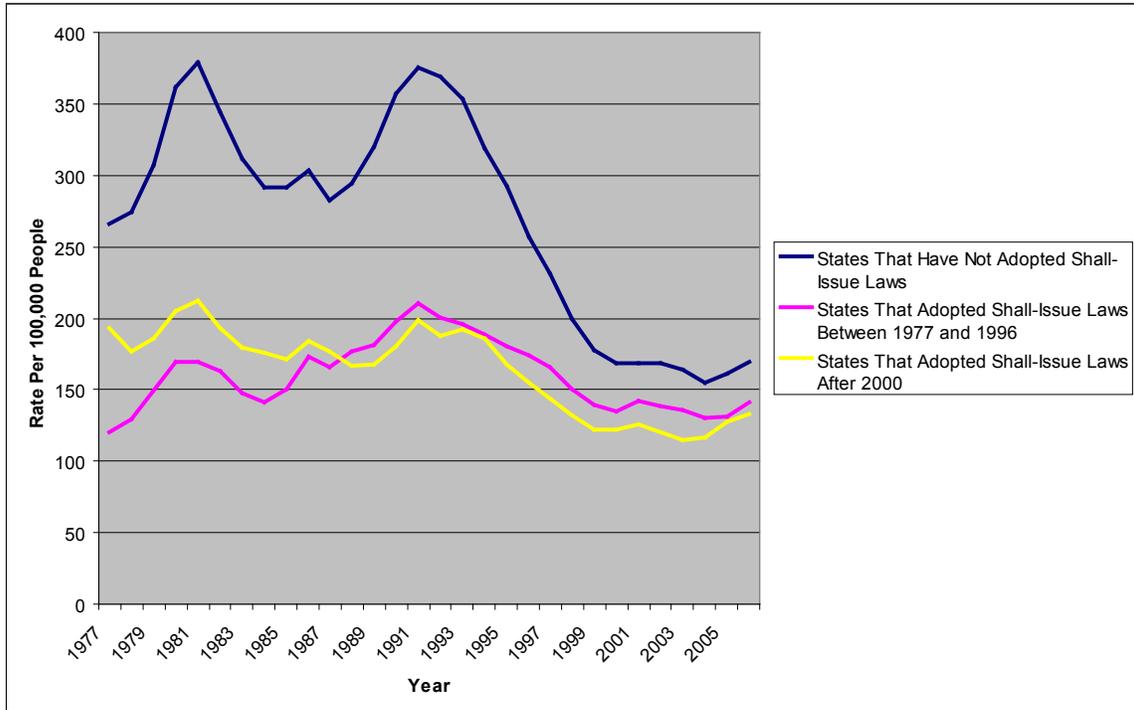
Source: FBI Uniform Crime Reports, via the Bureau of Justice Statistics.
Notes: Same as for Figure 2.

Figure 4: Aggregate Aggravated Assault Rates in States With and Without Shall-Issue Laws, 1977-2006

Source: FBI Uniform Crime Reports, via the Bureau of Justice Statistics.

Notes: Same as for Figure 2.

III. Using Moody and Marvell's Specification and Data to Generate Aggregate Estimates of the Impact of RTC Laws

Virtually all of the studies that Moody and Marvell cite as supporting the more guns, less crime hypothesis provided aggregated rather than state-specific estimates of the RTC impact. To test whether the conclusions of the earlier aggregate studies remain robust when data runs until 2000, this section explores whether the aggregate estimates that emerge from using Moody and Marvell's specification tell a story similar to that emerging from the state specific estimates. Once again, the aggregate regressions using the Moody and Marvell variables and specification undermine their claims.

Table 1 provides estimates of the impact of RTC laws for the full array of seven FBI Index I violent and property crimes. The hybrid model in row 3 estimates any shift in the level of crime and in the trend of crime following the

adoption of a RTC law, and is identical in every respect to what Moody and Marvell have estimated (that is, same data, same specification) with four exceptions: 1) Table 1 provides aggregated rather than state-specific estimates of the impact of RTC laws; 2) Moody and Marvell somehow erroneously dropped 1998 murder and rape data from their analysis, and we correct this error by including the missing 1998 data for these two crime categories; 3) Moody and Marvell treat Philadelphia as having a shall-issue law in effect for the same time period as the rest of Pennsylvania (post-1989), and we correct this by re-coding the Philadelphia post-passage dummy variable to represent the correct time period (post-1995); and 4) Moody and Marvell erroneously omit the trend variable for the District of Columbia when controlling for state trends, and we correct this programming error. To be consistent with the literature that Moody and Marvell cite, we also present in rows 1 and 2 the comparable aggregate estimates for shifting levels of crime (the dummy model), and for changing trends (the spline model). The bottom line is that these estimates provide *no* support for Moody and Marvell's more guns, less crime suggestion. Specifically, in all of Table 1, there are only two statistically significant coefficients – both highly significant at the 1% level – and they suggest that RTC laws *increase* the rate of aggravated assault.

Table 1: The Estimated Impact of Shall-Issue Laws, All Crimes, Controlling for State Trends, 1977-2000 Moody & Marvell County Data

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	1.7%	-1.3%	2.8%	2.0%	5.3%	-0.7%	1.5%
	(3.9%)	(3.9%)	(1.9%)	(3.3%)	(3.3%)	(1.9%)	(1.2%)
2. Spline model:	0.2%	-1.1%	**2.2%**	1.3%	1.2%	-0.2%	0.7%
	(1.1%)	(1.3%)	**(0.7%)**	(1.5%)	(1.2%)	(0.6%)	(0.6%)
3. Hybrid model:							
Postpassage dummy	1.5%	0.3%	-0.5%	0.1%	3.9%	-0.5%	0.5%
	(4.3%)	(4.1%)	(1.8%)	(2.6%)	(2.8%)	(1.8%)	(1.0%)
Trend effect	0.0%	-1.2%	**2.3%**	1.3%	0.9%	-0.1%	0.6%
	(1.2%)	(1.4%)	**(0.8%)**	(1.5%)	(1.2%)	(0.6%)	(0.6%)

Note: Table 1 provides three different aggregated estimates of the impact of RTC laws using the county-level data set for the period 1977-2000 that Moody and Marvell use in their paper. These estimates are computed after correcting Moody and Marvell's erroneous omission of 1998 data on murder and rape, their incorrect dating of the RTC law for Philadelphia., and their omission of the D.C. trend. While Moody and Marvell present state-by-state estimates of the effect of RTC laws using the hybrid model, this table presents a dummy variable model (row 1) and a spline model (row 2), in addition to the hybrid model, which generates both a dummy shift and a trend change estimate (beginning in row 3). The estimated effects of RTC laws on murder and rape are computed using the Moody and Marvell method of adding .1 to the murder and rape crime rates before taking the natural log. This procedure is followed in order to avoid dropping a considerable number of observations when the county has no murder or no rape in a given year (thereby preventing the taking of the natural log). Moody and Marvell also control for the violent crime arrest rate for all crime categories (including property crimes). Moody and Marvell use the coding for RTC passage dates provided by Ayres and Donohue (2003a).

    Apart from the aggregate estimate (as opposed to state-by-state estimates), the specification and data used to generate the

impact of RTC laws on crime are identical to what Moody and Marvell present in their article. Robust standard errors, clustered at the state level, are in parentheses. Estimates significant at the 1% level are bolded and underlined. No other estimate is statistically significant at even the 10% level. Note that, with the exception of aggravated assault, which shows a *positive* and highly significant trend estimate in models 2 and 3, none of these results is statistically significant. For murder, every estimated effect is less than its standard error.

We are uncertain how committed Moody and Marvell are to the elements of the one set of regressions they present. The factors that they emphasize are the state-specific estimation, the hybrid model, the state trends, the new crack measure, a lagged dependent variable, and their decision to add .1 to every rape and murder crime rate (to avoid losing county year observations with no rapes or murders).[13] While some may question these choices (and many others that Moody and Marvell do not mention[14]), it turns out – with one exception – that one can alter any of the above features (at least individually) without altering the Table 1 story that RTC laws are associated with *higher* rates of aggravated assault and no other statistically significant effect. The one factor that can influence the RTC estimates is the inclusion of state trends, to which we now turn.

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13 For example, the murder regressions have 65,902 observations when .1 is added to each murder rate, but only 37,113 when it is not added. The difference represents county-year observations in which there were no recorded murders, which get dropped when one takes the natural log of the murder rate.

The massive number of zero observations should not be as consoling as it might first appear. When a zero exists in the county data, one might expect this to indicate that no crime occurred for a particular county in a given month/year. According to Maltz (2006), however, a "0" datum could also indicate that: (1) an agency was not yet reporting data because it did not exist at the particular time (or alternatively, it did not have a crime reporting unit), (2) an agency's crime data was reported through another agency, (3) an agency ceased to exist (perhaps by merging with another agency), (4) due to local policy, an agency reported quarterly, semiannual or annual data, rather than the monthly data required for the FBI's compiled set, or (5) an agency simply did not submit a particular month's data. This presents another concern about the use of county data.

14 To list four of these additional concerns raised in Donohue and Ayres (2003a) that apply to the Moody and Marvell specifications in their current paper: (1) There is a mis-match between some of their key variables such as the level of incarceration (appropriately lagged one year), which is measured at the state level, and crime rates, which are measured at the county level (as we will see below, the same criticism exists for Moody and Marvell's crack control); (2) the key arrest rate variable is poorly measured at the county level and not a true clearance rate since it is simply the ratio of arrests to crimes for a particular crime category, which because of the existence of multiple offenders often exceeds one. Conversely, when a mass murder occurs, the ratio is completely misleading as in the case of Tim McVeigh who killed 168 individuals in the Oklahoma City federal building. McVeigh's arrest would suggest a miserable "clearance rate" of 1/168 under Moody and Marvell's accounting, when in fact all 168 murders had been cleared. Moreover, while Moody and Marvell suggest that they are using the violent crime arrest rate to explain violent crime and the property crime arrest rate to explain property crime, they actually use the violent crime arrest rate for all crimes. Finally, by using the contemporaneous arrest rate as an explanatory variable for crimes that are included in the denominator of that arrest rate, there is a concern of endogeneity and ratio bias; (3) there is extreme multicollinearity in the 36 demographic controls that Lott (and now Moody and Marvell) employ, as well as non-trivial measurement error in annual county data measures of these variables; and (4) there is a considerable degree of model sensitivity for all of the RTC estimates, which may reflect in part problems with endogenous adoption of RTC laws and the lack of controls for regression to the mean in the aftermath of the geographically disparate crime jumps of the late 1980s.

Table 2 exactly replicates the aggregated specifications of our Table 1 with a single change – it now drops the control for state trends that Moody and Marvell employed. As Table 2 reveals, dropping state trends only serves to reinforce the conclusion that RTC laws *increase* crime. While in Table 1 only aggravated assault appears to be increased because of RTC laws, in Table 2 we see statistically significant estimates suggesting RTC laws increase not only aggravated assault, but rape, robbery, larceny, auto theft, and burglary as well.

Table 2: The Estimated Impact of Shall-Issue Laws, All Crimes, Dropping State Trends, 1977-2000 Moody & Marvell County Data

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.8%	6.3%	**7.1%**	**9.1%**	<u>**10.3%**</u>	**3.9%**	<u>**8.8%**</u>
	(5.2%)	(4.1%)	**(3.0%)**	**(3.7%)**	<u>**(3.6%)**</u>	**(1.8%)**	<u>**(1.5%)**</u>
2. Spline model:	-0.7%	0.6%	**1.3%**	<u>1.3%</u>	**1.5%**	0.4%	<u>**1.5%**</u>
	(1.0%)	(0.8%)	**(0.6%)**	<u>(0.7%)</u>	**(0.7%)**	(0.4%)	<u>**(0.4%)**</u>
3. Hybrid model:							
Postpassage dummy	3.9%	<u>7.9%</u>	4.2%	7.6%	**8.7%**	**4.4%**	<u>5.5%</u>
	(4.5%)	<u>(4.3%)</u>	(2.8%)	(4.9%)	**(3.7%)**	**(1.9%)**	<u>(1.6%)</u>
Trend effect	-1.1%	-0.4%	0.7%	0.4%	0.4%	-0.1%	<u>0.8%</u>
	(1.0%)	(1.0%)	(0.6%)	(0.9%)	(0.7%)	(0.5%)	<u>(0.4%)</u>

Note: Table 2 provides three different aggregated estimates of the impact of RTC laws using the county-level data set for the period 1977-2000 that Moody and Marvell use in their paper. As in Table 1 above, .these estimates are computed after correcting Moody and Marvell's erroneous omission of 1998 data on murder and rape, and their incorrect dating of the RTC law for Philadelphia. While Moody and Marvell present state-by-state estimates of the effect of RTC laws using the hybrid model, this table presents a dummy variable model (row 1) and a spline model (row 2), in addition to the hybrid model, which generates both a dummy shift and a trend change estimate (beginning in row 3). For the murder and rape estimates, Moody and Marvell add .1 to all county murder and rape rates in order to avoid dropping a considerable number of observations when the county has no murder or no rape in a given year (thereby preventing the taking of the natural log). We again follow this approach in our estimations. Moody and Marvell also control for the violent crime arrest rate for all crime categories (including property crimes). Moody and Marvell use the coding for RTC passage dates provided by Ayres and Donohue (2003a).

Apart from the aggregate estimate (as opposed to state-by-state estimates) and **dropping state trends**, the specification and data used to generate the impact of RTC laws on murder are identical to what Moody and Marvell present in their article (with the exception that we include murder and rape data from 1998 in our analysis). Robust standard errors, clustered at the state level, are in parentheses. Estimates significant at the 10% level are underlined. Estimates significant at the 5% level are bolded. Estimates significant at the 1% level are bolded and underlined.

Note that Table 2 is identical to Table 1, except that Table 2 does not control for state trends. The primary difference in the two tables is that, while Table 1 suggested RTC laws increased aggravated assault and had no statistically significant effect on other crimes, Table 2 provides some evidence that all crimes other than murder are exacerbated by RTC laws.

Of course, we have always cautioned about too quickly rushing from findings of statistically significant coefficient estimates to strong causal conclusions about the impact of RTC laws – a concern that we wish Lott, Mustard, Moody, Marvell, and other champions of RTC laws would begin to heed – but the bottom

line from examining Moody and Marvell's approach while generating aggregate estimates (as opposed to state specific estimates) is only in the direction of RTC laws *increasing* crime.

As Donohue (2004) concluded almost five years ago after presenting an analysis of state data through 1999:

> Where does that leave us? Based on the current evidence, it would seem that one needs to make a judgment about whether the crime models presented in this Essay are working well. If the judgment is that they are, then one would have to embrace the finding that property crime will be *higher* when RTC laws are adopted. Conversely, some might contend that the fact that the models suggest that the RTC laws increase property crime is itself the best evidence that the statistical models are not working well. In any event, there is *no* evidence here that would support the more guns, less crime hypothesis. (638)

These concerns are still worth considering today. It is not clear why property crime should rise when RTC laws are adopted. Much evidence suggests that guns in the home are very attractive to burglars but RTC laws probably don't increase the number of guns in the home enough to make a difference to the massive amount of gun theft that occurs in the United States each year (Cook and Ludwig 2003). It may well be that the evidence in Table 2 of property crime increases resulting from RTC are spurious and should be taken as indications that the Table 2 model is not working well. This might push us towards the belief that state trends should be included in the regression, and therefore might further be taken to bolster the Table 1 conclusion that RTC laws *increase* aggravated assault.

But if aggravated assault goes up, one might ask, why isn't there Figure 1 evidence that RTC laws increase murders significantly? While this is an interesting issue that likely merits closer scrutiny in the future, for now, we can only speculate. Beginning with our first examination of RTC laws – Ayres and Donohue (1999) – we have been consistent in our belief that the murder spree that the crack trade induced made non-adopting states look bad relative to adopting states (since the non-adopters got a big jump in murder from crack while the adopting states during the problematic years of crack were less influenced by crack). If so, the ostensible lack of influence of RTC laws on murder may be the product of omitted variable bias obscuring a murder-inducing effect.

But hasn't the Moody and Marvell control for crack (secured from Fryer et al 2005) eliminated this problem? Unfortunately, no. While Moody and Marvell pronounce that their crack control deals with this vexing influence on murder, it does not. Moody and Marvell are estimating data on crime in counties but are

using a statewide measure of the severity of crack. This means that a rural county with absolutely no crack problem is given the same value on this control as the most densely urban county in the same state where crack is a serious concern. The Moody and Marvell measures assigns the same crack problem to the Bronx and Lake Placid, New York.[15] Fryer et al created the crack index to measure severity in urban areas, but their city index is not useful for the RTC estimation that Moody and Marvell employ. Therefore they rely on the Fryer et al statewide crack index, despite the qualifications from Fryer et al that: "One important difference relative to the city sample is that overall crime is not positively and statistically related to crack in the state sample," and "Not surprisingly given that crack was concentrated in large cities, the rise and fall of crack has less explanatory value at the state level." It is likely for this reason that the crack variable does not influence the RTC estimates in Moody and Marvell's county data estimation, even though crack and the subsequent regression to the mean from its aberrant increase in murders have been shown to badly bias the estimates of the impact of RTC laws on murder in a way that makes these laws look more benign than they are (Grambsch 2008).

IV. Conclusion

Moody and Marvell have given us a *single* new regression using the Lott county-level data that we corrected back in 2003 and analyzed in Ayres and Donohue (2003b) and that the National Research Council analyzed in its 2004 report. Moody and Marvell use this single regression – a state-specific hybrid model estimated on county data for the years 1977-2000 – to estimate the overall change in the cost of crime attributed to RTC laws for the period that these laws have been in effect for the 24 states they evaluate. Their own table indicates that RTC laws on net *increase* the costs of crime (albeit statistically insignificantly) in aggregate for 23 of the 24 jurisdictions they examine, but cause massive benefits in the single state of Florida.

In our view, Moody and Marvell's state-specific estimates support the view that RTC laws generally do not lower and may *increase* overall crime costs – at least if one endorses Marvell's own paper (with Kovandzic) arguing that the issuance of RTC permits did not alter crime in Florida. Since the Kovandzic and Marvell paper examining the impact of RTC law on Florida crime is listed in Moody and

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15 A number of anomalies plague the state crack index that Moody and Marvell employ. For example, New York state's crack index is always higher (except in the year 2000 when they are roughly the same) than the crack index value that Fryer et al assign to New York City. At the same time, Rochester and Syracuse have virtually zero crack values over the span of the Moody and Marvell data, but their counties would still be assigned the high and variable New York state crack value. Similarly, the crack value for the state of Maryland, which Moody and Marvell assign to all counties in the entire state, is always substantially higher than the crack value for the city of Baltimore.

Marvell's Table 2 as standing for the proposition "Shall Issue Has No Significant Effect on Crime," one might assume that they conclude that RTC laws have *not* lowered crime in Florida.

This generally pessimistic conclusion about the value of RTC laws would be broadly consistent with our previous demonstration that Plassmann and Whitley's RTC regressions (estimated on the same 1977-2000 data set that Moody and Marvell use) show that no crimes are reduced by RTC laws and most property crime is *increased* (Ayres and Donohue, 2003b, 1388).

When we use Moody and Marvell's data and their preferred specification, modified in only one respect – to follow the aggregated models used in the published studies they argue support the more guns, less crime hypothesis (such as Plassmann and Whitley) – we again find *no* support for the more guns, less crime hypothesis. Indeed, when controlling for state trends (as Moody and Marvell do), we see statistically significant evidence that RTC laws *increase* aggravated assault. Omitting state trends, we see that *all* index I crimes except for murder appear to be *increased* by RTC law passage.

Thus, we have three different explorations of county data for the period from 1977-2000, which, when coupled with Kovandzic and Marvell's assessment of RTC laws in Florida, paint a fairly uniform picture that RTC laws have no beneficial effect on crime and evidence from the corrected Plassmann and Whitley model and the aggregated form of Moody and Marvell's state specific regression suggest that RTC laws *increase* certain crime categories.[16]

Indeed, the fact that adding more years of data has consistently made RTC laws look worse, coupled with the impressionistic, visual evidence in Figures 1-3, suggests that as more years of data are systematically evaluated and better controls for endogeneity and regression to the mean are implemented (see Grambsch 2008), the picture of the impact of crime on RTC laws is likely only to get darker. For example, compare the aggravated assault rates in the 26 states that adopted RTC laws between 1980 and 1996 with the same crime rate for the 15 states (plus the District of Columbia) that had not adopted the law at least through 2005. At the beginning of that time frame, the states that did not get RTC laws had rates of aggravated assault that were about 17% higher than the states that were to soon to launch the RTC experiment. By the year 2006, the pattern had reversed and the states with RTC laws now had aggravated assault rates close to 15% *higher*. The pattern is even stronger for robbery: before they adopted their RTC laws, the prospective RTC states shown in Figure 2 had robbery rates that were almost 55% lower than the non-adopting states; now these RTC states have robbery rates only about 16% lower. If RTC laws reduce crime, it certainly isn't showing up in the aggregate data.[17]

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16 All three of these U.S. panel data studies use county data and cluster the standard errors by state, as Moody and Marvell recommend.

17 Even for murder where the story is a bit more complicated, we see that in about 1987, which was the

As for now, we note that Phil Cook and Jens Ludwig – two of the top researchers on crime in general and guns in particular – have written that "the best empirical evidence does not support" the "more guns, less crime hypothesis" (Cook and Ludwig, 2004). We agree.

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