The Impact of Right to Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy

May 20, 2010

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Abstract

For over a decade, there has been a spirited academic debate over the impact on crime of laws that grant citizens the presumptive right to carry concealed handguns in public – so-called right-to-carry (RTC) laws. In 2005, the National Research Council (NRC) offered a critical evaluation of the "more guns, less crime" hypothesis using county-level crime data for the period 1977-2000. 15 of the 16 NRC panel members essentially concluded that the existing research was inadequate to conclude that RTC laws increased or decreased crime. One member of the NRC panel concluded that the NRC panel data regressions supported the conclusion that RTC laws decreased murder, while the 15-member majority responded that the scientific evidence did not support that conclusion. We evaluate the NRC evidence and show that, unfortunately, the regression estimates presented in the report appear to be incorrect. We improve and expand on the report's county data analysis by analyzing an additional six years of county data as well as state panel data for the period 1977-2006. While we have considerable sympathy with the NRC's majority view about the difficulty of drawing conclusions from simple panel data models, we disagree with the NRC report's judgment that cluster adjustments to correct for serial correlation are not needed. Our randomization tests show that without such adjustments the Type 1 error soars to 40 – 70 percent. In addition, the conclusion of the dissenting panel member that RTC laws reduce murder has no statistical support.

Our paper highlights further important questions to consider when using panel data methods to resolve questions of law and policy effectiveness. We buttress the NRC's cautious conclusion by showing how sensitive the estimated impact of RTC laws is to different data periods, the use of state versus county data, particular specifications, and the decision to control for state trends. Overall, the most consistent, albeit not uniform, finding to emerge from the array of models is that aggravated assault rises when RTC laws are adopted. For every other crime category, there is little or no indication of any consistent RTC impact on crime. It will be worth exploring whether other methodological approaches and/or additional years of data will confirm the results of this panel-data analysis.

[1] The authors wish to thank David Autor for helpful comments and Yale Law School for financial support.

I. Introduction

The debate on the impact of "shall-issue" or "right-to-carry" (RTC) concealed handgun laws on crime has now raged on for over a decade.[2] John Lott and David Mustard initiated the discussion with their widely cited 1997 paper arguing that the adoption of RTC laws has played a major role in reducing violent crime. The ever expanding literature on this topic has included many subsequent empirical studies—employing varying models and time periods—that both support and challenge this "more guns, less crime" hypothesis.[3] But, as Ayres and Donohue (2003a) emphasized, Lott and Mustard's original data set ended just before the extraordinary crime drop of the 1990s occurred. Ayres and Donohue concluded that extending Lott and Mustard's data set beyond 1992 undermined the "more guns, less crime" hypothesis. Subsequent papers by Donohue in 2003 and 2004 raised further doubts about the claimed benefits of RTC laws (Donohue 2003; Donohue 2004).

But even as the empirical support for the Lott and Mustard thesis was weakening, its political impact was growing. State legislators continued to cite this work in support of their votes on behalf of RTC laws, and the "more guns, less crime" hypothesis has been invoked often in support of creating a personal right to have handguns under the Second Amendment. In the face of this scholarly and political ferment, in 2003, the National Research Council convened a committee of 16 top experts in criminology, statistics, and econometrics. Its purpose was to evaluate the existing literature and data in hopes of reconciling the various methodologies and findings concerning the relationship between firearms and violence, of which the impact of RTC laws was a single, but important, issue. With so much talent on board, it seemed reasonable to expect that the Committee would reach a decisive conclusion and perhaps even put the debate to rest once and for all.

The vast majority of the report, which was finally issued in 2005, was uncontroversial. The discussion of RTC laws was anything but that. Citing the extreme sensitivity of point estimates to various model specifications, the NRC report not only did not narrow the domain of uncertainty about the impact of RTC laws, but may have broadened it. Although the NRC majority report found no statistical support for the "more guns, less crime" hypothesis, one dissenter argued that RTC laws did reduce the rate of murder. Moreover, one member of the panel even doubted that *any* econometric evaluation could illuminate the impact of RTC laws. Given the high-powered cast of academics on the NRC panel and the wildly conflicting conclusions on everything from the substantive issue of the impact of RTC laws to empirical methodology and even the value of the econometric evaluation of law and policy, some effort to assess the NRC report is needed.

The outline of this paper is as follows. Section II offers some background on the debate over RTC laws, and Section III describes the positions taken in the NRC report. Section IV shows that the key tables in the NRC report, which led one panel member to conclude that RTC laws actually reduced murder, are flawed and cannot be replicated. Sections V and VI look at

[2] The term "RTC laws" is used interchangeably with "shall-issue laws" in the guns and crime literature.
[3] Appendix Table 1 lists some of the key articles that analyzed the original county-level panel data set employed by Lott and Mustard, which spanned the time period 1977 through 1992.

two key econometric issues—whether to control for state trends and whether to adjust standard errors through clustering. Section VII extends data through 2006, and offers additional robustness checks on the Lott and Mustard specification. Section VIII discusses the issue of whether the impact of RTC laws can be better estimated using county or state-level data. Section IX offers concluding comments on the current state of the evidence on RTC laws, the difficulties in ascertaining the true causal impact of legal interventions, and the dangers that exist when policymakers can simply pick the study they like from among a wide array of conflicting estimates.

II. Background on the Debate

In their 1997 paper, "Crime, Deterrence, and Right-to-Carry Concealed Handguns," John Lott and David Mustard argued, based on a panel data analysis, that RTC laws were the primary driving force behind falling rates of violent crime. Lott and Mustard used county-level data, including county and year fixed effects and a number of explanatory variables, to estimate the impact of RTC laws on crime rates over the time period 1977-1992. In essence, Lott and Mustard's approach was designed to identify the effect of RTC laws on crime in the ten states that adopted them during this time period. The standard strategy is called a difference-in differences model (controlling for state and year fixed effects) because the change in crime in the 10 adopting states is compared with the change in crime in the non-adopting states. The implicit assumption is that the control variables included in the regression will explain other movements in crime across the various states, and the remaining differences in crime patterns can be attributed to the presence or absence of the RTC laws.

Lott and Mustard estimated two difference-in-difference-type models to test the impact of RTC laws: a dummy variable model and a trend, or "spline," model. In the dummy variable approach, RTC laws are modeled as a dummy variable which takes on a value of one in the first full year after passage and retains that value thereafter (since no state has repealed its RTC law once adopted). If the above condition does not apply, the RTC dummy will have a value of zero. This model tests whether the average crime level in the pre-passage period is statistically different from the post-passage crime level (after controlling for other factors).

The spline model measures whether crime *trends* are altered by the adoption of RTC laws. Lott noted that the spline approach would be superior if the intervention caused a reversal in a rising crime rate. Such a reversal could be obscured in a dummy variable model that only estimates the average change in crime between the pre- and post-passage periods. Specifically, Lott argued that an effective RTC law might show no effect in the dummy variable model if the rise in the pre-passage crime rate and the fall in the post-passage rate left the average "before" and "after" crime levels the same. In both regression models, Lott and Mustard also included the following control variables: county-level arrest rates, county population, population density, several measures of income, and thirty-six categories of demographic composition. Seeing this list, any knowledgeable researcher would immediately be concerned by the absence of important explanatory variables, such as the incarceration rate from their model, as well as any control for the level of police force.

To one not attuned to the importance of the impact of incarceration and police on crime, however, Lott and Mustard's initial regression results seemed to support the contention that laws allowing concealed handgun ownership lead to less crime. The dummy variable regressions showed that murder, rape, aggravated assault, and overall violent crime fell by 4 to 7 percent following the passage of an RTC law. In contrast, property crime rates (auto theft, burglary, and larceny) were estimated to have increased by 2 to 9 percent. On this basis, Lott and Mustard concluded that criminals respond to RTC laws by switching from violent crime to property crime to reduce the risk that they would be shot (since victims are more often absent during the commission of property crime). They also declared that the "more guns, less crime" contention was strengthened by the trend analysis, and to this end, they presented results showing significant *decreases* in murder, rape, and robbery (but no significant increases in property crime).

From this evidence, Lott and Mustard dramatically concluded that allowing citizens to carry concealed handguns deters violent crimes more effectively than any other crime reduction policy: "concealed handguns are the most cost-effective method of reducing crime thus far analyzed by economists, providing a higher return than increased law enforcement or incarceration, other private security devices, or social programs like early education" (Lott and Mustard 1997). Lott and Mustard suggested that had remaining non-RTC states enacted such legislation, over 1,414 murders and over 4,177 rapes would have been avoided. They also estimated that an additional concealed handgun permit would reduce victim losses by up to $5,000.

A. The Far-Reaching Impact of "More Guns, Less Crime"

The Lott and Mustard paper and Lott's subsequent research (and pro-gun advocacy) have had a major impact in the policy realm. Over the past decade, politicians as well as interest groups such as the National Rifle Association (NRA) have trumpeted the results of this empirical research to oppose gun control efforts and promote less restrictive gun-carrying laws. Lott relied on his own research to advocate for the passage of state-level concealed-carry handgun legislation, testifying on the purported safety benefits of RTC laws in front of several state legislatures, including Nebraska, Michigan, Minnesota, Ohio, and Wisconsin (Ayres and Donohue 2003a).

The impact of Lott's research can also be seen at the federal level (Ayres and Donohue 2003a). In 1997, the now infamous Senator Larry Craig (R-Idaho) introduced the Personal Safety and Community Protection Act with Lott's research as supporting evidence. This bill was designed to allow state nonresidents with valid concealed-carry permits in their home state to possess concealed firearms (New York Giants football star, Plaxico Burress, sought to invoke this defense when he accidentally shot himself in a Manhattan nightclub with a gun for which he had previously obtained a Florida concealed carry permit, albeit expired by then). According to Senator Craig, Lott's research demonstrated that citizens carrying handguns would transfer protective benefits to the public because criminals would find themselves in the line of fire.[4]

Lott's work certainly provided academic cover for researchers, policymakers, and others holding the view that the Second Amendment conferred a private right to possess guns. Indeed, it is not impossible to imagine that Lott's work might have been enough to sway Justice Kennedy to vote with the majority in the 5-4 *Heller* decision, which ultimately established this new interpretation of the Second Amendment, while disregarding the Amendment's initial clause, referring to a "well-regulated militia."

B. Questioning "More Guns, Less Crime"

Within two years of the 1997 publication of the Lott and Mustard paper, a number of scholars raised serious questions concerning the "more guns, less crime" hypothesis (e.g., see the four studies list in Appendix A). Ayres and Donohue (2003a) began by pointing out that crime rose across the board from 1985 to 1992, although most dramatically in non-RTC states. Since the Lott and Mustard data set ended in 1992, it could not address the most dramatic reversal in crime in American history. Ayres and Donohue attributed Lott and Mustard's results to the crack epidemic, which hit large urban areas hardest—areas that largely did *not* adopt RTC laws prior to 1992. Thus, they argued that Lott and Mustard's model suffered from serious omitted-variable bias. To illustrate this point, Ayres and Donohue showed that when they restricted the period of analysis to 1991-1999, RTC laws were associated with large and statistically significant *increases* in crime. While one could argue that this analysis merely showed that the later adopters of RTC laws experienced crime *increases*, a more sensible conclusion was that other factors not captured in the Lott and Mustard models explained both why RTC adopters looked good in the 1980s and bad in the 1990s.

Ayres and Donohue (2003a) also identified a number of other factors that threatened the strength of the Lott and Mustard more "guns, less crime" hypothesis: the sensitivity of the estimates to the removal of some of Lott and Mustard's 36 demographic controls , the skewed pre-law and post-law comparison resulting from the widely varying years in which RTC laws were adopted, the absence of controls for the incarceration rate and the number of police, and the unreliability of county-level data in general and the problematic arrest rate in particular (see Donohue and Wolfers, 2009).

Ayres and Donohue introduced a more general model, referred to as the hybrid model, which essentially combined the dummy variable and spline models, to measure the immediate *and* long-run impact of RTC laws on crime. Since the hybrid model nests both the dummy and spline models, one can estimate the hybrid and generate either of the other models as a

[4] 143 CONG. REC. S5109 (daily ed. May 23, 1997) (statement of Sen. Craig). The bill was again introduced in 2000 by Congressman Cliff Stearns (R-Florida), who also cited Lott's work. 146 CONG. REC. H2658 (daily ed. May 9, 2000) (statement of Rep. Stearns). Indeed, this proposed legislation, now derisively referred to as "Plaxico's Law," is a perennial favorite of the NRA and frequently introduced by supportive members of Congress (Collins 2009).

special case (depending on what the data shows). After extending the years of Lott and Mustard's original data set, Ayres and Donohue (2003a) estimated this model on both state (through 1999) and county-level (through 1997) data. This exercise generated little support for the "more guns, less crime" hypothesis. In fact, their analysis of the county data set from 1977-1997 using the Lott and Mustard specification (tailored to measure state-specific effects) indicated that RTC laws across all states *raised* total crime costs by somewhere between $3 million and $524 million.

III. Findings of the National Research Council

The sharply conflicting academic assessments of RTC laws specifically and the impact of guns more generally, coupled with the heightened political salience of gun issues, prompted the National Research Council to impanel a committee to critically review the entire range of research on the relationships between guns and violence. The blue-chip committee, which included many notable academics, such as sociologist Charles Wellford (the committee chair), criminologist James Q. Wilson, and economists Joel Horowitz, Joel Waldfogel, and Steven Levitt, issued its wide ranging report in 2005. While the members of the panel agreed on eight of the nine chapters of the NRC report, the single chapter devoted to exploring the causal effects of RTC laws on crime proved to be the most contentious. After reviewing the existing (and conflicting) literature and undertaking their own evaluation of Lott's county-level crime data, 15 of the 16 Committee members concluded that, while they found no support for the Lott and Mustard hypothesis, they could not estimate the true impact of these laws on crime because: (1) the empirical results were imprecise and highly sensitive to changes in model specification, and (2) the estimates were not robust when the data period was extended eight years, through 2000, during which the largest number of states adopted the law.

One can get an inkling of the NRC majority's concern about pronounced model dependence by examining Table 2a, below, which reports estimates from the NRC report of the impact of RTC laws on seven crimes, using the Lott and Mustard dummy and spline models (along with their control variables) on county data for the period 1977-2000. The vastly different results emanating from the two models gave the majority considerable pause. For example, if one believed the dummy model, then RTC laws considerably *increased* aggravated assault and robbery, while the spline model suggested RTC laws *decreased* the rate of both of these crimes.

Two members of the Committee wrote separately on the issue of RTC laws with one seeking to reinforce the majority's skepticism and one seeking to refute it. Joel Horowitz was the radical skeptic. His chapter, entitled "Statistical Issues in the Evaluation of the Effects of Right-to-Carry Laws," represented a wholesale attack on the econometric evaluation of law and policy. James Q. Wilson offered the lone dissent to the Committee's report, claiming that Lott and Mustard's "more guns, less crime" finding actually held up under the committee's reanalysis. Specifically, Wilson rejected the majority's interpretation of the regression estimates shown in Table 2a. Although the NRC panel noted that the Lott and Mustard estimates disagreed in their two models (dummy and spline) for six of the seven crime categories, Wilson emphasized the similar finding of murder rate declines in the two models. The near uniform contradictory results in the two Lott and Mustard models made the panel cautious; the solitary agreement in the murder estimates emboldened Wilson. In other words, while the majority stressed that these regression estimates were 6/7 empty, Wilson stressed that they were 1/7 full. We discuss both the Wilson and Horowitz views in greater detail in the next two sections.

A. Wilson versus the NRC Majority

In some respects, it may not be surprising that Wilson was the lone dissenter, since he had grown up with guns and, prior to the creation of the NRC panel, had written admiringly of Lott's work evaluating RTC laws:

> "Lott's work convinces me that the decrease in murder and robbery in states with shall-issue laws, <u>even after controlling statistically for every other cause of crime reduction</u>, is real and significant. Of the many scholars who were given Lott's data and did their own analyses, most agree with his conclusions. States that passed these laws experienced sharp drops in murder, rape, robbery, and assault, <u>even after allowing for the effects of poverty, unemployment, police arrest rates, and the like</u>. States that did not pass these laws did not show comparable declines. And these declines were not trivial--he is writing about as many as 1,000 fewer murders and rapes and 10,000 fewer robberies. Carrying concealed guns reduces--it does not increase—the rate of serious crime, and that reduction is vastly greater than the generally trivial effect of gun-carrying on accidental shootings." Wilson (2000, emphasis added).

Note that Wilson had previously endorsed Lott's work despite the fact that it did not control for the effect of incarceration on crime. Perhaps Wilson's singular willingness to perceive evidence of the beneficial impact of RTC laws on murder stemmed from his life experience or the lock-in effect of having previously taken a strong position supporting Lott.

1. Wilson's Dissent

The majority tried to signal the fragility of the Lott and Mustard estimates—even with respect to the murder estimates that Wilson found compelling—by noting that if the Lott and Mustard controls were dropped from the panel data estimates (still controlling for state and year fixed effects), the murder results that Wilson embraced simply evaporated. One can see these NRC estimates in Table 1a, which show an insignificant effect for murder in both the dummy and spline models (with the latter being *positive*). The Committee's rationale for including the no-controls model was to underscore that the Lott and Mustard results were sensitive to the included controls and, unless one was confident that the controls were appropriate and complete, one must be wary about embracing the Lott and Mustard results. Wilson simply replied that controls are essential to this type of empirical analysis, so he didn't worry that the no-controls model contradicted his preferred estimates of the beneficial impact of RTC laws on murder.

Wilson also was not persuaded by a part of the majority report that showed that year-by-year estimates revealed no evidence that RTC laws reduced crime for many years after adoption. In a series of graphs mimicking those in Ayres and Donohue (2003a), the Committee noted:

> "The obvious striking feature of these figures is that the big reductions in crime occur roughly 9 years after adoption. Otherwise, the postadoption estimates are generally small and sometimes positive and are, in general, both statistically insignificant and statistically indistinguishable from the preadoption estimates. The trend model essentially fits a line with constant slope through the postadoption portions of these graphs, and the line's slope is affected by years long after adoption. These time patterns raise serious questions about whether the reductions in crime documented in the trend model are reasonably attributed to the change in the law."

Wilson responded to this point by saying that, although the year-by-year analysis showed RTC laws had no effect on murder for many years after adoption, he thought that this might impose too exacting a test on the Lott and Mustard hypothesis: "Estimating the effects of RTC laws by individual years reduces the number of observations and thus the likelihood of finding a significant effect."

But Wilson failed to acknowledge the majority's response to his criticism of the year-by-year analysis. The Committee had estimated a model designed to capture the impact of RTC laws for the five years after passage with an overall (rather than year-by-year) estimate. The Committee found that this estimate suggested RTC laws *increased* murder, although the effect was statistically insignificant. Only beyond six years, when the compositional effects mentioned above obscured the true impact of RTC laws, did the estimates turn negative. Hence, the Wilson critique did not apply to this key evidence that undermined the alleged beneficial impact of RTC laws on murder.

In addition, Wilson criticized the majority's reliance on eight years of data beyond the original Lott and Mustard's period that ended in 1992, even though Lott himself had created the data set through 2000 that the majority employed in its analysis. Wilson stated: "Even if the use of newer data calls into question the original Lott findings, a more reasonable conclusion is that Lott's findings depend on crime rate trends." (NRC Report, p. 271.) Wilson argued that the impact of RTC laws might differ in periods of rising crime (pre-1992) from the impact during periods of declining crime (post-1992). Wilson suggested that RTC laws might be more effective when crime is on the rise than when it is in decline, thereby explaining the better results for the Lott and Mustard thesis with the data set ending in 1992. After dismissing papers by Black and Nagin and Ayres and Donohue on the grounds they were "controversial," Wilson concluded his dissent, stating: "In sum, I find 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 Report, p. 271.)

The dummy model results that the Committee presented showed large *increases* in property crime ranging from 6 to 13 percent with the 1977-2000 county data. Presumably because the trend results showed no such effect, Wilson did not comment on these estimated effects on property crime or indicate why the same model on the same data would generate unreliable estimates for property crimes and non-homicidal violent crimes, but would yield reliable estimates for murder.

2. The Majority's Response to Wilson

The Committee wrote a separate response to Wilson's dissent which stressed that the only disagreement between the majority and Wilson throughout the entire report on all gun issues, concerned the impact of RTC laws on murder. They noted that, despite the number of significant and negative estimates for murder using the Lott and Mustard approach, there were nonetheless several positive, albeit statistically insignificant estimates. Importantly, the results for murder failed to support the "more guns, less crime" contention when restricting the time period of analysis to five years or less after RTC adoption. The important task was to try to reconcile these contradictions, which the committee believed was not possible with the existing literature.

In response to Wilson's criticism of the Committee for failing to accept the results of the Lott and Mustard estimates for murder, the other NRC Committee members stated:

> "There is no question that the empirical results on the effects of right-to-carry laws on murder (and other crimes) are sensitive to seemingly small variations in data and specification. Indeed, Wilson agrees that a few studies find positive effects of right-to-carry laws on murder. We cite four studies in Tables 6-3 and 6-4: Ayres and Donohue (2003a), Black and Nagin (1998), Moody, and Plassmann and Tideman (2001) (cited in Chapter 6). There are almost certainly others not reported in these tables." (p. 273)

Note that "positive" effects imply that RTC laws *increase* crime. Thus, the Committee was unmoved by Wilson's argument since he did not offer any criteria for preferring Lott's single model to other conflicting estimates in the published literature.

The Committee also disagreed with Wilson on the meaning of the result presented in the NRC report using state and year fixed effects, but no other controls (the "no-controls" model). The Committee denied they had claimed that the no-controls model was superior and agreed with Wilson that control variables do matter. However, the no-controls model showed that the set of controls had to be chosen carefully to avoid irrelevant and bias-inducing variables. In light of the disparate estimates that resulted from varying specifications, the Committee did not think this statistical problem could be resolved. The Committee pointed out Wilson's error in interpreting Table 6-7 as presenting estimates on a yearly basis. This table in fact presented estimates while constraining the number of post-adoption years in the analysis as a robustness check, which Lott's trend model failed.

Lastly, the Committee dismissed Wilson's claim that the undermining of Lott's results by the added years of data may simply reflect that RTC laws work better when crime is rising rather than falling. The Committee responded that if this were true, then all of the models would be misspecified. Wilson ignored this econometric criticism, merely responding that his argument for the differential effect of RTC laws was "more plausible." But while Wilson's speculation was creative, it seemed to have little evidentiary support. Ayres and Donohue (2003a) found that the effects of RTC laws estimated during the 1990s (when crime was falling) were all substantially pernicious. Thus, it is hard to see how Wilson could conclude that RTC laws were beneficial: if RTC laws dampened crime increases but undermined crime declines, one would need to weigh the pernicious effects against the putative benefits. Yet, Wilson never engages in such an analysis nor recognizes the competing influences that he implicitly posits. In any event, the Committee concluded its response to Wilson's dissent with the statement "we find his arguments to be unconvincing" and, at times, incorrect, and "we maintain that the scientific evidence does not support his position." (NRC Report, p. 275.)

B. Horowitz's Lament

Committee member (and noted econometrician) Joel Horowitz joined the refutation of Wilson but then authored his own appendix discussing the difficulties of measuring the impact of RTC laws on crime using observational rather than experimental data. While his chapter is directed at the analysis of RTC laws, his comments actually would apply to the vast array of econometric studies that were discussed throughout the entire NRC report, and not just to estimates of the impact of RTC laws.

Horowitz began by addressing the problem of measuring the difference in crime for a particular location with and without the RTC law, as it is not possible to observe both conditions at once. One possible solution is taking the difference in crime rate before and after the adoption of the law. However, he pointed out a number of flaws in this approach. One, if factors other than the adoption of the law change, then this difference in crime would not effectively isolate the impact of the law. Two, if crime increases before the adoption of the law at the same rate it decreases after adoption, then the zero difference would be misleading. The same problem arises for multiyear averages. Three, the adoption of RTC laws may be a response to crime waves. If so, the difference in crime rates may merely reflect these crime waves rather than the effect of the laws. Lastly, as Lott (2000) found in his data, RTC states differ noticeably from non-RTC states (e.g. Republican, low but rising rates of crime). It would not be surprising if these distinctive attributes influence the effect of RTC laws. But in that event, looking at the impact of RTC laws in current RTC states may not be useful for predicting impact of RTC laws if they are adopted in very different states.

Ideally, states would be randomly selected to get RTC laws, thereby eliminating the systematic differences between RTC states and non-RTC states. In the absence of such randomization, researchers introduce controls to try to account for these differences. But this generates debate over which set of controls is appropriate. Lott (2000) defended his model by claiming that it included "the most comprehensive set of control variables yet used in a study of crime" (p. 153). However, Horowitz noted that not only are the data limited for these variables, it is also possible to control for too many variables – or too few. Donohue (2003) found a statistically significant relationship between crime and *future* adoption of RTC laws, suggesting the likelihood of omitted variable bias and/or the endogenous adoption of the RTC laws. Horowitz noted that there is no empirical test that can determine the right set of controls: "it is not possible to carry out an empirical test of whether a proposed set of *X* variables is the correct one…it is largely a matter of opinion which set [of controls] to use" (see Horowitz appendix in NRC report, p. 307).

Horowitz also addressed the likelihood of misspecification in the evaluation of RTC laws. Specifically, the existing literature assumes that the relationship between the crime rate (or the logarithm of the crime rate) and the explanatory variables is linear when in reality this may not be the correct "shape" (i.e., the estimated model may not fit the data). In addition, as the number of explanatory variables increases, the number of possible shapes increases, which in turn increases the likelihood of misspecification. Horowitz stated that none of the models that the committee examined passed a simple specification test (called the RESET test). Noting that estimates derived from a misspecified model can be highly misleading, Horowitz concluded that there was little hope of reaching a scientifically supported conclusion on the validity of Lott's results.

C. Evaluating the NRC Report

At this point, there is something for everyone to be depressed about in the NRC report. If the NRC majority is right, then years of observational work by numerous researchers, topped off with a multi-year assessment of the data by a panel of top scholars, were not enough to tell us anything about the actual impact of RTC laws. If Horowitz is right, then the entire effort to estimate the impact of laws and policies from observational data is doomed. Indeed, few of the more than 400 studies cited across all the chapters of the NRC "Firearms and Violence" report could withstand the Horowitz assault. According to Horowitz, there is simply too much that we do not know about the proper structure of econometric models. As a result, all of observational econometrics is fruitless since one will never know the true model or the correct specification.

Although the majority did not join Horowitz in the broad condemnation of all observational microeconometrics, one can sympathize with Wilson's effort to distill a "best estimate" from the massive amount of econometric evaluations of the impact of RTC laws, which the majority declined to do. However, Wilson's dissent contained some key errors and strained conclusions. First, Wilson was incorrect in stating that the Committee's estimates showing a *positive* (albeit statistically insignificant), and thus crime-increasing, effect of RTC laws on murder only appeared in the year-by-year estimates. In fact it was also shown in the aggregated models for up to four or five years (Table 6-7). This was not a matter of judgment, but a matter of fact, and the majority's published response to Wilson's dissent (Appendix B of the NRC Report) specifically pointed out Wilson's error. Yet Wilson made no correction or acknowledgement on this point.

Second, and perhaps even more disquieting, is the fact that Wilson would adopt the "more guns, less murder" conclusion in the face of so much high-powered opposition when the only support for this conclusion came from a model that one would have thought Wilson would have found facially defective. Wilson stated that the estimates he found to be supportive of the Lott thesis included control variables for "all of the social, demographic, and public policies other than RTC laws that might affect crimes rates" and which "are essential to understanding crime." Lott's model, however, clearly did *not* control for all important influences on crime. Specifically, Lott's model had no control for the impact of crack cocaine, incarceration, and police, all of which had major impacts on crime in the 1980s and 1990s (Levitt 2004). In fact, as Wilson wrote on June 9, 2008, as a guest blogger for the Volokh Conspiracy:

> "The best scholars have estimated that between 25 and 30 percent of the recent decline in crime rates is the result of imprisonment. A comparison with England is helpful. At one time it imprisoned a higher fraction of offenders than did the US, but in the 1980s it changed by imprisoning fewer people. As a result (I think), the British crime rate soared while ours fell." (Wilson 2008)

Perhaps Wilson did not notice that Lott's model omitted a control for incarceration. Although the NRC majority report listed all the control variables in each table, and incarceration was *clearly* not included, the NRC report never explicitly highlights the fact, or importance, of this omission. Given, however, his strong feeling on the value of incarceration in dampening crime, and his own statement that one needed to control for all relevant factors, one might have thought that Wilson would have been alert to this key omission. It is, quite frankly, unimaginable that Wilson would have credited a crime study based on a regression analysis that ignored the impact of incarceration if he had doubts about the study's conclusion. But, while this is the way that most individuals assess evidence – moving from a conclusion to an endorsement of the evidence rather than from an assessment of whether the evidence in fact justifies the conclusion – the entire exercise of statistical study becomes nothing more than useless window-dressing when this approach is employed.

The story thus far has been discouraging for those hoping for illumination on the impact of law and policy through econometric analysis. Horowitz considers the entire enterprise to be worthless, while the majority of committee members could only say that Lott and Mustard's models did not provide reliable evidence on the impact of RTC laws. Only Wilson was willing to take a position, but his dissent was marred by an obvious error in improperly rejecting evidence that undermined the "more guns, less crime" hypothesis and the odd oversight of the massive omitted-variable bias that plagues the Lott and Mustard specifications (which we discuss further below). Unfortunately, there is even more bad news, to which we now turn.

IV. Our Attempt to Replicate the NRC Findings

In a follow-up to their initial 2003 *Stanford Law Review* paper, Ayres and Donohue showed (in an article published in the same journal) how coding errors in data prepared by John Lott yielded inaccurate estimates of the effect of RTC laws on crime. Commenting on a paper by Lott, Florenz Plassman, and John Whitley, Ayres and Donohue (2003b) described numerous coding errors found in the Lott crime data. After correcting these errors, Ayres and Donohue found that the evidence supporting the "more guns, less crime" hypothesis collapsed.[5]

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[5] Lott removed his name from the paper after the data coding errors were revealed, although he claims this decision was prompted by unhappiness with the law review editorial process.

A. Panel-Data Models with No Covariates

Since the NRC panel used data by John Lott, we thought it prudent to carefully examine the NRC committee's new estimates. We first attempt to replicate the results of the report using the NRC 1977-2000 county data set, which the Committee supplied to us. We begin with the Committee's no-controls model, which apart from the dummy and trend variables, only includes year and county fixed effects. The reported NRC estimates are presented in Table 1a, and the first two rows of Table 1b show our efforts at replicating them. While the estimates from the dummy variable model are reasonably close, the trend estimates are not at all comparable: the sign on the estimates in the spline model switch when going from Table 1a to Table 1b for all crimes except auto theft. Table 1b also includes our own estimates from the more flexible version of these specifications—the hybrid model—which combines the dummy and trend approaches. In other words, taken at face value, Table 1b tells us that crime clearly worsened for 6 or 7 crime categories after the passage of RTC laws, regardless of whether one used the dummy variable, spline, or hybrid models.

We contacted the Committee to see if we might be able to understand why the efforts at replication were failing, but the do-files for their tables had not been retained. In an attempt to reconcile the divergence, we initially speculated that perhaps the NRC Committee did not weight its panel-data regressions by county population as we do throughout, but this turned out not to explain the difference. Our best guess is that the NRC *did* weight the regression by population, since they essentially adopted the Lott and Mustard (1997) approach. We also determined that the NRC data set was missing all county identifiers for 1999 and 2000, so we speculated that this might explain the results (since data for any year with a missing country identifier would be omitted from the regression). Again, we could not replicate the NRC spline model results of Table 1a, whether we included all years of data or dropped 1999 and 2000.

At this point, we thought it wise to collect county-level data from scratch and construct our own data set, which we will refer to as the Donohue 2009 data set. We create the same variables found in Lott's data—crime rates, demographic composition, arrest rates, income, population, and population density—and extend the years to as far forward as the data are available—2006 (Lott's data used by the NRC ended in 2000). We also add 0.1 to *all zero* crime values before taking the natural log in our own county-level data set, as the NRC did. This data extension also gives us an opportunity to explore how the NRC's results are affected when using the most up-to-date data available. In Section VII, this additional data will also enable us to estimate the effect of six additional state adoptions of RTC laws: Michigan (2001), Colorado (2003), Minnesota (2003), Missouri (2003), New Mexico (2003), and Ohio (2004).[6]

[6] Kansas and Nebraska adopted RTC laws in 2006, which is too late to be captured in our analysis, since we assume a state to be an "RTC state" beginning in the first *full* year after a law's passage.

Table 1a: The Estimated Impact of RTC Laws – Actual NRC Estimates – No Controls, All Crimes, 1977-2000 (County Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-1.95%	**17.91%**	**12.34%**	**19.99%**	**23.33%**	**19.06%**	**22.58%**
	1.48%	**1.39%**	**0.90%**	**1.21%**	**0.85%**	**0.61%**	**0.59%**
2. Spline model:	0.12%	**-2.17%**	**-0.65%**	**-0.88%**	**0.57%**	**-1.99%**	**-0.71%**
	0.32%	**0.30%**	**0.20%**	**0.26%**	**0.19%**	**0.13%**	**0.13%**

Table 1b: The Estimated Impact of RTC Laws – Using NRC County Data – No Controls, All Crimes, 1977-2000[7]

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.58%	**18.40%**	**12.60%**	**19.70%**	**22.80%**	**19.00%**	**22.60%**
	1.87%	**2.29%**	**1.40%**	**1.75%**	**1.69%**	**1.24%**	**1.08%**
2. Spline model:	-0.57%	**2.36%**	**1.52%**	**2.43%**	**3.17%**	**2.23%**	**3.01%**
	0.34%	**0.39%**	**0.25%**	**0.31%**	**0.30%**	**0.24%**	**0.22%**
3. Hybrid model:							
Postpassage dummy	-0.06%	**16.20%**	**11.90%**	**17.40%**	**16.80%**	**17.70%**	**18.50%**
	2.33%	**2.22%**	**1.69%**	**1.88%**	**1.86%**	**1.34%**	**1.20%**
Trend effect	-0.56%	0.58%	0.22%	0.51%	**1.32%**	0.28%	**0.98%**
	0.43%	0.40%	0.30%	0.35%	**0.35%**	0.27%	**0.25%**

We downloaded our crime data from the University of Michigan's Interuniversity Consortium for Political and Social Research, which has the most comprehensive collection of UCR data. Unfortunately, UCR county crime data for 1993 is currently unavailable. The National Archive of Criminal Justice Data has recently discovered an error in the crime data imputation procedure for 1993 and for this reason, has made 1993 data inaccessible until the error has been corrected. Thus, for all of the following tables with estimates using our updated 2009 county data set, we are missing values for 1993.

Table 1c reproduces Table 1b using our own newly constructed data set (with 1993 omitted). In the case of every crime-model permutation, the switch to this new dataset made the crime-reducing effects of RTC laws look worse. Of course Table 1c differs from Table 1b in two respects – it uses our new dataset instead of the NRC, and it omits 1993 data. To see how important the 1993 omission is, we reproduced Table 1b (using the NRC data) dropping that year, and show the results in Table 1d. As one can see, dropping 1993 data has little effect on the estimates. The bottom line is that 1) we cannot replicate the NRC no-controls estimates of Table 1a whether we use our own newly constructed county data or the data used by the NRC Committee, and 2) the best estimates in the no-controls model overwhelmingly show that all crime was *higher* after RTC laws adoptions (see Table 1c).

[7] For this table, as well as all subsequent tables, 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**.

Table 1c: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – No Controls, All Crimes, 1977-2000 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.20%	**27.80%**	**16.40%**	**19.50%**	**23.90%**	**22.80%**	**28.10%**
	1.87%	**3.53%**	**2.16%**	**2.06%**	**2.27%**	**2.06%**	**2.29%**
2. Spline model:	**0.68%**	**4.65%**	**4.31%**	**3.18%**	**4.72%**	**5.06%**	**6.02%**
	0.28%	**0.46%**	**0.26%**	**0.27%**	**0.28%**	**0.25%**	**0.27%**
3. Hybrid model:							
Postpassage dummy	**-7.99%**	**12.00%**	-3.50%	**8.91%**	**5.50%**	1.44%	3.26%
	2.19%	**3.08%**	2.72%	**2.32%**	**2.70%**	2.60%	2.98%
Trend effect	**1.34%**	**3.66%**	**4.60%**	**2.44%**	**4.27%**	**4.94%**	**5.75%**
	0.33%	**0.37%**	**0.32%**	**0.30%**	**0.32%**	**0.31%**	**0.35%**

Table 1d: The Estimated Impact of RTC Laws – Using NRC County Data – No Controls, All Crimes, 1977-2000 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.91%	**18.50%**	**13.60%**	**20.90%**	**23.70%**	**19.70%**	**23.50%**
	1.93%	**2.38%**	**1.46%**	**1.81%**	**1.75%**	**1.29%**	**1.13%**
2. Spline model:	-0.57%	**2.34%**	**1.55%**	**2.45%**	**3.15%**	**2.22%**	**3.02%**
	0.34%	**0.41%**	**0.25%**	**0.31%**	**0.31%**	**0.24%**	**0.22%**
3. Hybrid model:							
Postpassage dummy	-0.53%	**16.20%**	**13.50%**	**19.20%**	**18.00%**	**18.90%**	**19.80%**
	2.38%	**2.22%**	**1.74%**	**1.95%**	**1.93%**	**1.39%**	**1.23%**
Trend effect	-0.52%	0.59%	0.10%	0.37%	**1.20%**	0.18%	**0.88%**
	0.43%	0.40%	0.31%	0.35%	**0.35%**	0.27%	**0.26%**

B. Panel Data Models With Covariates

After failing to replicate the NRC "no-covariates" model, we next undertook the same replication exercise with the "covariates" model, which adds to the county and year fixed effects model the following Lott and Mustard explanatory variables: arrest rate, population density, real per capita income variables, and 36 variables designed to capture the country's demographic composition.[8] Although we have already noted Lott's claim that this is "the

[8] The NRC uses the Lott-Mustard method of calculating arrest rates, which is the number of arrests crimes divided by the contemporaneous number of crimes. Econometrically, it is inappropriate to use this contemporaneous measure since it leaves the dependent variable on both sides of the regression equation (a better approach would lag this variable one year, as discussed in Ayres and Donohue (2009b)). Another issue about the arrest rates is unclear: the NRC report does not indicate whether it uses the individual Index I crime categories to compute arrest rates, or alternatively, if they use the broad categories of violent and property crimes, as has been used in recent papers (Moody and Marvell 2008; Moody and Marvell 2009). We adopt this latter approach for all tables in this paper, although we also explored the possibility of arrest rates for individual crimes. Regardless of which arrest rate we used, our estimates still diverged considerably from the estimates presented by the NRC.

most comprehensive set of control variables yet used in a study of crime," in fact, this set of variables omits many important influences on crime, which we will re-introduce in Section VIII, below.

Just to be clear about our approach, we use annual county-level panel crime data for the United States from 1977 through either 2000 (to conform to the NRC report) or 2006 (the last year for which data is available). We explore the impact of RTC laws on seven Index I crime categories by estimating the reduced-form regression in equation (1) below:

$$Y_{it} = \eta RTC_{jt} + \alpha_i + \theta_t + \beta_{jt} + \gamma X_{ijt} + \varepsilon_{it} \qquad (1)$$

where the dependent variable Y_{it} denotes the natural log of the individual violent and property crime rates for county *i* and year *t*. Our explanatory variable of interest—the presence of an RTC law within a particular state *j* in year *t*—is represented by RTC_{jt}. The exact form of this variable shifts according to the three variations of the model we employ. These include the Lott and Mustard dummy and spline models, as well as the Ayres and Donohue hybrid model.[9]

In this fixed-effects model, the variable α_i indicates county-level fixed effects (unobserved county traits) and θ_t stands for year effects. As we will discuss below, there is no consensus on the use of state-specific time trends in this analysis, and the NRC report did not address this issue. Nevertheless, we will explore this possibility, with β_{jt} indicating these state-specific trends, which are introduced in selected models. Since neither Lott and Mustard (1997) nor the NRC (2005) examine state trends, this term is dropped when we estimate their models. The term X_{ijt} represents a matrix of observable county and state characteristics thought by researchers to influence criminal behavior. The components of this term, however, varies substantially across the literature. For example, while Lott uses only "arrest rates" as a measure of criminal deterrence, we discuss the potential need for other measures of deterrence, such as incarceration levels or police presence, which are measured at the state level.

In Tables 2a through 2d, we follow the same pattern of Tables 1a through 1d: we begin by showing the NRC published estimates (Table 2a) and then show our effort at replication using the NRC data set (Table 2b). We then show our estimates based on our reconstruction of the county dataset from 1977-2000 (Table 2c, which omits 1993 data), and show the effect of omitting 1993 data on our Table 2b estimates (Table 2d). The basic story that we saw above with respect to the no-covariates model holds yet again: we cannot replicate the NRC results using the NRC's own data set (compare Tables 2a and 2b), and omitting 1993 data does not make much difference (compare Tables 2b and 2d). Once again, our Table 2c estimates diverge wildly from the Table 2a estimates, which appeared in the NRC report. As

[9] As noted previously, in the dummy variable approach, the RTC variable is a dichotomous indicator that takes on a value of one in in the first full year that a state j has an RTC law. In the spline model, the RTC variable indicates the number of post-passage years. The hybrid specification contains both dummy and trend variables.

we will see in a moment, what James Q. Wilson had found to be consistent evidence of RTC laws reducing murder (the Table 2a results) was completely inaccurate (see Table 2c). Presumably, had Wilson been given more accurate estimates, he would not have endorsed the view that RTC laws reduce the rate of murder.

C. Potential Problems with the NRC Models and Data

Before turning to the implications of the errors in the NRC estimates, we note a few small errors in the NRC data that we corrected in all our tables. First, we identified an extraneous demographic variable that caused a substantial number of observations to drop from the NRC dataset (over 20,000).[10] We do not know if the committee dropped this variable before conducting its analysis, but we drop it in our own analysis.[11] Second, Philadelphia's year of adoption is coded incorrectly—as 1995 instead of 1996. Third, Idaho's year of adoption is coded incorrectly—as 1992 instead of 1991. Fourth, the area variable, which is used to compute county density, has missing data for years 1999 and 2000.[12]

Table 2a: The Estimated Impact of RTC Laws – Actual NRC Estimates – With Lott and Mustard Controls, All Crimes, 1977-2000

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	**-8.33%**	-0.16%	**3.05%**	**3.59%**	**12.74%**	**6.19%**	**12.40%**
	1.05%	0.83%	**0.80%**	**0.90%**	**0.78%**	**0.57%**	**0.59%**
2. Spline model:	**-2.03%**	**-2.81%**	**-1.92%**	**-2.58%**	**-0.49%**	**-2.13%**	**-0.73%**
	0.26%	**0.20%**	**0.20%**	**0.22%**	**0.13%**	**0.19%**	**0.13%**

[10] The variable is called "ppnpermpc." We stumbled into using this variable as we tried to incorporate Lott and Mustard's 36 demographic variables, which denote the percentage of each county's population that falls into each of six age groups based on three racial categories for men and for women. All 36 of these variables begin with the prefix "ppn," which will then be included in the analysis if one uses a STATA command that groups together all variables with this common "ppn" prefix. For example, "ppnm2029" indicates the percentage of a county population that is male and neither white nor black. We do not know how the "ppnpermpc" variable fits into this grouping (or even if it is meant be a part of this group of variables). The mean value of this variable is -3.206657, with the individual observations ranging from -12.05915 to 4.859623. While the the other ppn variables reflect some sort of percentage, the mean negative value obviously indicates that this variable is not a percentage.

[11] We found that whether we include or exclude this variable, we still cannot replicate the NRC's results shown in our Table 2a.

[12] Because the NRC area numbers are the same for a county across all years, we fill in this gap by simply using the 1998 values for these two years. (However, we note that area should *not* be constant across all years, as the Census updates this data every decade.) We include complete area data in our new dataset.

Table 2b: The Estimated Impact of RTC Laws – Using NRC Data – With Lott and Mustard Controls, All Crimes, 1977-2000

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.80%	**10.50%**	**11.20%**	**11.20%**	**16.80%**	**11.00%**	**17.60%**
	2.14%	**2.18%**	**1.55%**	**1.81%**	**1.54%**	**0.98%**	**0.86%**
2. Spline model:	-0.61%	**1.38%**	**1.91%**	**1.63%**	**2.61%**	**1.62%**	**3.12%**
	0.38%	**0.36%**	**0.25%**	**0.32%**	**0.29%**	**0.19%**	**0.17%**
3. Hybrid model:							
Postpassage dummy	-2.51%	**9.77%**	**7.01%**	**9.02%**	**12.20%**	**8.92%**	**9.72%**
	2.63%	**2.28%**	**1.76%**	**1.92%**	**1.74%**	**1.06%**	**0.94%**
Trend effect	-0.30%	0.18%	**1.05%**	0.53%	**1.11%**	**0.52%**	**1.92%**
	0.47%	0.36%	**0.27%**	0.33%	**0.34%**	**0.22%**	**0.19%**

Table 2c: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – With Lott and Mustard Controls, All Crimes, 1977-2000 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	**-3.80%**	**9.82%**	**8.96%**	**5.44%**	**13.60%**	**4.36%**	**12.90%**
	1.87%	**2.74%**	**1.34%**	**1.45%**	**1.40%**	**0.95%**	**0.88%**
2. Spline model:	-0.26%	0.48%	**1.10%**	0.26%	**1.50%**	**0.30%**	**1.16%**
	0.28%	0.33%	**0.18%**	0.21%	**0.19%**	**0.15%**	**0.14%**
3. Hybrid model:							
Postpassage dummy	-3.98%	**11.40%**	**6.34%**	**6.39%**	**10.60%**	**4.53%**	**11.80%**
	2.22%	**2.62%**	**1.48%**	**1.66%**	**1.57%**	**1.05%**	**0.94%**
Trend effect	0.04%	-0.38%	**0.63%**	-0.23%	**0.70%**	-0.04%	0.28%
	0.33%	0.30%	**0.20%**	0.25%	**0.22%**	0.16%	0.15%

Table 2d: The Estimated Impact of RTC Laws – Using NRC Data – With Lott and Mustard Controls, All Crimes, 1977-2000 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-4.25%	**11.00%**	**12.20%**	**12.00%**	**17.30%**	**11.50%**	**18.40%**
	2.26%	**2.30%**	**1.65%**	**1.91%**	**1.60%**	**1.02%**	**0.90%**
2. Spline model:	-0.59%	**1.38%**	**1.94%**	**1.65%**	**2.56%**	**1.60%**	**3.15%**
	0.39%	**0.37%**	**0.26%**	**0.32%**	**0.29%**	**0.20%**	**0.17%**
3. Hybrid model:							
Postpassage dummy	-3.44%	**10.50%**	**8.46%**	**10.30%**	**13.30%**	**9.73%**	**10.80%**
	2.74%	**2.37%**	**1.87%**	**2.02%**	**1.81%**	**1.10%**	**0.97%**
Trend effect	-0.17%	0.12%	**0.92%**	0.41%	**0.96%**	0.43%	**1.85%**
	0.48%	0.35%	**0.28%**	0.34%	**0.34%**	0.23%	**0.20%**

The major differences in Table 2a (the NRC Committee's estimates) and Table 2c (what we think is the best estimate of what the NRC intended to present) are profound enough that they might well have changed the nature of the report. Recall that Wilson had looked at the Table

2a results and decided that since the dummy and spline estimates were both consistent and statistically significant for only one crime – murder – these were the only estimates that should be accepted. But applying this same logic to the Table 2c estimates would lead to the quite different conclusion that for four crimes -- aggravated assault, auto theft, burglary, and larceny -- Table 2c provides uniform evidence that RTC laws *increase* crime (while the evidence for the other crimes is mixed). One might go further and say that all the Table 2c dummy and spline estimates show crime *increases*, except for murder.

Although we think that Table 2c reflects where the NRC Committee should have ended up if it wanted to portray Lott and Mustard's country data analysis, there is actually far more that the Committee could have done to go beyond Table 2c. This is not necessarily a criticism of the NRC majority since it concluded that the evidence was already too fragile to draw strong conclusions, and further support for this conclusion would merely have been cumulative. However, we now turn to some avenues of inquiry that Wilson might have considered before adopting the Lott and Mustard conclusion vis-à-vis murder.

V. Debate over the Clustering of Standard Errors

A. Is Clustering Necessary?

To this point we have said little about the important question of estimating the standard errors in panel data estimates. The estimates presented thus far follow the NRC in providing heteroskedasticity-robust standard errors. Recent research has found, though, that the issue whether or not to "cluster" the standard errors may have a profound impact on assessments of statistical significance. This issue gained prominence beginning primarily with a 1990 paper by Brent Moulton. Moulton (1990) pointed to the possible need for the clustering of observations when treatments are assigned at a group-level. In such cases, there is an additive source of variation that is the same for all observations in the group, and ignoring this unique variation leads to standard errors that are underestimated. Lott, however, suggests that clustered standard errors are not needed (Lott 2004). He believes that including county-level fixed effects in the model implicitly controls for state-level effects, and therefore, clustering the standard errors by state is unnecessary.

On this point, the NRC committee (2005) sided with Lott, stating that "There is no need for adjustments for state – level clustering." (p. 138). We agree with the NRC report that the problem that Moulton addressed does not require clustering in our case. But it is now recognized that there is a second reason for clustering that the NRC report did not address. Specifically, serial correlation in panel data can lead to major underestimation of standard errors. Wooldridge (2003, 2006), as well as Angrist and Pischke (2009), suggest that clustering the standard errors by state (along with heteroskedasiticity-robust standard errors) will help address this problem.

B. Does Clustering Influence the Results?

To get a sense of how clustering would have changed the NRC's estimates, we run the NRC committee's covariates model *with standard errors clustered by state* on our county-level data set. Table 3 shows that clustering the standard errors in this model eliminates most of the statistical significance we saw in Table 2c (the same model but without clustering). The

Table 3 estimates provide no support for the claim that RTC laws reduce crime, and in fact reveals evidence that aggravated assault, auto theft, and larceny all rise by between 9 and 14 percent. While this might suggest that RTC laws *increase* crime, the auto theft and larceny results do not readily comport with any plausible theory about the impact of RTC laws, and so we would proceed with caution in interpreting those results (even if we had greater confidence in the Lott and Mustard speculation than we do).[13]

Table 3: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – With Lott and Mustard Controls, With Clustered Standard Errors, All Crimes, 1977-2000 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.80%	9.82%	8.96%	5.44%	**13.60%**	4.36%	**12.90%**
	6.25%	11.20%	5.33%	5.53%	**5.83%**	3.58%	**3.97%**
2. Spline model:	-0.26%	0.48%	1.10%	0.26%	1.50%	0.30%	1.16%
	0.80%	1.22%	0.81%	0.85%	0.83%	0.50%	0.82%
3. Hybrid model:							
Postpassage dummy	-3.98%	11.40%	6.34%	6.39%	10.60%	4.53%	**11.80%**
	7.08%	10.20%	4.43%	5.69%	6.18%	3.92%	**2.95%**
Trend effect	0.04%	-0.38%	0.63%	-0.23%	0.70%	-0.04%	0.28%
	0.89%	0.86%	0.76%	0.81%	0.77%	0.49%	0.65%

Table 4: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – With Lott and Mustard Controls, With Clustered Standard Errors and State Trends, All Crimes, 1977-2000 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-6.17%	-10.80%	3.00%	-5.31%	0.21%	-5.19%	-0.40%
	5.31%	8.27%	3.60%	5.66%	5.85%	3.55%	3.04%
2. Spline model:	-1.21%	-2.64%	**3.02%**	-0.06%	0.82%	0.00%	1.18%
	1.46%	3.48%	**1.23%**	2.26%	1.27%	1.29%	1.12%
3. Hybrid model:							
Postpassage dummy	-5.14%	-8.28%	-0.64%	-5.69%	-0.83%	-5.63%	-1.95%
	5.07%	5.65%	3.79%	6.28%	5.99%	3.95%	3.25%
Trend effect	-0.87%	-2.09%	**3.06%**	0.32%	0.88%	0.38%	1.31%
	1.43%	3.28%	**1.29%**	2.42%	1.30%	1.40%	1.19%

C. Using Placebo Laws to Test the Impact of Clustering

We have just seen that clustering makes a major difference in the results generated by the Lott and Mustard models that the NRC report used. But who is correct on the clustering issue—Lott and the NRC panel on the one hand, or Wooldridge, Angrist and Pischke, as well as most high-end applied econometricians on the other? To address this important question

[13] Lott and Mustard offered a crime-substitution theory based on a view that if RTC laws reduced robbery (because criminals feared encountering armed victims), the criminals might turn to property crimes that were less likely to result in armed resistance. Note, though, that Table 3 gives no support for a robbery-reduction effect, so the premise of the crime-substitution story is not supported.

we run a series of placebo tests. In essence, we randomly assign RTC laws to states, and re-estimate our model iteratively (1000 times), counting the number of times that the variable(s) of interest are "statistically significant." For this experiment, we use the most flexible model explored in this paper: the hybrid model (that incorporates both a dummy and a trend variable), with the Lott and Mustard controls employed by the NRC panel.

We run three versions of this test. For the first test, we first generate a placebo law in a random year for all 50 states and the District of Columbia. Once the law is applied, it persists for the rest of our data period. Second, we apply a placebo law in a random year to the 32 states that actually implemented right-to-carry laws during the period we are analyzing. The remaining 19 states assume no RTC law. Finally, we randomly select 32 states to receive a placebo law in a random year. The results of these three tests are presented in Table 5a.

Given the random assignment, one would expect to reject the null hypothesis of no effect of these randomized "laws" roughly 5 percent of the time if the standard errors in our regressions are estimated correctly. Instead, the table reveals that the null hypothesis is rejected 50-70 percent of the time for murder and robbery with the dummy variable and even more frequently with the trend variable (60-74 percent). Clearly, this exercise suggests that the standard errors used in the NRC report are far too small.

Table 5b replicates the exercise of Table 5a but now uses the STATA cluster correction for standard errors (clustering by state). Table 5b suggests that clustering standard errors does not excessively reduce significance, as the NRC panel feared. In fact, the percentages of "significant" estimates produced in all three versions of the test still lie well beyond the 5% threshold. Tables 5c and 5d employ the dummy model instead of the hybrid model, but show results similar to Tables 5a and 5b. All of these tests show that if we do *not* cluster the standard errors, the likelihood of obtaining significant estimates is astonishingly (and unreasonably) high. The conclusion we draw from this exercise is that clustering is clearly needed to adjust the standard errors in these panel data regressions. Accordingly, we will use this clustering adjustment for all remaining regressions in this paper.

Table 5a: Hybrid Model - Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 County-Level Data – Lott and Mustard Controls, Without Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable	Trend Variable
1. All 50 States + D.C.	Murder	50.2%	67.4%
	Robbery	56.7%	65.6%
2. Exact 32 States	Murder	64.2%	71.9%
	Robbery	59.8%	67.2%
3. Random 32 States	Murder	57.8%	59.9%
	Robbery	70.6%	74.2%

Table 5b: Hybrid Model - Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 County-Level Data – Lott and Mustard Controls, With Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable	Trend Variable
1. All 50 States + D.C.	Murder	8.9%	11.5%
	Robbery	8.1%	8.1%
2. Exact 32 States	Murder	10.0%	11.0%
	Robbery	9.2%	7.1%
3. Random 32 States	Murder	11.2%	13.5%
	Robbery	10.3%	8.8%

Table 5c: Dummy Variable Model – Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 County-Level Data – Lott and Mustard Controls, Without Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable
1. All 50 States + D.C.	Murder	51.1%
	Robbery	66.1%
2. Exact 32 States	Murder	57.1%
	Robbery	66.3%
3. Random 32 States	Murder	58.7%
	Robbery	59.3%

Table 5d: Dummy Variable Model – Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 County-Level Data – Lott and Mustard Controls, With Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable
1. All 50 States + D.C.	Murder	9.0%
	Robbery	6.5%
2. Exact 32 States	Murder	6.2%
	Robbery	5.8%
3. Random 32 States	Murder	10.4%
	Robbery	9.2%

VI. Debate over the Inclusion of Linear Trends

An important issue that the NRC did not address was whether there was any need to control for state-specific linear trends. Inclusion of state trends could be important if, for example, a clear pattern in crime rates existed before a state adopted an RTC law that continued into the post-passage period. On the other hand, there is also a potential danger in using state-specific trends if their inclusion inappropriately extrapolates a temporary swing in crime long into the future. Lott and Mustard (1997) never controlled for state-specific trends in analyzing handgun laws, while Moody and Marvel (2008, 2009) always control for these trends. Ayres and Donohue (2003a) present evidence with and without such trends.

Table 4 replicate the NRC's Lott and Mustard covariates model in Table 3 while adding linear state trends to this county data model. Strikingly, Table 4 suggests that RTC laws increase aggravated assault by roughly 3 percent each year, but no other statistically significant effect is observed. Thus, the addition of state trends to Table 3 eliminates the potential problematic result of RTC laws increasing property crimes, which actually increases our confidence in these results. Certainly an increase in gun carrying and prevalence induced by a RTC law could well be thought to spur more aggravated assaults. Nonetheless, one must at least consider whether the solitary finding of statistical significance is merely the product of running seven different models, is a spurious effect flowing from a bad model, or reflects some other anomaly (such as changes in the police treatment of domestic violence cases, which could confound the aggravated assault results).[14]

VII. Extending the Data Through 2006

Thus far we have been presenting panel-data regression results for the period 1977-2000. Since more data is now available, we can estimate the NRC Lott and Mustard covariates specification on data extended through 2006. Since we have become convinced from our placebo exercise that some method of adjusting the standard errors to control for serial correlation is necessary, we only present results with a cluster adjustment. Table 6a presents the comparable estimates with clustering, which can be compared with Table 3 (which also clusters but is estimated on the shorter time period). This comparison reveals that the additional six years of data somewhat strengthens the evidence that RTC laws *increase* aggravated assault, auto theft, burglary, and larceny. Table 6b simply adds state trends to the Table 6a models, which can then be compared to Table 4 (clustering, state trends, and 1977-2000 data).

The regressions in Table 6 reveal that the added six years of data do not appreciably change the results on the shorter period. The inclusion of state trends on the longer data set renders all estimates insignificant except for the evidence of marginally significant *increases* in aggravated assault.

[14] We tested this theory by creating a new right-hand side dummy variable that identified if a state passed legislation requiring law enforcement officials to submit official reports of all investigated domestic violence cases. Eight states have passed this legislation of which we are aware: Florida (1984), Illinois (1986), Louisiana (1985), New Jersey (1991), North Dakota (1989), Oklahoma (1986), Tennessee (1995), and Washington (1979). We included this dummy variable when running both the NRC specification (through 2000) and our preferred specification (through 2006), and found that this dummy indicator of domestic violence reporting statutes did not undermine the finding that RTC laws *increase* aggravated assaults.

Table 6a: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – With Lott and Mustard Controls, With Clustered Standard Errors, All Crimes, 1977-2006 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-5.44%	10.40%	**11.40%**	3.10%	**14.40%**	<u>7.48%</u>	<u>**12.90%**</u>
	5.91%	13.20%	**4.84%**	4.47%	**6.65%**	<u>3.85%</u>	<u>**3.96%**</u>
2. Spline model:	-0.28%	0.61%	1.05%	0.39%	0.99%	0.44%	**1.07%**
	0.60%	1.03%	0.69%	0.54%	0.61%	0.43%	**0.51%**
3. Hybrid model:							
Postpassage dummy	-5.35%	9.77%	**8.39%**	1.69%	**12.60%**	<u>6.99%</u>	<u>**10.10%**</u>
	6.05%	12.00%	**3.48%**	5.43%	**5.91%**	<u>3.99%</u>	<u>**3.68%**</u>
Trend effect	-0.02%	0.14%	0.65%	0.30%	0.39%	0.10%	0.59%
	0.61%	0.74%	0.63%	0.65%	0.47%	0.44%	0.49%

Table 6b: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – With Lott and Mustard Controls, With Clustered Standard Errors and State Trends, All Crimes, 1977-2006 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-4.45%	-13.00%	3.44%	-0.22%	3.81%	-0.77%	1.51%
	4.44%	8.14%	3.13%	5.48%	4.84%	3.53%	3.10%
2. Spline model:	-0.96%	-4.51%	<u>1.72%</u>	-0.95%	-0.91%	-0.82%	-0.66%
	0.96%	3.74%	<u>0.94%</u>	1.60%	1.10%	1.04%	0.87%
3. Hybrid model:							
Postpassage dummy	-3.98%	-10.70%	2.53%	0.31%	4.36%	-0.32%	1.89%
	4.55%	7.01%	3.09%	5.55%	4.67%	3.64%	3.08%
Trend effect	-0.86%	-4.26%	<u>1.66%</u>	-0.96%	-1.01%	-0.82%	-0.70%
	0.98%	3.69%	<u>0.93%</u>	1.62%	1.08%	1.07%	0.89%

VIII. Revising the Lott and Mustard Specification

We have already suggested that the Lott and Mustard specification that the NRC employed is not particularly appealing along a number of dimensions. The most obvious problem – omitted variable bias -- has already been alluded to in discussing Wilson's recent writing that stresses the impact of increased incarceration on crime: the Lott and Mustard model had no control for incarceration, which Wilson considered to be one of the most important influences on crime in the last 20 years. In addition to a number of important omitted variables, the Lott and Mustard model includes a number of questionable variables, such as the highly dubious ratio of arrests to murders, and the 36 demographic controls.[15]

To explore whether these specification problems are influencing the resulting regression estimates, we alter the Lott and Mustard models in a number of ways. First, we drop the arrest rate variable and add in two preferable measures of state law enforcement: the incarceration rate and the rate of police, both of which are measured at the state level. Second, we add two additional controls to capture economic conditions: the unemployment

[15] For extended discussion on the abundant problems with this pseudo arrest rate, see Donohue and Wolfers (2009).

rate and the poverty rate, which are also state-level variables. Finally, mindful of Horowitz's admonition that the Lott and Mustard model might have *too many* variables, we reduce the number of demographic controls from 36 to 6, and only control for the percent of black males and white males age 10-19, 20-29, and 30-39 in each county.

The results with this new specification are presented in Tables 7a-7b (which correspond to Tables 6a-6b estimated using the Lott and Mustard specification). In particular, one sees a strong adverse shift for murder. Note that had the NRC panel used our preferred specification while maintaining its view that neither clustering nor controls for state trends are needed, then we would have overwhelming evidence that RTC laws *increase* crime across every crime category. We don't show this regression since we are convinced that clustering is needed, although of course when we cluster in Table 7a, the point estimates remain the same (although significance is drastically reduced). It would indeed be a troubling state of the world if the NRC view on clustering (and linear trends) were correct, for in that event, RTC laws would *increase* every crime category other than murder by 20-40 percent (the dummy model) or increase it by 2-4 percent every year (the spline model) – all at the .01 level. At the same time, the murder results are weaker statistically in this non-clustered model but still show that RTC laws *increase* murder at the .10 level in the spline model and at the .05 level in the trend term of the hybrid model.

When we do cluster, however, as shown in Table 7a, we are left with large positive point estimates but far fewer significant results: nonetheless, this more reasonable specification with clustering suggests (albeit at only the .10 level) that RTC laws *increase* aggravated assault, robbery, and larceny. Interestingly, adding state trends in Table 7b wipes out all evidence of statistical significance.

This discussion reveals how critical the choices of clustering and state trends are to an assessment of RTC laws. Using neither, these laws are uniformly and profoundly harmful. With only clustering, RTC laws show (marginally significant) signs of increases for two violent crime categories as well as for larceny. With both clustering and state trends, all statistically significant effects are wiped out. The only clear conclusion from either the Lott and Mustard specification or our preferred specification on county data is that RTC laws *never* provide any net benefits, and may cause either a little or a great deal of harm.

Table 7a: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – With Preferred Controls, With Clustered Standard Errors, All Crimes, 1977-2006 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.44%	21.30%	21.60%	19.30%	24.80%	26.60%	29.50%
	7.13%	19.40%	19.00%	14.50%	21.10%	22.40%	26.00%
2. Spline model:	0.31%	2.34%	3.16%	2.64%	3.12%	3.59%	4.20%
	0.79%	1.83%	1.89%	1.46%	2.11%	2.27%	2.61%
3. Hybrid model:							
Postpassage dummy	-2.72%	12.60%	7.40%	7.92%	12.00%	11.10%	10.90%
	6.96%	15.40%	15.80%	12.10%	16.80%	18.20%	20.50%
Trend effect	0.45%	1.70%	2.78%	2.24%	2.51%	3.03%	3.64%
	0.81%	1.39%	1.62%	1.27%	1.74%	1.94%	2.15%

Table 7b: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – With Preferred Controls, With Clustered Standard Errors and State Trends, All Crimes, 1977-2006 (Without 1993 Data)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.11%	-15.50%	0.02%	1.15%	1.89%	-3.98%	-3.22%
	4.81%	10.80%	9.70%	7.25%	9.89%	10.90%	12.50%
2. Spline model:	-0.41%	-6.69%	0.61%	-0.82%	-0.97%	-1.92%	-2.25%
	1.31%	4.77%	2.44%	2.28%	2.66%	2.83%	3.15%
3. Hybrid model:							
Postpassage dummy	-2.97%	-13.00%	-0.22%	1.48%	2.29%	-3.25%	-2.35%
	5.08%	9.98%	10.30%	7.64%	10.40%	11.50%	13.10%
Trend effect	-0.35%	-6.46%	0.61%	-0.85%	-1.01%	-1.87%	-2.21%
	1.35%	4.76%	2.54%	2.35%	2.76%	2.96%	3.29%

IX. State versus County Crime Data

In their initial study, Lott and Mustard (1997) tested the more guns, less crime hypothesis by relying primarily on county-level data from the FBI's *Uniform Crime Reports* (UCR).[16] These FBI reports present yearly estimates of crime based on monthly crime data from local and state law enforcement agencies across the country. The NRC report followed Lott and Mustard in this choice and presented regression estimates using only county data. But, according to criminal justice researcher Michael Maltz, the FBI's county-level data is highly problematic.

The major problem with county data stems from the fact that law enforcement agencies voluntarily submit crime data to the FBI. As a result, the FBI has little control over the accuracy, consistency, timeliness, and completeness of the data it uses to compile the UCR reports. In a study published in the *Journal of Quantitative Criminology,* Maltz and Targonski (2002) carefully analyzed the shortcomings in the UCR data set and concluded that UCR county-level data is unacceptable for evaluating the impact of RTC laws. For example, in Connecticut, Indiana, and Mississippi, over 50% of the county-level data points are missing crime data for more than 30% of their populations (Maltz and Targonski 2002). In another thirteen states, more than 20% of the data points have gaps of similar magnitude.

Based on their analysis, Maltz and Targonski concluded that:

> "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 shortcomings 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

[16] Lott and Mustard do present results that are based on state-level data, but they strongly endorse their county-level analysis over their state-level analysis: "the very different results between state- and county-level data should make us very cautious in aggregating crime data and would imply that the data should remain as disaggregated as possible" Lott and Mustard, 1997: 39.

their current condition, county-level UCR crime statistics cannot be used for evaluating the effects of changes in policy" (pp. 316-317).

Because of the concerns raised about county-level crime data, it is prudent to test our models on state-level data. According to Maltz and Targonski (2003), state-level crime data are less problematic than county-level data because the FBI's state-level crime files take into account missing data by imputing all missing agency data. County-level files provided by NACJD, however, impute missing data only if an agency provides at least six months of data; otherwise, the agency is dropped completely (Maltz 1999). As with our estimations using county-level data, we compiled the state-level data from scratch, and will refer to it here as "Donohue 2009 State-level Data."

Unsurprisingly, the regression results reproduced using state-level data are again different from the NRC committee's estimates using county-level data. This is shown in Table 8a, which presents the NRC's covariates specification (the Lott and Mustard specification) but with the cluster adjustment.[17] Table 8b simply adds state trends to the Table 8a estimates. When we compare these state-level estimates to the county-level estimates (using the Donohue 2009 county-level data set), we see that there are marked differences. Considering the preceding discussion on the reliability—or lack thereof—of county data, this result is unsurprising. Importantly, state level data through 2000 (the last year depicted in the NRC report) show not a hint of any statistically significant evidence that RTC laws reduce murder.

We follow up the Table 8a and 8b estimations by again extending our data through 2006 (Tables 9a and 9b). Interestingly, while the Lott and Mustard specification on county data for both 1977-2000 and 1977-2006 (Tables 6 and 7) intimated that either RTC laws increased or had no effect on crime, the comparable Tables 8 and 9 run on state data are a bit muddier with some models in particular years suggesting some crime increases and others showing crime decreases. None of the state results are robust to the addition or exclusion of state linear trends. RTC laws never reduce the rate of murder.

Table 8a: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Lott and Mustard Controls, With Clustered Standard Errors, All Crimes, 1977-2000

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.61%	-3.63%	-1.10%	-3.73%	4.30%	**-4.21%**	**3.27%**
	4.15%	2.97%	3.64%	3.90%	3.88%	**1.83%**	**1.38%**
2. Spline model:	0.43%	-0.43%	0.90%	0.20%	0.14%	-0.12%	**0.67%**
	0.71%	0.55%	0.80%	0.81%	0.49%	0.51%	**0.31%**
3. Hybrid model:							
Postpassage dummy	-6.44%	-2.82%	-5.20%	-5.62%	4.90%	**-4.90%**	1.34%
	4.67%	2.60%	3.12%	3.95%	4.86%	**2.06%**	1.48%
Trend effect	0.85%	-0.24%	1.23%	0.57%	-0.18%	0.20%	0.58%
	0.76%	0.57%	0.83%	0.86%	0.61%	0.53%	0.35%

[17] Our placebo test on county data showed that standard errors needed to be adjusted by clustering. In Appendix B, we repeat this exercise for the state data, and again find that clustering is needed. As a result, all our state data estimates use the clustering adjustment.

Table 8b: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Lott and Mustard Controls, With Clustered Standard Errors and State Trends, All Crimes, 1977-2000

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-4.86%	-4.28%	-4.15%	-4.78%	0.87%	-2.43%	1.41%
	4.24%	2.24%	2.18%	4.03%	2.63%	2.00%	1.58%
2. Spline model:	-0.99%	0.27%	**2.38%**	-0.67%	**-2.22%**	-0.52%	0.46%
	0.97%	0.78%	**0.86%**	1.27%	**1.08%**	0.97%	0.63%
3. Hybrid model:							
Postpassage dummy	-4.19%	**-5.06%**	**-7.44%**	-4.48%	3.64%	-2.06%	1.01%
	4.35%	**2.29%**	**2.60%**	4.03%	3.27%	1.85%	1.62%
Trend effect	-0.62%	0.72%	**3.03%**	-0.27%	**-2.55%**	-0.34%	0.37%
	0.95%	0.80%	**0.99%**	1.25%	**1.22%**	0.97%	0.67%

Table 9a: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Lott and Mustard Controls, With Clustered Standard Errors, All Crimes, 1977-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-4.94%	**-5.04%**	1.44%	**-6.96%**	0.31%	**-4.97%**	2.32%
	3.61%	**2.29%**	4.11%	**2.90%**	3.98%	**2.22%**	1.58%
2. Spline model:	-0.03%	-0.49%	0.80%	-0.16%	**-0.87%**	-0.44%	0.40%
	0.54%	0.33%	0.66%	0.60%	**0.42%**	0.45%	0.29%
3. Hybrid model:							
Postpassage dummy	-5.62%	-3.77%	-1.69%	**-7.41%**	4.00%	-3.92%	1.03%
	4.25%	2.36%	3.26%	**3.59%**	4.88%	2.03%	1.80%
Trend effect	0.19%	-0.35%	0.86%	0.12%	**-1.02%**	-0.29%	0.36%
	0.58%	0.36%	0.64%	0.64%	**0.50%**	0.46%	0.32%

Table 9b: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Lott and Mustard Controls, With Clustered Standard Errors and State Trends, All Crimes, 1977-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.32%	-3.33%	-1.12%	-3.36%	2.64%	-1.93%	1.21%
	3.47%	2.20%	2.78%	3.04%	2.71%	1.37%	1.07%
2. Spline model:	0.42%	0.34%	**2.49%**	0.46%	**-1.95%**	0.35%	0.39%
	0.82%	0.88%	**0.61%**	1.00%	**0.72%**	0.79%	0.60%
3. Hybrid model:							
Postpassage dummy	-3.83%	-3.78%	-3.33%	-3.90%	4.51%	-2.33%	0.92%
	3.58%	2.42%	2.84%	3.10%	2.85%	1.62%	1.28%
Trend effect	0.61%	0.54%	**2.67%**	0.66%	**-2.19%**	0.47%	0.35%
	0.81%	0.92%	**0.63%**	1.00%	**0.77%**	0.83%	0.64%

Tables 10 and 11 repeat Tables 8 and 9 but use our preferred set of explanatory variables instead of the Lott and Mustard model. The main question raised by Table 11a and 11b (our preferred controls on state data from 1977-2006) is whether state trends are needed in the regression models. If not, there is evidence that RTC laws increase assault and larceny. If state trends are needed, some muddiness returns but aggravated assault appears to be increased by RTC laws, while declines in rape are marginally significant.

Table 10a: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data – With Preferred Controls, With Clustered Standard Errors, All Crimes, 1977-2000

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.45%	-3.51%	1.80%	3.16%	**11.80%**	-1.02%	**5.68%**
	4.54%	3.69%	3.91%	4.50%	**5.62%**	2.74%	**2.15%**
2. Spline model:	-0.48%	**-1.12%**	0.96%	-0.16%	1.13%	-0.54%	0.29%
	0.88%	**0.55%**	0.71%	0.76%	0.97%	0.40%	0.46%
3. Hybrid model:							
Postpassage dummy	-2.22%	1.21%	-2.74%	5.28%	**10.40%**	1.51%	<u>**6.40%**</u>
	4.67%	3.70%	3.83%	4.67%	**4.90%**	3.30%	<u>**1.94%**</u>
Trend effect	-0.31%	**-1.21%**	1.16%	-0.55%	0.37%	-0.65%	-0.18%
	0.98%	**0.57%**	0.77%	0.81%	0.91%	0.51%	0.46%

Table 10b: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data – With Preferred Controls, With Clustered Standard Errors and State Trends, All Crimes, 1977-2000

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.70%	**-4.46%**	-2.58%	2.89%	<u>6.20%</u>	0.72%	2.52%
	2.94%	**1.95%**	2.59%	4.31%	<u>3.43%</u>	2.33%	1.56%
2. Spline model:	0.09%	0.45%	<u>**3.32%**</u>	0.39%	-0.49%	-0.32%	0.65%
	1.31%	0.89%	<u>**0.92%**</u>	1.93%	1.29%	1.12%	1.08%
3. Hybrid model:							
Postpassage dummy	-0.83%	**-5.17%**	<u>-5.95%</u>	2.69%	<u>7.05%</u>	1.08%	2.04%
	3.04%	**1.98%**	<u>3.09%</u>	5.05%	<u>4.02%</u>	2.60%	2.01%
Trend effect	0.14%	0.78%	<u>**3.70%**</u>	0.22%	-0.94%	-0.39%	0.52%
	1.35%	0.90%	<u>**1.02%**</u>	2.09%	1.42%	1.19%	1.16%

Table 11a: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With Clustered Standard Errors, All Crimes, 1977-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.93%	-0.62%	5.05%	5.36%	7.03%	2.24%	**6.72%**
	3.94%	3.76%	3.71%	4.28%	6.05%	3.00%	**2.98%**
2. Spline model:	-0.16%	-0.44%	1.09%	0.64%	0.45%	0.00%	0.57%
	0.61%	0.54%	0.60%	0.75%	0.62%	0.39%	0.46%
3. Hybrid model:							
Postpassage dummy	-2.75%	1.71%	0.15%	3.09%	6.29%	2.82%	5.22%
	3.75%	3.52%	3.56%	4.74%	5.49%	3.21%	3.05%
Trend effect	-0.04%	-0.52%	1.09%	0.50%	0.17%	-0.13%	0.34%
	0.63%	0.56%	0.63%	0.83%	0.56%	0.43%	0.50%

Table 11b: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With State Trends and Clustered Standard Errors, All Crimes, 1977-2006

	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:							
Postpassage 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%

IX. Further Thoughts on Omitted Variable Bias

As discussed above, we believe it is likely that the NRC's estimations of the effects of RTC legislation are undermined by their failure to account for all plausible exogenous influences on crime within their model. In our attempt to make this endeavor more successful (at least to a degree) than the original Lott-Mustard model, we included additional explanatory variables such as the incarceration and police rates (among other factors). We recognize, however, that there are additional criminogenic influences for which we cannot fully control. In particular, we suspect that a major shortcoming of the NRC model (and by extension, the Lott-Mustard model) is its failure to account for the possible influence of the crack cocaine epidemic on crime.

Many scholars now suggest that rapid growth in the market for crack cocaine in the late 1980s and the early 1990s was likely one of the major influences on increasing crime rates (and violent crimes in particular) during this period (Levitt 2004). Moreover, the harmful criminogenic effect of crack was likely more acute in urban areas of states hesitant to adopt RTC laws. Meanwhile, many rural states adopted such laws during this era. If this was indeed the case, this divergence between states could account for much of the purported "crime-reducing" effects attributed by Lott and Mustard to gun laws (which were then supported by scholars such as James Q. Wilson). The regression analysis would then identify a relationship between rising crime and the failure to adopt RTC legislation, when the actual reason for this trend was the influence of crack (rather than the passage of the RTC law).

We now explore how results from our main models vary when we restrict the analysis to the time periods both before and after the peak of the American crack epidemic. According to Fryer et al. (2005), the crack problem throughout most of the country peaked at some point in the early 1990s. Coincidentally, the original Lott-Mustard period of analysis (1977-1992) contains years that likely represent the height of domestic crack use. With this in mind, we run our main regressions after breaking up our dataset into two periods: the original Lott-Mustard period of analysis (1977-1992) as well as the post-Lott-Mustard period (1993-2006). We first present the results for the era that includes the crack epidemic (1977-1992) on our preferred model. We run these regressions (with clustered standard errors) on state-level data, with and without state trends. These results are presented in Tables 12a and 12b. We then estimate the same models on the post-crack period (see Tables 13a and 13b).

Table 12a: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With Clustered Standard Errors, All Crimes, 1977-1992

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.69%	**-12.10%**	-6.55%	-4.85%	7.28%	-3.73%	0.12%
	3.81%	**3.41%**	4.66%	4.07%	4.73%	2.45%	1.52%
2. Spline model:	-0.88%	**-2.87%**	0.52%	**-2.28%**	0.51%	-0.34%	-0.10%
	1.44%	**0.80%**	1.70%	**0.72%**	1.13%	0.83%	0.33%
3. Hybrid model:							
Postpassage dummy	-2.32%	**-7.59%**	**-11.80%**	1.08%	9.07%	-4.37%	0.54%
	4.70%	**3.01%**	**5.64%**	5.32%	4.61%	3.87%	1.82%
Trend effect	-0.56%	**-1.83%**	2.13%	**-2.42%**	-0.73%	0.26%	-0.17%
	1.67%	**0.59%**	1.47%	**1.08%**	0.85%	0.97%	0.42%

Table 12b: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With State Trends and Clustered Standard Errors, All Crimes, 1977-1992

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-5.61%	-4.14%	-2.02%	-3.78%	-0.04%	-3.05%	1.28%
	3.57%	3.61%	3.70%	4.25%	3.84%	2.23%	1.96%
2. Spline model:	**-5.41%**	0.27%	-0.05%	-4.35%	-1.62%	-2.36%	0.37%
	2.45%	1.11%	1.17%	2.48%	2.20%	1.43%	1.15%
3. Hybrid model:							
Postpassage dummy	2.47%	-6.67%	-2.89%	3.08%	3.17%	0.18%	1.16%
	4.31%	3.52%	5.10%	6.91%	4.98%	4.26%	2.02%
Trend effect	**-6.01%**	1.88%	0.65%	-5.10%	-2.38%	-2.41%	0.09%
	2.51%	1.18%	1.84%	3.30%	2.64%	2.11%	1.26%

Note that the regression results in Table 12 from the initial Lott and Mustard 16 year time period (1977-1992) do suggest that rape, robbery, and aggravated assault are dampened by RTC laws if state trends are not needed and murder may have declined if state trends are needed. If we look at the following 14 year period from 1993 – 2006 in Table 13, however, the conclusion flips around: now there is evidence that all four violent crimes *rose* when states adopted RTC laws. This evidence supports the initial speculation by Ayres and Donohue (1999) that the Lott and Mustard finding was likely the result of impact of crack raising crime in non-RTC states.

Table 13a: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With Clustered Standard Errors, All Crimes, 1993-2006

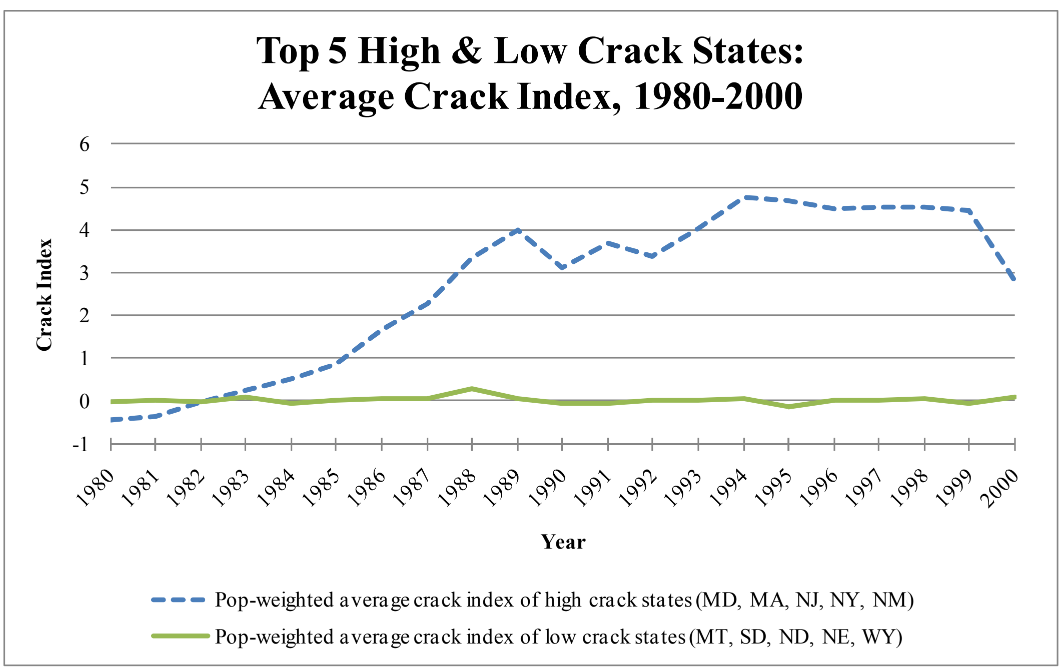
	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	3.12%	-3.47%	1.36%	3.64%	2.46%	3.58%	0.27%
	3.61%	2.47%	3.54%	4.89%	4.50%	2.57%	2.74%
2. Spline model:	1.11%	-0.21%	**1.91%**	**1.78%**	-0.30%	0.35%	0.08%
	0.63%	0.68%	**0.74%**	**0.87%**	0.80%	0.71%	0.55%
3. Hybrid model:							
Postpassage dummy	2.36%	-3.35%	0.03%	2.42%	2.70%	3.37%	0.22%
	3.82%	2.46%	4.05%	4.73%	4.33%	2.57%	2.76%
Trend effect	1.09%	-0.17%	**1.91%**	**1.75%**	-0.34%	0.31%	0.08%
	0.64%	0.67%	**0.76%**	**0.87%**	0.77%	0.70%	0.55%

Table 13b: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With State Trends and Clustered Standard Errors, All Crimes, 1993-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	3.12%	0.27%	2.38%	3.81%	2.83%	0.89%	0.33%
	3.62%	2.66%	2.59%	3.33%	3.39%	2.19%	1.83%
2. Spline model:	-1.99%	**2.61%**	**4.34%**	-0.17%	-5.53%	-0.71%	-1.49%
	2.00%	**1.16%**	**1.53%**	1.89%	2.77%	1.74%	1.31%
3. Hybrid model:							
Postpassage dummy	4.04%	-0.75%	0.79%	4.04%	5.12%	1.20%	0.93%
	3.87%	2.46%	2.40%	3.48%	3.43%	2.29%	1.98%
Trend effect	-2.44%	**2.69%**	**4.25%**	-0.62%	**-6.10%**	-0.84%	-1.59%
	2.10%	**1.14%**	**1.61%**	1.95%	**2.99%**	1.80%	1.42%

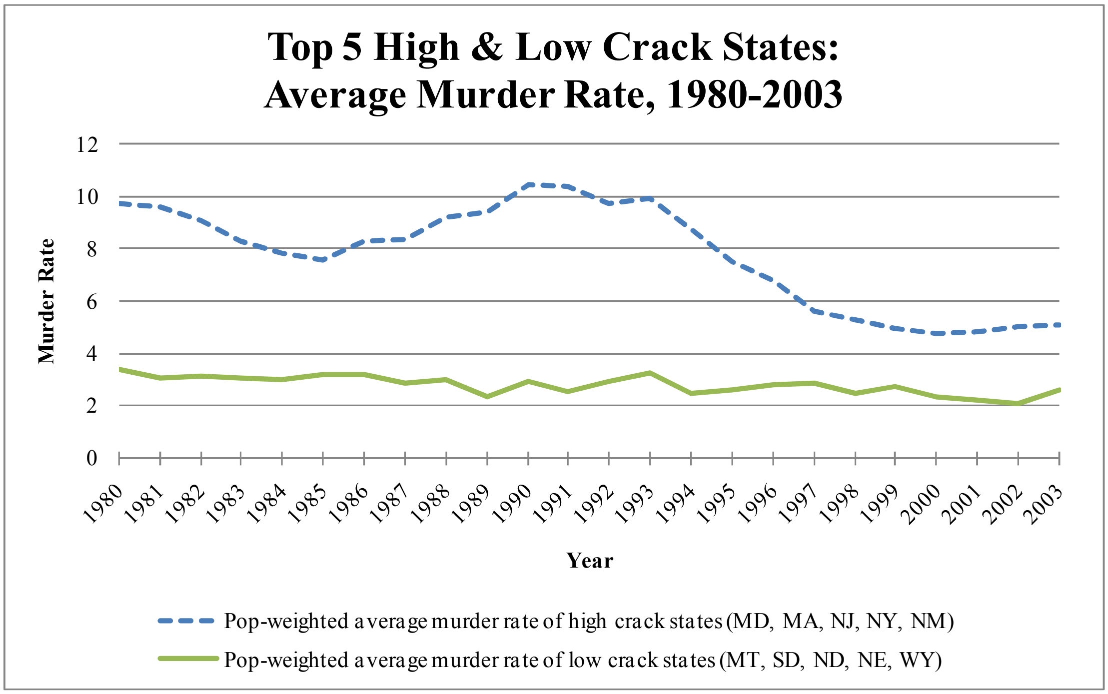
Figure 1 depicts a measure of crack prevalence for the period 1980 - 2000 in the five states deemed to have the highest crack problem as well as the five states with the least crack, according to the index created by Fryer et al. 2005. Figure 2 shows the murder rates over time for these two sets of states. What is clear is that crime rose in the high crack states when the crack index rises in the mid to late 1980s but that the crack index does not turn down in those states at the time crime started to fall. Apparently, the rise of the crack market triggered a great deal of violence but once the market was stabilized, the same level of crack consumption could be maintained while the violence ebbed.

Figure 1: The Prevalence of Crack in the 5 Most and 5 Least Affected States



Source: Authors' calculations based on the crack index of Fryer et al. (2005).

Figure 2



Of course, omitting an appropriate control for the criminogenic influence of crack is problematic if the high-crack states tend not to adopt RTC laws and the low-crack states tend to be adopters. This is in fact the case: all of the five "high-crack" states are non-RTC states during this period, whereas four of the five "low-crack" states are RTC states (all four adopted an RTC law by 1994).[18] The only exception is Nebraska, the state with the fifth lowest average crack index that did not adopt an RTC law until 2007, which is out of the scope of our current analyses.[19]

[18] New Mexico, one of the five high crack states, adopted its RTC law in 2003. Wyoming and Montana adopted their RTC laws in 1994 and 1991, respectively. North Dakota and South Dakota adopted their RTC laws prior to the start of our data set (pre-1977), although the precise dates are contested (see Lott and Mustard 1997, and Moody and Marvell 2008).

[19] In fact, out of the ten states with the lowest crack cocaine index, seven adopted an RTC law by 1994. The exceptions are Nebraska, Minnesota (2003), and Iowa (no RTC law).

Table 14: Population-weighted Statistics of RTC-Adopting States between 1977 and 1990[20]

State	Year of RTC Law Adoption	Murder Rate	Crack Index
Indiana	1980	6.53	0.17
Maine	1985	2.53	-0.04
North Dakota	1985	1.29	0.01
South Dakota	1986	2.10	-0.03
Florida	1987	11.73	0.67
Virginia	1988	7.90	0.65
Georgia	1989	12.28	0.92
Pennsylvania	1989	5.73	0.65
West Virginia	1989	5.65	0.32
Idaho	1990	3.56	0.30
Mississippi	1990	11.65	0.25
Oregon	1990	4.85	0.76

Moreover, as Table 14 reveals, the 12 states that adopted RTC laws during the initial Lott and Mustard period (1977-1992) had crack index levels substantially below the level of the five high-crack states shown in Figures 1 and 2. Specifically, none of the RTC adopters shown in Table 14 has an average crack index that even reaches one, while Figure 2 reveals that the high-crack states had a crack index level in the neighborhood of four or five. In other words, over the initial Lott and Mustard data (ending in 1992), the criminogenic influence of crack made RTC laws look good since crack was raising crime in non-RTC states. In the later period, crime was falling sharply in the high-crack states making RTC laws look bad. Therefore, the effects estimated over this entire period will necessarily water down the initial Lott and Mustard results. The hope is that estimating the effect over the entire period will wash out the impact of the omitted variable bias generated by the lack of an adequate control for the criminogenic influence of crack.

[20] The crack index data comes from Fryer et al (2005), which constructs the index based on several indirect proxies for crack use, including cocaine arrests, cocaine-related emergency room visits, cocaine-induced drug deaths, crack mentions in newspapers, and DEA drug busts. The paper does suggest that these values can be negative. The state with the lowest mean value of the crack index over our data period is Maine (-0.04) and the state with the highest mean value is New York (1.15). (The paper does suggest that the crack index values can be negative.)

X. Conclusion

In this paper, we have explored the NRC committee's 2005 report detailing the impact of right-to-carry gun laws on crime. Using the committee's regression models as a starting point for our analysis, we present further evidence demonstrating the sensitivity of these models to data and modeling changes. In the process, we also highlight some issues that should be considered when evaluating the NRC report.

Data reliability is one concern in the NRC study is. We corrected several coding errors in the data that were provided to us by the NRC, which the Committee had taken from Lott's webpage. Accurate data is essential to making precise causal inferences about the effects of policy and legislation—and this issue becomes particularly important when we are considering a topic as controversial as gun control. We attempted to mitigate any uncertainty over data reliability by re-collecting the data and constructing our own data set. However, when attempting to replicate the NRC specifications—on both the NRC and our own newly constructed data sets—we consistently obtained point estimates that differed substantially from those published by the committee.

Thus, an important lesson for both producers and consumers of econometric evaluations of law and policy is to understand how easy it is to get things wrong. In this case, Lott's data set had errors in it, which then got transmitted to the NRC Committee when they sought to replicate Lott and Mustard's regressions. Then the Committee published tables that could not be replicated, and made at least James Q. Wilson think (incorrectly it turns out – see our Tables 2a through 2d) that running Lott and Mustard regressions on both data periods (through 1992 and through 2000) would generate consistent statistically significant evidence that RTC laws reduce murder.

This episode suggests to us the value of posting data and do-files on the web that generate published econometric results. This exercise can both help to uncover errors prior to publication, and then assists other researchers in the process of replication, thereby aiding the process of more quickly identifying and correcting any errors that can so easily mar econometric estimates.

A second lesson is that there are still disputes over econometric methodology about which top researchers disagree, which can powerfully influence the results of econometric estimates. Thus, we see that the NRC Committee report suggested that clustering the standard errors in the panel data models used to estimate the impact of RTC laws is not necessary, even though this has now become a common practice in the wake of the Bertrand, Duflo, and Mullainathan (2004) as a means to account for serial correlation in panel data. Given that our placebo tests showed that standard errors are greatly understated without clustering, we believe strongly that clustered standard errors are necessary for both county-level and state-level analyses of guns and crime. Otherwise, significance is severely exaggerated. Other issues—such as the inclusion of state-specific linear trends, the danger of omitted variable bias, and the choice of county over state-level crime data, all of which the NRC neglected—also warrant closer examination.

A third lesson is how easy it is to be lulled by superficially supportive empirical results, particularly if they comport with one's own intuitions or prior preferences. How else can one explain the decision of James Q. Wilson to buck the conclusions of the other 15 members of the Committee (which included many top econometricians), and conclude that RTC laws reduce murder? Wilson looked at the data from the perspective of someone who was confident that RTC laws reduced crime, when that was the proposition to be tested.

Unfortunately, while econometrics is intended to restrain the excesses of theory or ideology, it often seems to be the case that theory and ideology can unhinge all critical capacity when given expression through ostensibly congenial econometric findings. What was likely beguiling to Wilson was the thought that hard scientific evidence supported a belief that he had come to hold about the value of guns in fighting crime (as we saw, he had taken a strong position to that effect a number of years earlier). Once one has staked out a strong claim, it is easy to overlook obvious infirmities in one's own position. Importantly, the potential flaws in the Lott and Mustard were not all arcane (although many were, such as the need for clustering and the issue about whether one should or should not control for state trends). Everyone in social science knows about omitted variable bias, and a major factor influencing the pattern of crime in the US in the last couple of decades was the increase in the extent of incarceration–as Wilson clearly knew. Yet Lott's model had no control for incarceration![21] On that basis alone, one should have hesitated before adopting the more guns, less crime hypothesis so uncritically.

Given Wilson's own dissent highlighting that the no "controls model" was inadequate because one needed to control for the other influences on crime, one would have thought Wilson would have been reluctant to endorse the Lott and Mustard specification. The answer may simply be that it is easy to be disarmed by sophisticated econometric studies that claim to control for all relevant variables (and certainly don't highlight their deficiencies) when they comport with one's prior beliefs.

If a study emerges with a seemingly impressive regression analysis, those who are inclined to believe the results tend to accept it much more readily than they would accept a study of similar quality reaching a less congenial conclusion. Wilson's oversight on the huge omitted variable problem led him to champion a position for which there was no credible scientific support. As a result, Lott, with no credible support for his view that right to carry laws reduce murder, is still parading around the country saying that one of the most eminent criminologists of our time supports his position. Needless to say, many members of the public and political figures sympathetic to the NRA and gun manufacturers are quite eager to trumpet such conclusions.

Fourth, while much of the work of applied econometricians is exactly of the sort that was set forth as evidence of the impact of RTC laws in the NRC report, the Committee found this evidence inadequate to reach a conclusion, doubtless because the results seemed too dependent on different modeling choices. But Horowitz's appendix is even more nihilistic, essentially rejecting all applied econometric work. In the NRC (2005) report Horowitz writes:

> "the choice of explanatory variables matters. [T]here is and can be no empirical test for whether a proposed set of explanatory variables is correct. There is little prospect

[21] The Lott and Mustard model omitted a control for the incarceration rate (which is indicated implicitly–though not explicitly—in the notes to each table of the NRC report that indicated the controls that were included in each specification).

> for achieving an empirically supportable agreement on the right set of variables. For this reason, in addition to the goodness-of-fit problems…, it is unlikely that there can be an empirically based resolution of the question of whether Lott has reached the correct conclusions about the effects of right-to-carry laws on crime." (p. 304)

Of course, if there can be no empirically based resolution of this question, it means that short of doing an experiment in which RTC laws were randomly assigned to various states, there will be no way to learn the impact of these laws. The econometrics community needs to think deeply about what the NRC report and the Horowitz appendix imply for modern applied econometrics.

Finally, despite our belief that the NRC's analysis was imperfect in certain ways, we agree with the committee's cautious final judgment regarding the effects of shall-issue RTC laws: "with the current evidence it is not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates." Our results here further underscore the sensitivity of guns-crime estimates to various modeling decisions. If one had to make judgments based on panel data models of the type presented in the NRC report, one would have to conclude that RTC laws likely increase the rate of aggravated assault. Further research will be needed to see if this conclusion survives as more data and better methodologies are employed to estimate the impact of RTC laws on crime.

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Appendix A: Early Academic Studies Reviewing the "More Guns, Less Crimes" Hypothesis

Studies supporting Lott (using data through 1992)
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Plassmann, F. and T.N. Tideman. 2001. Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only A Count Analysis Can Say. *Journal of Law and Economics* 44: 771-798.

Studies questioning Lott (using data through 1992)
Ayres, I. and J, Donohue, III. 1999. Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy. *American Law and Economics Review* 1: 436-70.
Black, D.A. and D.S. Nagin. 1998. Do Right-to-Carry Laws Deter Violent Crime? *Journal of Legal Studies* 27: 209-219.
Dezhbakhsh, H. and P.H. Rubin. 1998. Lives Saved or Lives Lost—The Effects of Concealed-Handgun Laws on Crime. *American Economic Review* 88: 468-474.
Ludwig, J. 1998. Concealed Gun Carrying Laws and Violent Crime: Evidence from State Panel Data. *International Review of Law and Economics*: 239-54.

Appendix B: Using Placebo Laws to Test the Impact of Clustering in the State Data

Using state-level data, we again conduct our experiment with placebo laws to examine the effects of clustering the standard errors. As seen in Tables 1-4 of Appendix B, we find results similar to those generated on our county data: without clustering, the Type 1 error rates are often an order of magnitude too high or worse for our murder and robbery regressions (see Tables B1 and B3). In fact, even *with* clustered standard errors (Tables B2 and B4), the rejection of the null hypothesis (that RTC laws have no significant impact on crime) occurs at a relatively high rate. This finding suggests that at the very least, we should include clustered standard errors to avoid unreasonably high numbers of significant estimates.

Table B1: Hybrid Model - Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 State-Level Data – Lott and Mustard Controls, Without Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable	Trend Variable
1. All 50 States + D.C.	Murder	47.1%	67.2%
	Robbery	46.0%	61.7%
2. Exact 32 States	Murder	48.5%	57.3%
	Robbery	51.2%	71.1%
3. Random 32 States	Murder	49.3%	64.2%
	Robbery	50.0%	66.0%

Table B2: Hybrid Model - Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 State-Level Data – Lott and Mustard Controls, With Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable	Trend Variable
1. All 50 States + D.C.	Murder	18.5%	22.6%
	Robbery	12.5%	15.4%
2. Exact 32 States	Murder	17.1%	19.4%
	Robbery	15.2%	20.3%
3. Random 32 States	Murder	22.0%	22.7%
	Robbery	16.3%	18.2%

Table B 3: Dummy Variable Model – Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 State-Level Data – Lott and Mustard Controls, Without Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable
1. All 50 States + D.C.	Murder	44.3%
	Robbery	46.7%
2. Exact 32 States	Murder	50.3%
	Robbery	49.4%
3. Random 32 States	Murder	51.9%
	Robbery	50.8%

Table B 4: Dummy Variable Model – Percentage of Significant Estimates (at the 5% level) – Using Donohue 2009 State-Level Data – Lott and Mustard Controls, With Clustered Standard Errors, 1977-2006 (Without 1993 Data)

		Dummy Variable
1. All 50 States + D.C.	Murder	18.0%
	Robbery	14.1%
2. Exact 32 States	Murder	16.0%
	Robbery	16.4%
3. Random 32 States	Murder	22.7%
	Robbery	14.3%

Appendix C – Summarizing Estimated Effects of RTC Laws Using Different Models, State v. County Data, and Different Time Periods

This appendix provides graphical depictions of 16 different estimates of the impact of RTC laws for the dummy and spline models. For example, Figure C1 shows estimates of the impact of RTC laws on murder using the dummy model, designed to capture the average effect of RTC laws during the post-passage period. The first bar in each of the eight grouping corresponds to county-level estimates; the second bar corresponds to state-level estimates, for a total of 16 estimates per figure. The value of the figures is that it permits quick visual observation of whether any estimates are statistically significant. Note, for example, that *none* of the estimates in either Figure C1 or Figure C2 is significant at even the .10 threshold. This sharp contrast from the conclusion drawn by James Q. Wilson on the NRC panel is in part driven by the fact that all of the estimates in this appendix come from regressions in which we adjusted the standard errors by clustering.

In contrast to the wholly insignificant estimates for murder, the estimates of the impact of RTC laws on aggravated assault in Figure C6 are generally significant as indicated by the shading of the columns, where again no shading indicates insignificance, and the shading darkens as significance increases (from a light grey indicating significance at the .10 level, slightly darker indicating significance at the .05 level, and black indicating significance at the .01 level). Note that the overall impression from Figure C6 is that RTC laws *increase* aggravated assaualt. Even in Figure C6, though, one can see that some of the estimates differ between county and state level data and tend to be strongest in state data controlling for state trends. Figure C5, which provides estimates of the effect of RTC laws on aggravated assault using the dummy model (rather than the spline model of Figure C6), reveals that the conclusion that RTC laws *increase* aggravated assault is model dependent: if the dummy model is superior, then if we confine our attention to the complete 1977-2006 data set, the conclusion that RTC laws increase aggravated assault only holds in the Lott and Mustard county data model.

Figure C1

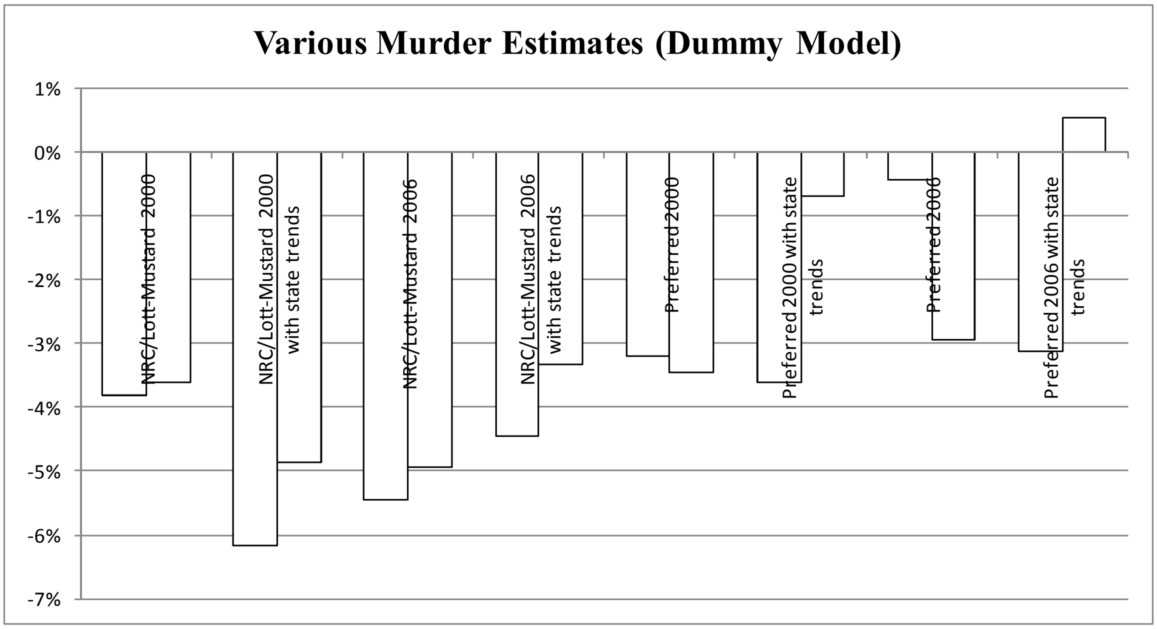


Figure C2

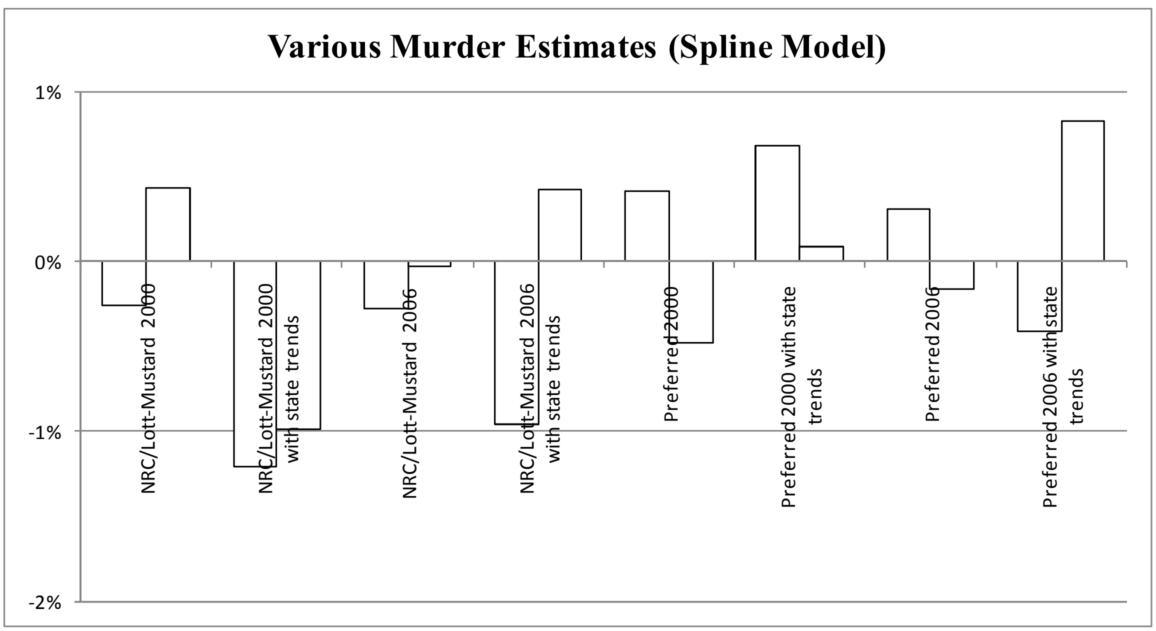


Figure C3

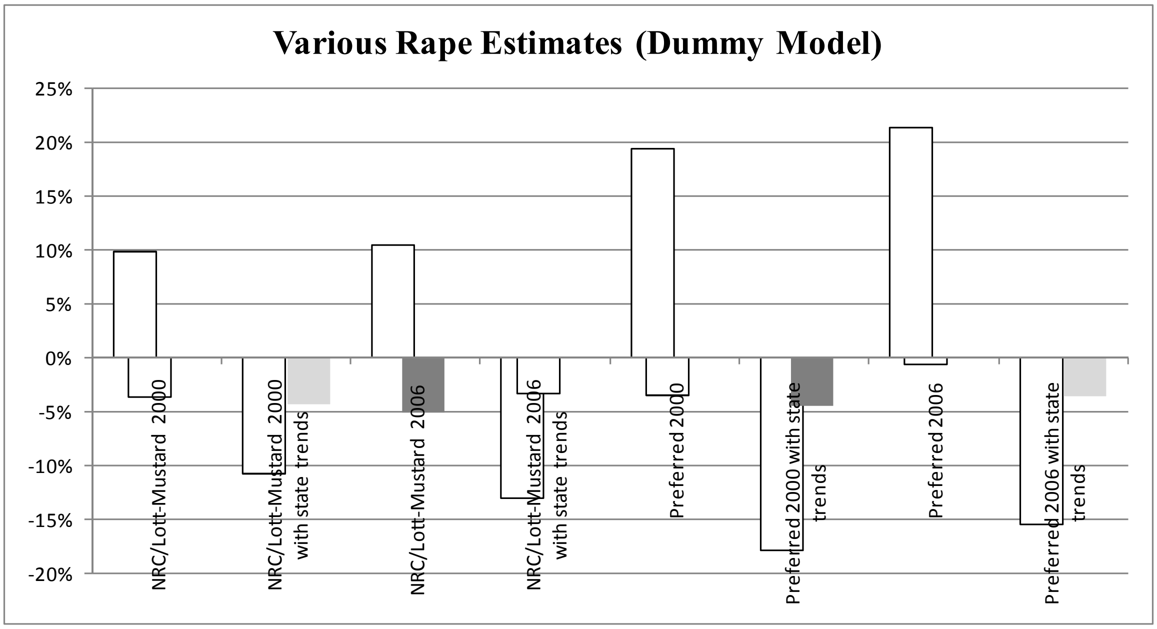


Figure C4

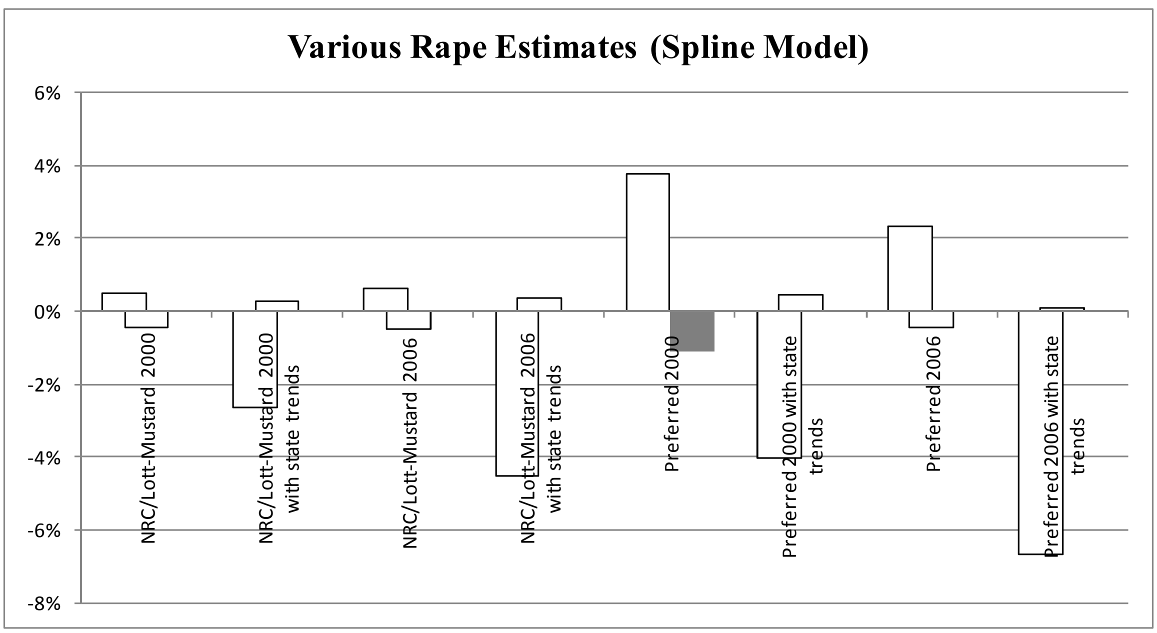


Figure C5

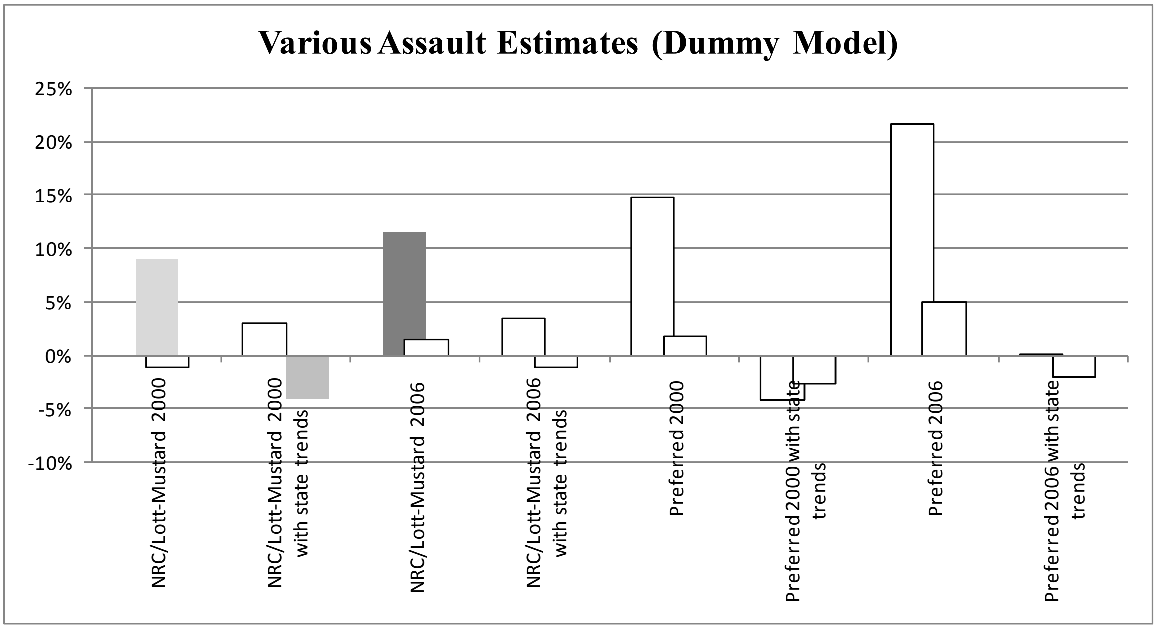


Figure C6

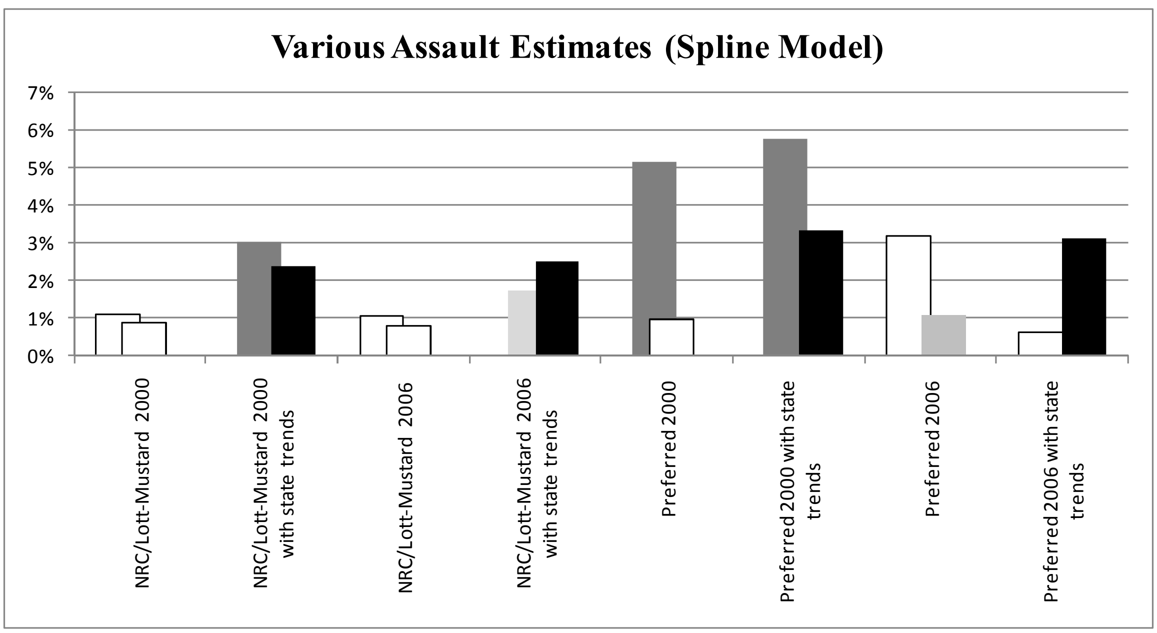


Figure C7

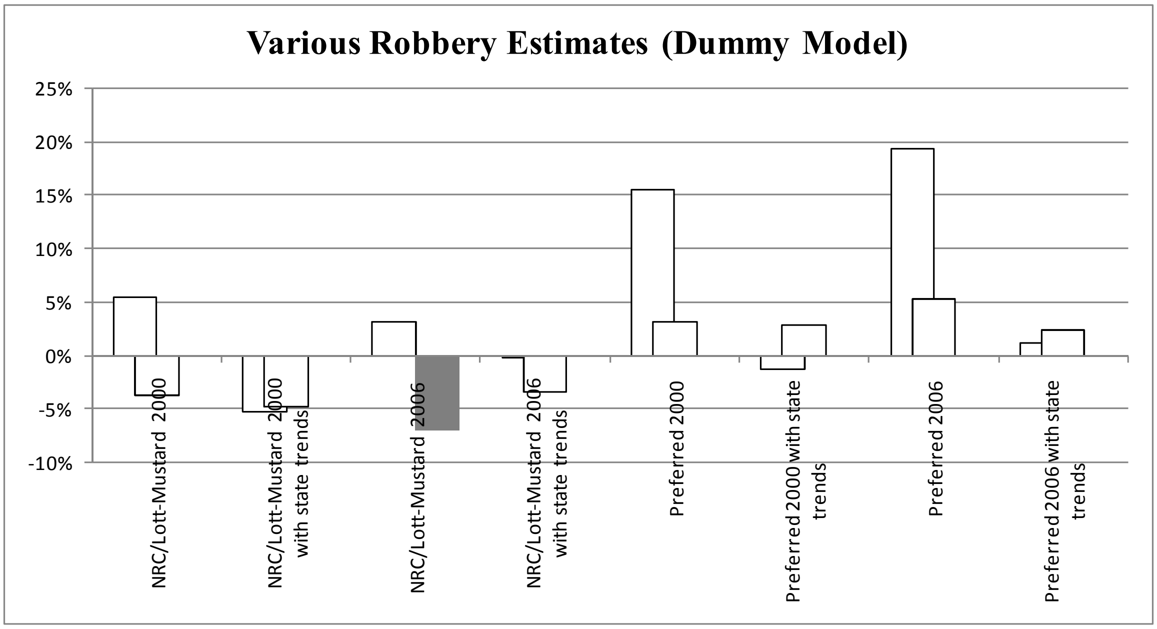


Figure C8

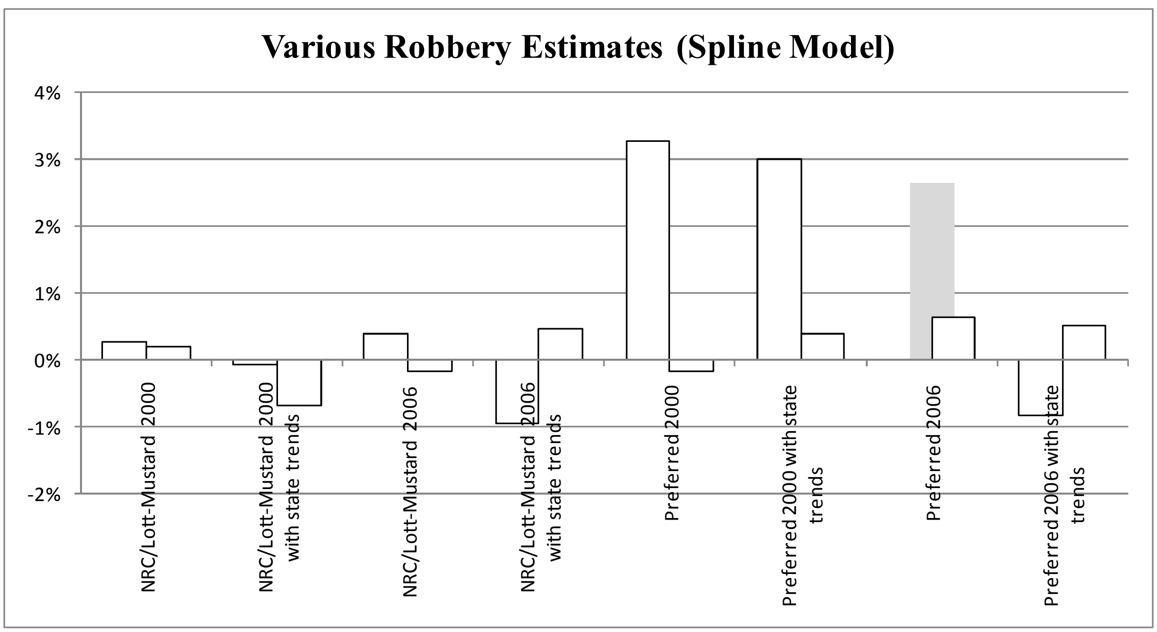


Figure C9

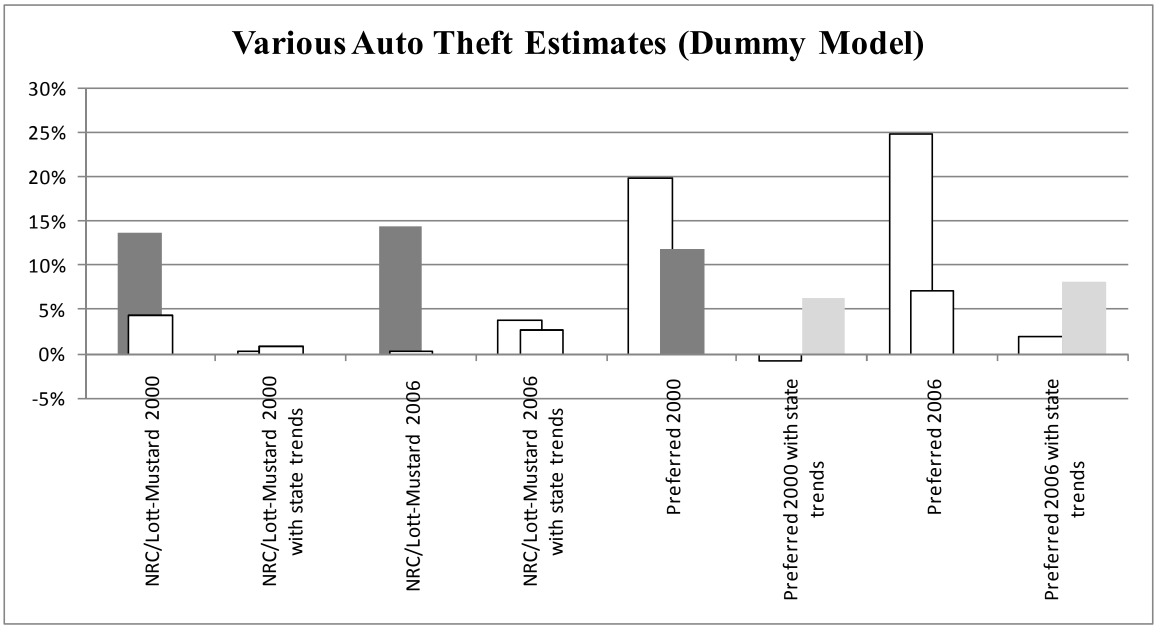


Figure C10

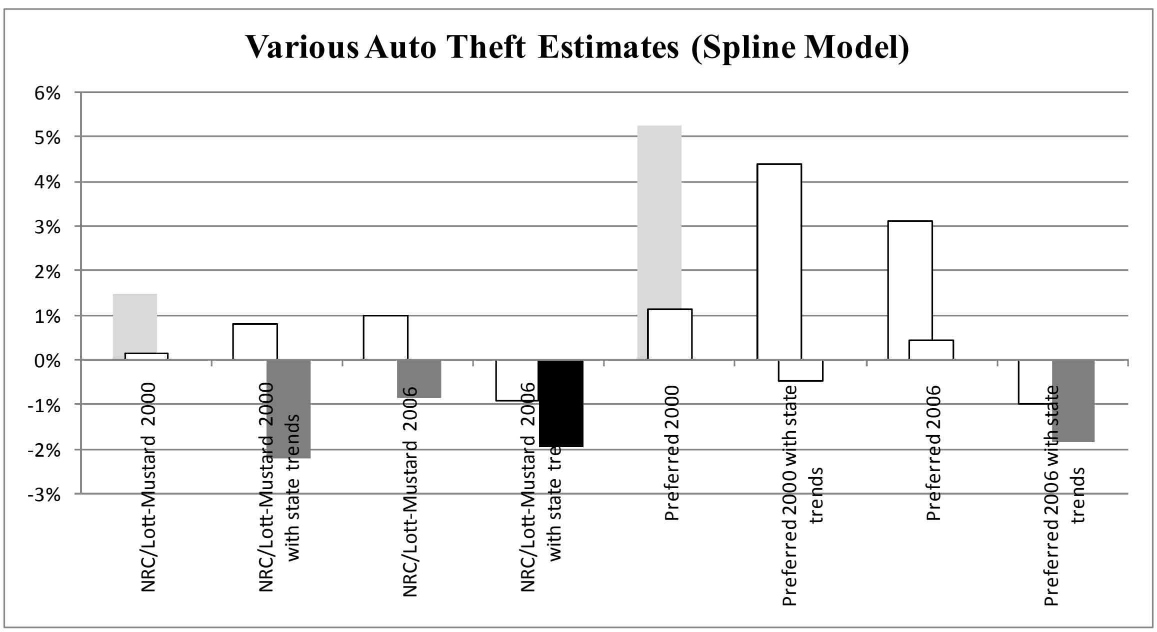


Figure C11

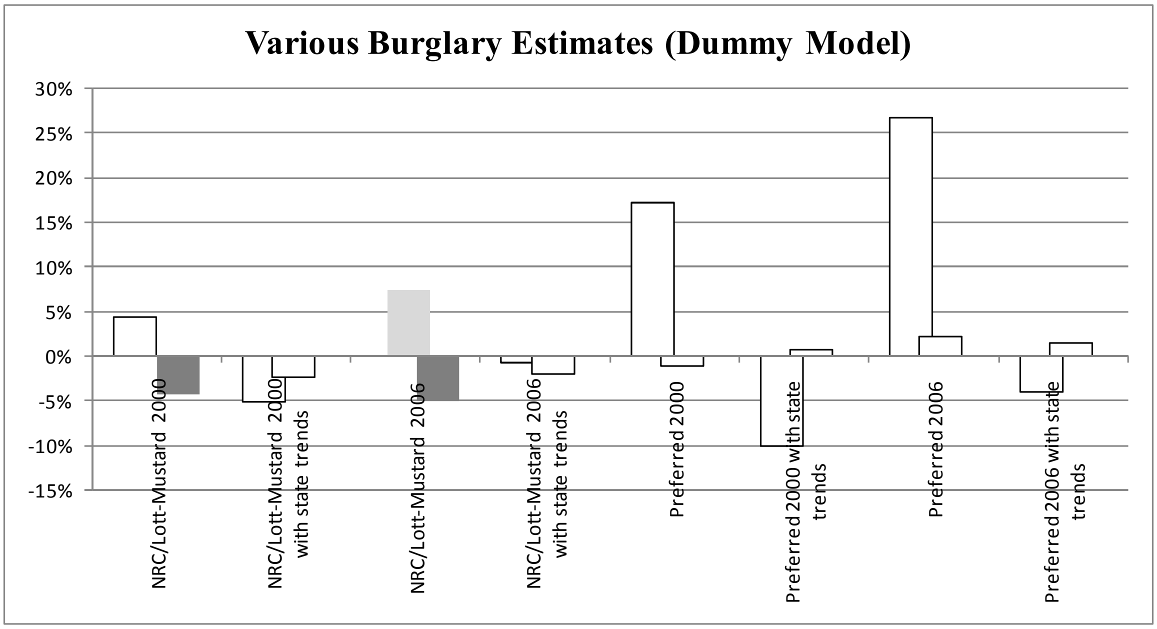


Figure C12

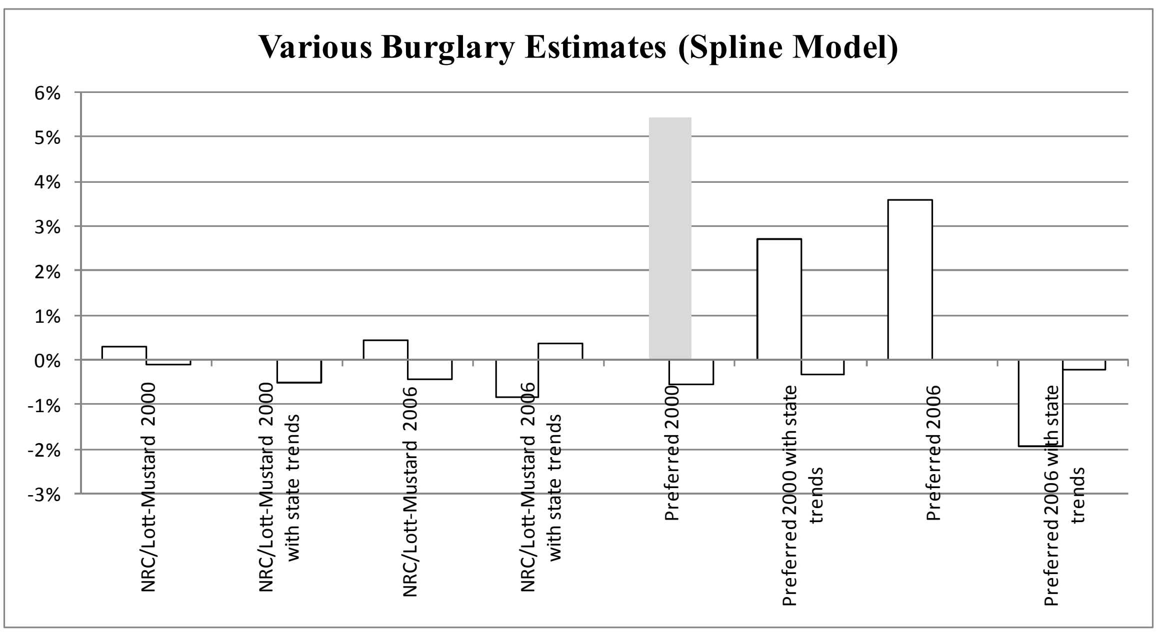


Figure C13

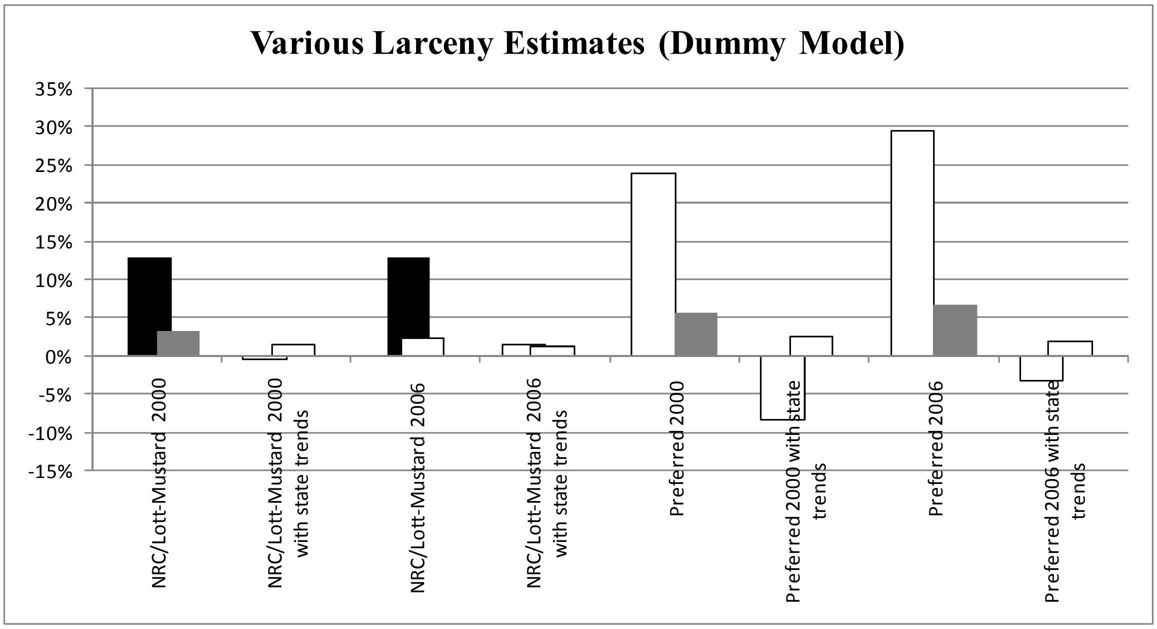
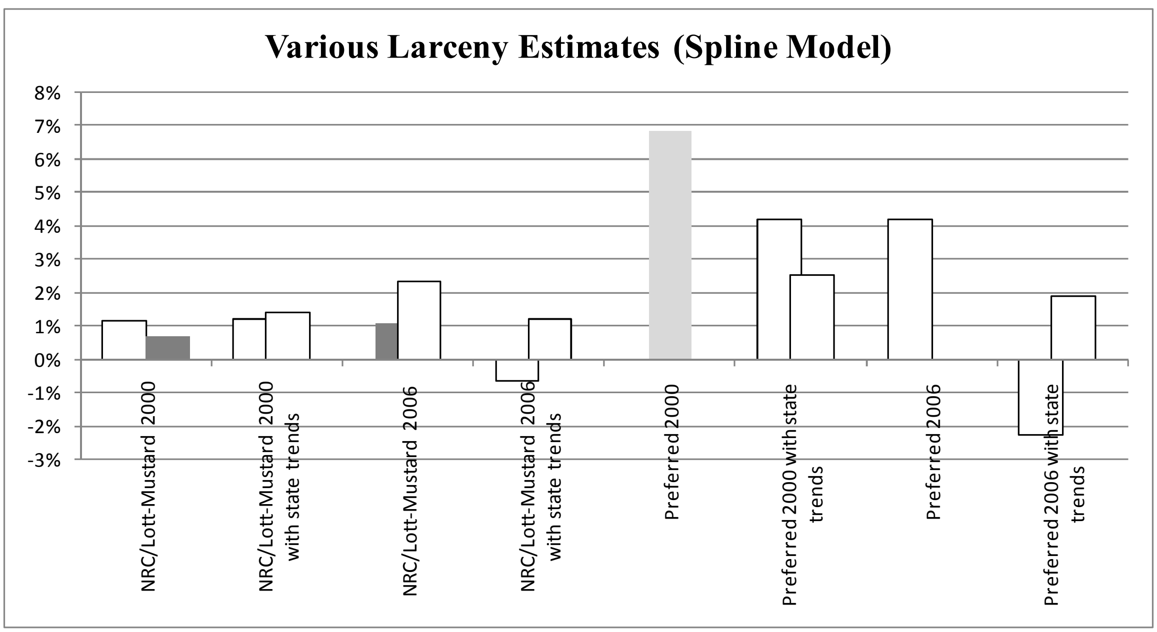


Figure C14



In Figure C14, the state-level estimates of the preferred specifications (without state trends) through 2000 and 2006 are essentially zero (no impact) so only the county-level estimates show up in the graph.

Appendix D – Trimming the Sample to Address Questions of Model Fit

Given our concerns about how well the guns-crime econometric models fit **all** US states, we decided to examine the residuals from various regressions models. For example, one potentially important issue is whether one should include linear state trends in our models. To further explore this issue, we examined the variance of the residuals for the aggravated assault regression estimates using our preferred models on state data for the period through 2006—both with and without state trends.[22] In particular, we found that the residual variance was high for smaller states, even when we do not weight our regressions by population.[23]

We explored how these "high residual-variance" states (defined from the aggravated assault regressions on our preferred model through 2006) might be influencing the results. We estimate our preferred model (both with and without state trends) after removing the 10 percent of states with the highest residual variance. This step is also repeated after removing the highest 20% of states in terms of residual variance. Our original full-sample results for our preferred specification (which includes clustered standard errors, and is run over the entire time period) are shown in Table 11a and 11b (without and with state trends, respectively). The results from our two trimmed set of states are presented below. Tables D1 and D2 should be compared to Table 11a (no state trends) and Tables D3 and D4 should be compared to Table 11b (adding in state trends).

Removing high residual-variance states (based on the aggravated assault regressions) has little impact on the story told in Table 11a (no state trends): there was no hint that RTC laws reduce crime in Table 11a and this message comes through again in Tables D1 and D2. All three of these tables show at least some evidence that RTC laws *increase* aggravated assault. Removing the high residual-variance states from the models with state trends does nothing to shake the Table 11b finding that RTC laws *increase* aggravated assault. The somewhat mixed results for auto theft seen in Table 11b also remain in Table D3 and D4.

[22] Since our most robust results across the specifications in this paper were for aggravated assault, we focused specifically on the residuals obtained using assault rate as the dependent variable.

[23] We removed the population weight for this exercise because it is likely that when regressions are weighted by population, the regression model will naturally make high-population states fit the data better. As a result, we expect that residuals for smaller states will be higher. We find, however, that the results are qualitatively similar even when we obtain the residuals from regressions that include the population-weighting scheme.

Table D1: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With Clustered Standard Errors, All Crimes, 1977-2006, Dropping States with Highest Residual Variance (Top 10%: MT, ME, WV, NH, TN)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-3.53%	-0.98%	4.33%	5.04%	6.80%	1.38%	5.75%
	4.02%	3.95%	3.15%	4.41%	6.27%	3.05%	2.96%
2. Spline model:	-0.13%	-0.50%	**1.16%**	0.66%	0.57%	0.01%	0.57%
	0.62%	0.56%	**0.57%**	0.77%	0.63%	0.39%	0.47%
3. Hybrid model:							
Postpassage dummy	-3.69%	1.65%	-1.21%	2.53%	5.26%	1.69%	3.94%
	3.80%	3.69%	3.22%	4.98%	5.80%	3.30%	2.98%
Trend effect	0.04%	-0.58%	1.21%	0.55%	0.34%	-0.07%	0.40%
	0.64%	0.58%	0.60%	0.86%	0.58%	0.43%	0.50%

Of the states dropped from Tables D1, the following four states adopted RTC laws during the 1977-2006 period (with date of adoption in parentheses): Montana (1991), Maine (1985), West Virginia (1989), and Tennessee (1994).

Table D2: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With Clustered Standard Errors, All Crimes, 1977-2006, Dropping States with Highest Residual Variance (Top 20%: MT, ME, WV, NH, TN, NE, VT, HI, OH, KY)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-4.99%	-0.28%	3.94%	5.80%	8.13%	2.86%	**6.75%**
	4.23%	4.28%	2.40%	4.97%	6.60%	3.20%	**3.23%**
2. Spline model:	-0.16%	-0.50%	0.84%	0.90%	0.71%	0.29%	0.71%
	0.66%	0.59%	0.47%	0.83%	0.70%	0.37%	0.50%
3. Hybrid model:							
Postpassage dummy	-5.38%	2.53%	0.15%	2.09%	6.16%	1.91%	4.39%
	3.93%	3.95%	3.05%	5.54%	6.13%	3.64%	3.37%
Trend effect	0.09%	-0.61%	0.83%	0.81%	0.43%	0.21%	0.52%
	0.68%	0.61%	0.54%	0.92%	0.66%	0.43%	0.55%

Of the *additional* states dropped from Table D2, the following four states adopted RTC laws during the 1977-2006 period (with date of adoption in parentheses): Ohio (2004), Kentucky (1996), Indiana (1980), and Oklahoma (1995).[24]

[24] In implementing our protocol of dropping high residual-variance states, we examined the residuals of the dummy and spline models separately to identify the high-variance states. While they match across models for

Table D3: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With State Trends and Clustered Standard Errors, All Crimes, 1977-2006, Dropping States with Highest Residual Variance (Top 10%: MT, NH, VT, WV, KY)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	1.17%	-3.56%	-0.13%	2.28%	**7.82%**	1.31%	1.77%
	2.95%	2.16%	2.82%	3.75%	**3.26%**	2.03%	1.66%
2. Spline model:	0.80%	0.15%	**2.83%**	0.32%	**-2.01%**	-0.31%	-0.21%
	0.91%	0.81%	**0.82%**	1.37%	**0.83%**	0.91%	0.79%
3. Hybrid model:							
Postpassage dummy	0.73%	-3.71%	-1.77%	2.14%	**9.13%**	1.51%	1.93%
	3.12%	2.32%	2.80%	4.04%	**3.23%**	2.31%	1.84%
Trend effect	0.77%	0.27%	**2.89%**	0.25%	**-2.29%**	-0.35%	-0.27%
	0.95%	0.83%	**0.84%**	1.42%	**0.83%**	0.95%	0.83%

Results from Table D3 come from dropping similar RTC states to Table D1, although Kentucky (1996) is dropped rather than Tennessee, and New Hampshire (1959) is dropped rather than Maine.[25]

Table D4: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data –With Preferred Controls, With State Trends and Clustered Standard Errors, All Crimes, 1977-2006, Dropping States with Highest Residual Variance (Top 20%: MT, NH, VT, WV, KY, NE, NV, SD, ND, DE, IN)

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	2.09%	-2.88%	-1.35%	4.63%	**8.94%**	1.42%	2.41%
	2.97%	2.29%	2.78%	3.44%	**3.18%**	2.14%	1.68%
2. Spline model:	0.92%	0.25%	**2.42%**	0.63%	**-2.11%**	-0.43%	-0.12%
	0.97%	0.83%	**0.80%**	1.44%	**0.88%**	0.99%	0.83%
3. Hybrid model:							
Postpassage dummy	1.69%	-3.03%	-2.50%	4.39%	**10.00%**	1.63%	2.50%
	3.09%	2.40%	2.83%	3.71%	**3.18%**	2.40%	1.87%
Trend effect	0.88%	0.32%	**2.48%**	0.53%	**-2.35%**	-0.47%	-0.18%
	1.01%	0.84%	**0.81%**	1.50%	**0.87%**	1.02%	0.87%

three of the four tables, in the case of Table D4, the ordinal rank of the states in terms of residual variance were slightly different for the dummy versus the spline model. For this table, Indiana had the 9th highest residual variance when looking at the dummy model results, while North Dakota had the 11th highest variance. For the spline results, the residual variance ranks of these two states were reversed. Thus, for this table, we dropped both states to estimate our regressions.

[25] The dropped states are slightly different between Tables D1 and D3, as well as between Tables D2 and D4, because the state ranks based on residual variances differed when the models were run with and without state trends.

Finally, in addition to the five RTC states that were dropped in Table D3, Table D4 dropped the following four RTC states: Nevada (1995), South Dakota (1986), North Dakota (1985), and Indiana (1980).

Appendix E – Panel Data Models Over the Full Period With No Covariates

The NRC panel sought to underscore the importance of finding the correct set of covariates by presenting panel data estimates of the impact of RTC without covariates but including county and year fixed effects. For completeness, this Appendix presents these same estimates for the preferred models (with and without state trends) on both county and state data for the period from 1977-2006.

If one compares the results from these four tables with no controls with the analogous tables using the preferred model for the same time period, one sees some interesting patterns. For example, if we compare the county results without state trends from both our preferred specification (Table 7a) and the no-controls specification (Table E1), we see that the results are quite similar in terms of magnitude and direction, although adding in our suggested covariates seems to both dampen the coefficients and reduce the significance. The basic story from our analysis is again strengthened: there seems to be virtually no effect of RTC laws on murder, while if there is *any* RTC effect on other crimes generally, it is a crime-*increasing* effect. The results are slightly less similar when we compare the results from the models that include state trends (Tables 7b and E2). While we see that estimates are similar for murder, rape, robbery, and auto theft, the estimates for assault, burglary, and larceny change in either magnitude or direction (or both) when adding controlling factors to the model. In general, though, we only see decreases when adding state trends to either specification, and even then, the results are much too imprecise to make causal inferences.

When we shift to a comparison of the state-level results, we again see similarities between the preferred and no-controls specifications. When looking at the results without state trends, we see that the estimates are very similar in terms of direction, although the no-controls estimates are often larger in magnitude and more statistically significant. When doing a similar comparison of the specifications that now add in state trends, we also see similar results for nearly all crimes. The exception is aggravated assault, for which we see that our preferred specification produces more negative estimates for the dummy model (although this result is not particularly precise). Again, when the comparison is taken as a whole, support is lacking for the view that RTC laws lead to reductions in crime.

Table E1: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – No Controls, With Clustered Standard Errors, All Crimes, 1977-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.55%	33.10%	27.30%	25.50%	33.50%	35.90%	38.00%
	8.30%	22.60%	18.90%	14.60%	21.50%	22.00%	25.50%
2. Spline model:	0.35%	3.35%	3.20%	**2.86%**	3.42%	3.85%	4.27%
	0.76%	1.94%	1.66%	**1.36%**	2.01%	2.00%	2.29%
3. Hybrid model:							
Postpassage dummy	-3.48%	21.40%	14.30%	14.30%	21.40%	21.50%	21.30%
	8.07%	18.70%	16.90%	12.70%	17.60%	18.90%	21.60%
Trend effect	0.54%	2.17%	2.41%	2.07%	2.24%	2.66%	3.09%
	0.72%	1.25%	1.27%	1.08%	1.48%	1.54%	1.69%

Table E2: The Estimated Impact of RTC Laws – Using Donohue 2009 County-Level Data – No Controls, With State Trends and Clustered Standard Errors, All Crimes, 1977-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-2.80%	-13.10%	5.02%	3.10%	5.58%	1.50%	2.98%
	5.03%	10.60%	9.31%	7.71%	9.47%	10.50%	11.70%
2. Spline model:	-0.54%	-4.74%	1.95%	-0.37%	-0.14%	-0.78%	-0.80%
	1.23%	4.06%	2.30%	2.33%	2.52%	2.45%	2.61%
3. Hybrid model:							
Postpassage dummy	-2.52%	-10.50%	3.94%	3.35%	5.73%	1.97%	3.48%
	5.22%	10.10%	10.20%	8.27%	10.20%	11.40%	12.80%
Trend effect	-0.48%	-4.52%	1.87%	-0.44%	-0.26%	-0.82%	-0.87%
	1.27%	4.07%	2.42%	2.42%	2.63%	2.61%	2.80%

Table E3: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data – No Controls, With Clustered Standard Errors, All Crimes, 1977-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-1.79%	8.33%	**11.70%**	**20.00%**	**24.70%**	**18.30%**	**16.60%**
	7.54%	8.22%	**4.62%**	**7.90%**	**11.60%**	**6.69%**	**4.04%**
2. Spline model:	0.08%	0.78%	**1.47%**	**1.98%**	2.03%	**1.73%**	**1.63%**
	0.88%	0.90%	**0.64%**	**0.96%**	1.17%	**0.72%**	**0.46%**
3. Hybrid model:							
Postpassage dummy	-3.22%	5.90%	5.36%	13.30%	**19.60%**	**12.70%**	**11.00%**
	6.96%	5.81%	3.82%	7.36%	**9.00%**	**4.96%**	**3.69%**
Trend effect	0.26%	0.45%	1.17%	1.24%	0.90%	0.99%	**1.00%**
	0.89%	0.71%	0.63%	0.96%	0.86%	0.56%	**0.42%**

Table E4: The Estimated Impact of RTC Laws – Using Donohue 2009 State-Level Data – No Controls, With State Trends and Clustered Standard Errors, All Crimes, 1977-2006

	Murder	Rape	Aggravated Assault	Robbery	Auto Theft	Burglary	Larceny
1. Dummy variable model:	-0.31%	**-4.66%**	0.62%	3.43%	8.38%	1.10%	0.92%
	3.73%	**2.00%**	3.36%	4.92%	5.28%	2.93%	2.37%
2. Spline model:	0.78%	-0.54%	<u>**2.46%**</u>	0.29%	-0.16%	-0.20%	-0.46%
	0.93%	0.92%	<u>**0.91%**</u>	1.39%	1.71%	0.80%	0.63%
3. Hybrid model:							
Postpassage dummy	-0.80%	**-4.39%**	-0.90%	3.30%	8.63%	1.25%	1.24%
	3.67%	**2.03%**	3.37%	5.30%	5.17%	3.23%	2.55%
Trend effect	0.80%	-0.44%	<u>**2.48%**</u>	0.21%	-0.39%	-0.23%	-0.49%
	0.93%	0.91%	<u>**0.92%**</u>	1.43%	1.70%	0.84%	0.67%