

COMMENTS

Confirming “More Guns, Less Crime”

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Analyzing county-level data for the entire United States from 1977 to 2000, we find annual reductions in murder rates between 1.5% and 2.3% for each additional year that a right-to-carry law is in effect. For the first five years that such a law is in effect, the total benefit from reduced crimes usually ranges between approximately \$2 billion and \$3 billion per year.

Ayres and Donohue have simply misread their own results. Their own most general specification that breaks down the impact of the law on a year-by-year basis shows large crime-reducing benefits. Virtually none of their claims that their county-level hybrid model implies initial significant increases in crime are correct. Overall, the vast majority of their estimates—based on data up to 1997—actually demonstrate that right-to-carry laws produce substantial crime-reducing benefits. We show that their models also do an extremely poor job of predicting the changes in crime rates after 1997.

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INTRODUCTION

Quite a few empirical papers have examined the impact of right-to-carry laws on crime rates. Most studies have found significant benefits, with some finding reductions in murder rates twice as large as the original research.¹

1. For a summary, see John R. Lott, Jr., *Guns, Crime, and Safety: Introduction*, 44 J.L. & ECON. 605, 605-14 (2001). Individual papers that show a benefit from the law include William Alan Bartley & Mark A. Cohen, *The Effect of Concealed Weapons Laws: An Extreme Bound Analysis*, 36 ECON. INQUIRY 258, 258-65 (1998); Stephen G. Bronars & John R. Lott, Jr., *Criminal Deterrence, Geographic Spillovers, and Right-to-Carry Laws*, AM.

Even the critics did not provide evidence that such laws have increased violent crime, accidental gun deaths, or suicides.²

Unlike previous authors, Ian Ayres and John Donohue claim to have found significant evidence that right-to-carry laws increased crime. However, they have misread their own results. The most detailed results they report—following the change in crime rates on a year-by-year basis before and after right-to-carry laws were adopted—clearly show large drops in violent crime that occur immediately after the laws were adopted. Their hybrid results showing a small increase in crime immediately after passage are not statistically significant and are an artifact of fitting a straight line to a curved one. When one examines a longer period—from 1977 to 2000—even this type of result disappears.

Ayres and Donohue's efforts have been valuable in forcing others to reexamine the evidence, extend the dataset over more years, and think of new ways to test hypotheses, and we appreciate their efforts.³ They are both highly regarded and well-known for their research, such as claiming that the legalization of abortion can account for half the drop in murder during the 1990s.⁴ Unfortunately, their research on this issue inaccurately describes the literature and also fails to address previous critiques of their work. For example, Ayres and Donohue claim that “[w]hen we added five years of county data and seven years of state data, allowing us to test an additional fourteen jurisdictions that adopted shall-issue laws, the previous Lott and Mustard findings proved not to be robust.”⁵ All their tables report results for “Lott’s

ECON. REV., May 1998, at 475-79; John R. Lott, Jr. & John E. Whitley, *Safe-Storage Gun Laws: Accidental Deaths, Suicides, and Crime*, 44 J.L. & ECON. 659, 659-89 (2001); Tomas B. Marvell, *The Impact of Banning Juvenile Gun Possession*, 44 J.L. & ECON. 691, 691-714 (2001); Carlisle E. Moody, *Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness*, 44 J.L. & ECON. 799, 799-813 (2001); David B. Mustard, *The Impact of Gun Laws on Police Deaths*, 44 J.L. & ECON. 635, 635-58 (2001); David E. Olson & Michael D. Maltz, *Right-to-Carry Concealed Weapon Laws and Homicide in Large U.S. Counties: The Effect on Weapon Types, Victim Characteristics, and Victim-Offender Relationships*, 44 J.L. & ECON. 747, 747-70 (2001); Florenz Plassmann & T. Nicolaus Tideman, *Does the Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say*, 44 J.L. & ECON. 771, 771-98 (2001); Eric Helland & Alexander Tabarrok, *Using Placebo Laws to Test “More Guns, Less Crime”: A Note* (Univ. of Chi. Graduate Sch. of Bus., Working Paper, 2002).

2. In fact, these critics provided a great deal of supportive evidence. See *infra* app. tbl.1.

3. For an earlier discussion on Lott's research, see Ian Ayres & John J. Donohue III, *Nondiscretionary Concealed Weapons Law: A Case Study of Statistics, Standards of Proof, and Public Policy*, 1 AM. L. & ECON. REV. 436 (1999); John R. Lott, Jr., *More Guns, Less Crime: A Response to Ayres and Donohue* (Yale Law Sch., Working Paper, 1999), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=248328#Paper%20Download.

4. John J. Donohue III & Steven D. Levitt, *The Impact of Legalizing Abortion on Crime Rates*, 116 Q.J. ECON. 379, 379-420 (2001).

5. Ian Ayres & John J. Donohue III, *Shooting Down the “More Guns, Less Crime”*

Time Period (1977-1992)" and compare those estimates with the "Entire Period (1977-1997)." Yet, whatever differences in results arise, they are not due to the inclusion of more data for a longer period. Their paper gives a misleading impression as to how much their research extends the data period, since Lott's book and other work examined both the county and state data up through 1996.⁶ Ayres and Donohue's work thus extends the county-level data by *one year*, from twenty to twenty-one years.

Part I of this Response reviews some of Ayres and Donohue's claims and shows that even their own estimates imply fairly consistently large annual benefits from reducing crime. We then extend the U.S. county-level data to 2000 in Part II, and, consistent with previous work, find large benefits from states adopting right-to-carry laws. As others have already found, the results are not sensitive to the inclusions of particular control variables, such as demographic measures. Finally, Part III provides direct responses and corrections to several specific claims made by Ayres and Donohue.

I. WHAT DOES THEIR EVIDENCE SHOW?

The most general specifications show the year-by-year changes in crime rates before and after the enactment of a right-to-carry law. Ayres and Donohue provide this breakdown for state-level data from 1977 to 1999. Their state-level data show the crime rates in the first year after the law was passed, the second year, and so on. While we disagree with some of their assumptions, their results provide a very useful starting point as their results stake out one side of the debate.

John Donohue has another paper addressing these issues published by the Brookings Institution.⁷ This paper presents the year-by-year changes for *county*-level data from 1977 to 1997. The county-level estimates report the crime rates in two-year intervals, and a separate dummy variable measures the combined effects whenever the state has had the law for eight or more years.⁸

Hypothesis, 55 STAN. L. REV. 1193, 1296 (2003).

6. In one footnote Ayres and Donohue acknowledge that these additional data were used, but they claim that Lott "only reports results for this dataset from tests of the trend specification." *Id.* at 1265 n.90. In Lott's book, figures 9.1 to 9.5 provide information on the nonlinear before-and-after trends; table 9.3 reports the relationship between the percent of the population with permits and crime rates (both linearly and nonlinearly); figures 9.6 to 9.9 show the impact of interacting the percent of the adult population with permits to different demographic characteristics; and figures 9.10 to 9.13 examine the sensitivity of the estimates to different combinations of the control variables. See JOHN R. LOTT, JR., *MORE GUNS, LESS CRIME* (2d ed. 2000). Other work that has examined the data through 1996 includes Lott & Whitley, *supra* note 1, and Mustard, *supra* note 1.

7. John J. Donohue, *The Impact of Concealed-Carry Laws*, in *EVALUATING GUN POLICY: EFFECTS ON CRIME AND VIOLENCE* 287 (Philip J. Cook & Jens Ludwig eds., 2003).

8. There are several advantages to the approach used for their county-level estimates. Using the two-year interval approach provides a better measure of trends without the

One of the county estimates includes a separate state time trend for each state. Although our principal focus is on Ayres and Donohue's joint paper, we will refer to Donohue's Brookings paper several times in this Part because the papers are very similar and because several important results (like the county-level year-by-year breakdowns) are only reported in the Brookings paper.

Note also that the county and state estimates use two different definitions of the implementation of state right-to-carry laws, with the county-level data using a "corrected" version of the dates that Lott and Mustard used from Kopel and Cramer, and the state-level data using definitions supplied by Vernick and Hepburn.⁹

A. *Is There a "Robbery Effect"?*

Robbery is a good place to start our inquiry because it is committed in public more than any other crime, and should be the crime most likely to decline if the Lott and Mustard story of deterrence has any plausibility.¹⁰

[T]he failure of the model to show a drop in robbery[] cast[s] doubt on the causal story that they advance.¹¹

Ayres and Donohue have consistently argued in several papers that robbery is the key result upon which the deterrence by right-to-carry laws is based.¹² In contrast, Lott has argued many times that there is no a priori reason to believe that the benefits of right-to-carry laws are larger for robbery than for other violent crimes.¹³ Putting that debate aside, the robbery results presented by

constraint of making all years have the same trend. The "wild" swings in both directions, as at the thirteenth and fourteenth year points in their state-level data, is simply due to only Maine being present for those observations. Examining just one state a decade and a half or more after a law is passed poses problems, particularly with state-level data where there is only one observation per year. Not only does it raise questions about what other factors may have changed in just that one state, but it also leads to extremely large confidence intervals. Thus, very little weight should be placed on those estimates, whether they are decreasing or increasing.

9. Jon Vernick & Lisa Hepburn, Description and Analysis of State and Federal Laws Affecting Firearm Manufacture, Sale, Possession, and Use, 1970-1999 (Johns Hopkins Ctr. for Gun Policy Research, Working Paper, 2001); *see also infra* tbl.5.

10. Ayres & Donohue, *supra* note 5, at 1206.

11. *Id.* at 1230.

12. Ayres & Donohue, *supra* note 3; *see also* Ayres & Donohue, *supra* note 5, at 1206, 1227 (noting that robbery is an important result in determining the deterrent effect of right-to-carry laws).

13. Lott notes that residential robberies are the second largest category of robbery and that concealed handgun laws could actually cause them to rise as criminals substitute out of street robberies. *See* LOTT, *supra* note 6, at 133-34. Just as criminals may switch between robbery and burglary, Lott also notes, "but to rank some of these different crimes [murder, rape, robbery, and aggravated assault], one requires information on how sensitive different types of criminals are to the increased threat." *Id.* at 134.

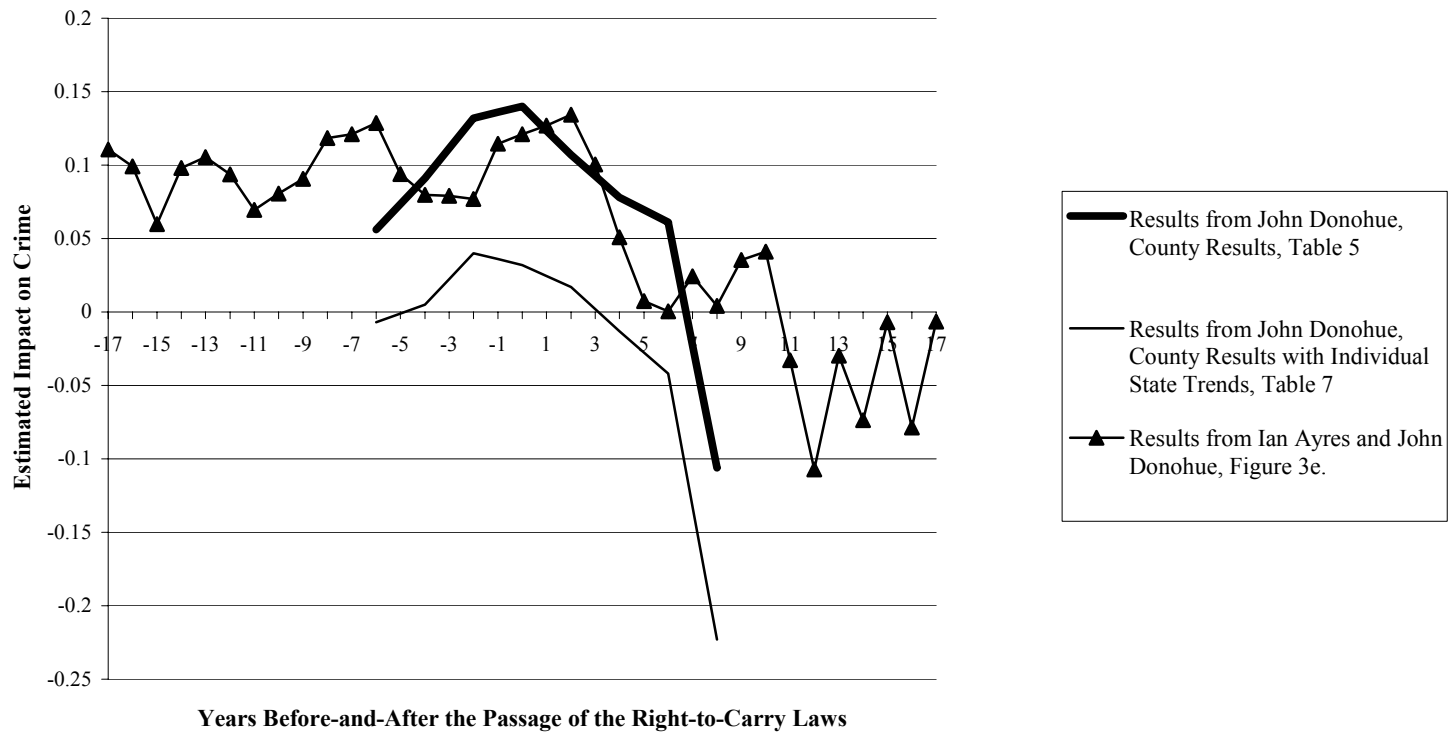
Ayres and Donohue present a very clear, consistent story.¹⁴ The state-level analysis shows that robbery rates continued rising, though at a slower rate, for the first two years after the law was passed. After that, however, robbery rates in right-to-carry states fell relative to non-right-to-carry states for the next nine years, and then remained fairly constant through year seventeen. The two sets of county-level estimates are even more dramatic. Robbery rates in right-to-carry states were rising until the laws were passed and then fell continually after that point. The pattern is very similar to that shown earlier by Lott in examining county-level data from 1977 to 1996.¹⁵

The changes are also very large. By the time the law has been in effect for six years, the county and state-level data imply a drop in robbery rates of eight and twelve percent respectively. It is difficult to see how anyone could look at these year-by-year results and accept their claims that the "robbery effect" is sensitive to the "time frame" examined or to the coding of when state laws were adopted. While Ayres and Donohue acknowledge the problems in using simple before-and-after averages in evaluating the impact of the law, they do not consistently apply that insight when discussing the evidence.

14. See *infra* fig. 1a.

15. See LOTT, *supra* note 6, at 172-74.

FIGURE 1A: AYRES AND DONOHUE'S ESTIMATED IMPACTS ON ROBBERY
(ALL THE YEAR-BY-YEAR ESTIMATES THAT IAN AYRES AND JOHN DONOHUE REPORT)



B. *Murder Rates*

Figure 1b illustrates Ayres and Donohue's own year-by-year estimates for murder. Their county and state estimates paint a very consistent picture, but they dismiss the fact that state data estimated a 4.5% drop in murder rates during the first three years of the law as showing "relatively little movement."¹⁶ Their state-level regressions indicate that murder rates were rising in the three years prior to the law being passed and then falling over the next thirteen years. Only one state, Maine, has had the law in effect for more than thirteen years. The increase during years fourteen, fifteen, sixteen, and seventeen thus solely reflects changes in Maine's murder rate, and since this is state-level data, each coefficient represents only one data point. The values for these four years show up in the data only because Ayres and Donohue recode Maine's right-to-carry law as going into effect in 1981 instead of 1985 as previous research had done.¹⁷ The increase between years thirteen and fourteen is also more apparent than real. The real "increase" is actually not due to any sudden change in Maine's crime rates, but to the fact that other states are included in calculating the crime rate for year thirteen, while only Maine is used for year fourteen.

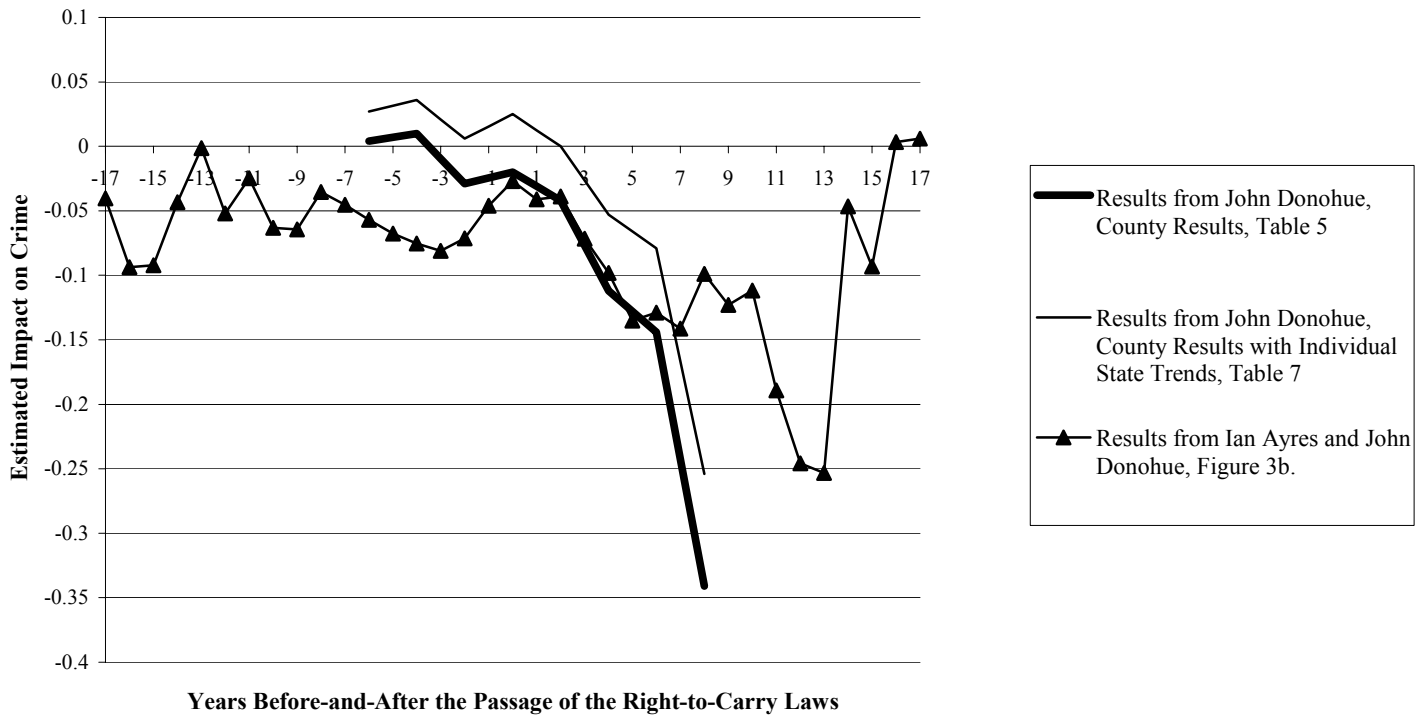
Both sets of county-level data again imply a large drop in crime that begins immediately after the law has been adopted and continues sharply down after that point.¹⁸ By the time the law has been in effect for six years, Ayres and Donohue's very own county and state estimates imply that murder rates had fallen by at least ten percent.

16. Ayres & Donohue, *supra* note 5, at 1245.

17. The laws in 1981 and 1985 differed in one crucial aspect: Under the 1981 law, city councils and mayors had responsibility for issuing permits. However, the police chiefs in Portland (with almost 20% of the state's population in 1985) and other major cities resisted issuing permits. The 1985 law overcame this problem by taking this power away from the city governments, particularly the Portland police chief, Frank Maoroso. Interview with Randy Kozuch, Legislative Affairs Director, NRA (Fall 2002).

18. Ayres and Donohue lump together the year of passage (year zero) with the first full year that the law is in effect, but if they had separated out the two, both the murder and robbery results would have also shown a drop in crimes between these two years.

FIGURE 1B: AYRES AND DONOHUE'S ESTIMATED IMPACTS ON MURDER
(ALL THE YEAR-BY-YEAR ESTIMATES THAT IAN AYRES AND JOHN DONOHUE REPORT)



C. *Rapes and Aggravated Assaults*

Ayres and Donohue's county and state-level results for rapes and aggravated assaults are more ambiguous. The county-level estimates without the individual state trends show that both rape and aggravated assault fell almost continually after the laws were enacted.¹⁹ Even choosing for comparison the sixth year after the law, where rape and aggravated assault rates have slightly risen back up, this still leaves rapes nine percent below their peak and aggravated assaults three percent below theirs.

The county-level estimates with individual state trends provide a mixed picture. With the exception of one single year, rape rates rise before the law and fall thereafter. In stark contrast, using individual state trends changes the aggravated assault rate into a line that rises continuously over almost the entire period until the law has been in effect for eight or more years. Yet, since the aggravated assault rate was rising for years prior to the law at least as fast as it was rising after the law was passed, it is hard to blame the right-to-carry law for this rise.

Ayres and Donohue's state-level results are also somewhat ambiguous, though even here the rape rates fall by ten percent for the first six years after the adoption of the law, and remain below the prelaw levels for at least twelve years. Only when Maine becomes the sole remaining state in the sample does the rape rate rise, and it rises above the prelaw levels for just one year (by seven percent). The rape rate then plunges by over twenty-five percent. With only one crime observation present here, the confidence intervals are so large that even with these "wild swings," the changes are too small to conclude that the temporary surge in rapes placed it above the prelaw levels. There is only one year out of the seventeen years after the law was passed that the rape rate exceeds any of the values during the twelve years before the law. This is similar for aggravated assaults: Only three of the seventeen years after the adoption of the law show higher rates than any of the ten years prior to the law.

19. See *infra* figs.1c, 1d.

FIGURE 1C: AYRES AND DONOHUE'S ESTIMATED IMPACTS ON RAPE
(ALL THE YEAR-BY-YEAR ESTIMATES THAT IAN AYRES AND JOHN DONOHUE REPORT)



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FIGURE 1D: AYRES AND DONOHUE'S ESTIMATED IMPACTS ON AGGRAVATED ASSAULT
(ALL THE YEAR-BY-YEAR ESTIMATES THAT IAN AYRES AND JOHN DONOHUE REPORT)



D. *Critiques of Year-by-Year Breakdown of the Law's Impact*

The debate over simple dummies, splines, or hybrids becomes irrelevant when one has examined the year-by-year breakdown. All these approaches are simple ways to summarize the crime patterns and can provide useful statistics to test whether there is a change in crime rates, but the year-by-year dummies provide a much more accurate picture of changing crime patterns.

It is important to understand what these year-by-year dummy estimates represent. The question is not whether these coefficients are different from zero,²⁰ but whether they have changed relative to other coefficients. A positive coefficient shows that the crime rate was higher in right-to-carry states than in non-right-to-carry states for that year, and coefficients that become less positive over time indicate that the crime rates in right-to-carry states are declining relative to other states. The patterns for robbery, murder, and rape clearly show that the longer the law is in effect, the greater the drop in crime.

It is also not relevant, as Ayres and Donohue suggest, to compare the crime levels before and after the law.²¹ When crime rates rise before the law and fall afterwards, there might be very little change in the before-and-after means even though, as the diagram for robbery indicates, something dramatic has changed. The key is to compare the trends before and after the law, and Ayres and Donohue's results in tables 10 and 11 imply large and statistically significant changes.²²

The year-by-year results do not support their claim that "the main effect of the shall-issue laws is positive but over time this effect gets overwhelmed as the linear trend turns crime down."²³ Their county-level results indicate that by

20. Donohue advocates this interpretation when he writes: "For the 1977-97 period the effect for the 'two or three years after' dummy is seen to be highly positive and statistically significant in seven of the nine categories. The other two categories are insignificant, with one negative (murder) and one positive (rape)." Donohue, *supra* note 7, at 312.

21. See Ayres & Donohue, *supra* note 5, at 1219. A comparison of years -1 and -2 before the law with years 2 and 3 after the law shows consistent declines. Murder declines from -2.9 to -4.2, rape from 3.7 to 2.6, robbery from 13.2 to 11, and aggravated assault from 6.7 to 6.3. These differences are not statistically significant by themselves, but as part of the trends they represent, the before-and-after trends are statistically significantly different from each other.

22. In Donohue's Brookings paper he writes: "Lott mentions . . . the so-called inverted V hypothesis. Although there might be some hint of this . . . , the effects are not statistically significant (and, even if real, could be caused by a regression to the mean effect as opposed to a benign influence of the shall-issue law)." Donohue, *supra* note 7, at 312. To test this, one must compare the before-and-after trends, and Ayres and Donohue's own estimates in their current paper do not support this claim. Both the spline and hybrid estimates on the 1977 to 1997 data reported in tables 10 and 11 of the Ayres and Donohue paper indicate consistent statistically significant changes in trends.

23. Ayres & Donohue, *supra* note 5, at 1264.

the second and third years after the law has been adopted, all violent crime rates are below the values that they had been in the two years preceding passage of the law.

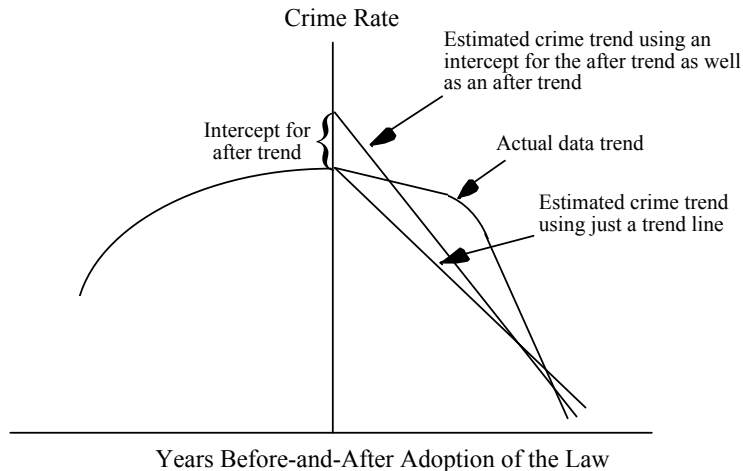
The figures and the standard errors associated with these estimates also allow us to directly evaluate their claims of model misspecification. One concern is about the claim regarding the county-level data that Donohue makes in his Brookings paper: "This particular result of a positive main effect and a negative trend effect is inconsistent with any plausible theoretical prediction of the impact of a shall-issue law, since it is not clear why the law should initially accelerate crime and then dampen it."²⁴ Yet, Ayres and Donohue's year-by-year estimates shown in our Figures 1a through 1d indicate that no such "positive main effect" is occurring.

A claim might be made that the hybrid model is misspecified solely because Ayres and Donohue are fitting a straight line to a nonlinear relationship. As an example, take Figure 2, where the crime rate is falling at an increasing rate after the adoption of the right-to-carry law. In order to fit a regression with both an intercept shift and a linear trend line to these nonlinear data, the intercept will have to be positive and the trend line will become steeper compared to a specification that uses only a trend line but no intercept shift.²⁵ This does not mean that there is actually an initial increase in crime, but only that it is an artifact of fitting a straight line to nonlinear data. Even a quadratic curve with an intercept may overestimate the crime rates in the initial years. The only solution is to measure the changes in crime on a year-by-year basis.

24. Donohue, *supra* note 7, at 297. Ayres and Donohue, *supra* note 5, at 1264, write: "[T]he main effect of the shall-issue laws is positive but over time this effect gets overwhelmed . . ." See also *id.* at 1221.

25. Indeed, if one compares the spline and the hybrid estimates in all of Ayres and Donohue's tables that compare the spline and the hybrid models, this is exactly the pattern that one observes. Comparison of the results in lines 5 and 6 of these tables shows that the positive intercept shift is associated with the trend line becoming more steeply negative.

FIGURE 2: SHOWING WHAT HAPPENS WHEN YOU FIT AN INTERCEPT SHIFT AND A TREND TO A NONLINEAR TREND



We can use their tables to address a second type of misinterpretation of estimation results. In discussing the state-level year-by-year estimates shown in their figures 3a through 3i, Ayres and Donohue note that:

[O]ne can see that in four of the five violent crime categories and for burglary, even before adopting states passed their shall-issue legislation, crime was substantially higher than the regression model would have otherwise predicted (given the full array of explanatory variables). This raises concerns about the reliability of the regression model . . .²⁶

Their statement that “crime was substantially higher” is misleading because the differences are not statistically significant. While Ayres and Donohue’s state-level figures 3a through 3i do not report standard errors, this information is reported in Donohue’s Brookings paper for the 1977 to 1997 time frame.²⁷ The crime rates for violent crimes, rape, robbery, and aggravated assault were never

26. Ayres & Donohue, *supra* note 5, at 1255.

27. Donohue, *supra* note 7, at 310-11 tbl.8-6. The differences in point estimate values between Donohue’s table 8-6, *id.*, and Ayres and Donohue’s figures 3a-3i, Ayres & Donohue, *supra* note 5, at 1246-54, simply arise because Ayres and Donohue include all possible year dummies for their figures, while only a portion of them is included for Donohue’s Brookings paper table 8-6. The addition of two years to the dataset is not the crucial difference. The difference is that in Donohue’s Brookings paper’s table 6, the year values prior to year -6 are in the intercept term. Raising the intercept term reduces the size of the coefficients for the remaining year-by-year dummies. The relative pattern of the year-by-year dummies remains unchanged, but their significance relative to zero does change. This very point makes it clear how arbitrary it is to focus on whether these year-by-year dummies are different from zero, and not the more relevant question of whether the year-by-year dummies differ from each other in systematic ways.

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statistically significantly different from zero (we assume that Ayres and Donohue refer to "zero" when they say "the regression model would otherwise have predicted") for at least the four years prior to the adoption of the law. For murder, the difference was statistically significant for only three to four years prior to adoption, but not in years one, two, five, or six.

We will review our concerns with Ayres and Donohue's tests and how they interpret them in Part III, but even putting aside those concerns, it is relevant to point out that their own estimates provide a consistent story of the benefits from right-to-carry laws. Despite a nonrandom reporting of regressions that are not even consistent across tables, and using a hybrid model over a short period that overpredicts the costs, the vast majority of their evidence implies that the passage of concealed handgun laws reduces violent crime rates.

E. *The Estimated Benefits from the Law*

Table 1 takes all the county-level estimates reported by Ayres and Donohue in their current paper using the 1977 to 1997 data and applies their method of evaluating the changes in the social cost of crime from the concealed handgun law. Table 2 applies this method of determining the total costs or benefits to all the tables provided in Donohue's Brookings paper.²⁸ The separate estimates in Donohue's Brookings paper do include year-by-year effects of the law, in addition to the dummy, spline, and hybrid specifications.²⁹

Ayres and Donohue's estimated \$1 billion increase in the annual costs of crime from the concealed handgun law relies on the hybrid estimates from their table 13.³⁰ The other two county hybrid results that they report imply annualized benefits from reduced crime of \$1.7 and \$1.05 billion. Despite their hybrid model overpredicting crime rates in early years, which is exacerbated by

28. Tables 10 and 11 in Ayres & Donohue, *supra* note 5, at 1262, 1269, and tables 8-1 and 8-3 in Donohue, *supra* note 7, at 294, 303, represent the same specifications. While these two sets of tables use identical specifications over the same 1977 to 1997 period, there are usually only small differences. The results are qualitatively the same.

29. A caveat worth noting is that Ayres and Donohue do not report the same sets of specifications (dummy, spline, and hybrid) across all tables. There is no discussion of why splines are reported in some tables and not others, but the omitted estimates frequently produce the largest benefits. In any case, we will stick to the sets of specifications that the authors report.

30. We calculate the estimated social costs/benefits for the dummy, spline, and hybrid models for the first five years of the law in the same way as Ayres and Donohue. However, Ayres and Donohue do not make these calculations for the regressions estimating the changing year-by-year impact of the law. Even though Ayres and Donohue include a prelaw crime rate trend (as used in the spline or hybrid models), no prelaw trend exists as a baseline for the year-by-year estimates. Therefore, we will use the crime rate when the law was passed. While this is the simplest approach, it also biases down (up) the gains (losses) from the law, especially for the aggravated assault when individual state-level trends are included (Figure 1d).

the short five-year period they examine, their three hybrid estimates imply annualized benefits of \$560 million. Even when the dummy variable estimates are included, which even Ayres and Donohue agree are flawed, their estimates imply an average annualized benefit of \$233 million. Dropping the dummy estimates raises the estimated benefit to \$1.34 billion per year.

We also broke down Ayres and Donohue's state-level estimates reported in their tables 1 through 9 and their figures 3a through 3i. Using the same five-year period after the adoption of the law, there is an average annualized benefit from right-to-carry laws across all the specifications of \$766 million.³¹

Table 2 does the same breakdown for all the tables listed in Donohue's Brookings paper, using both the county- and state-level data from 1977 to 1997. While Donohue argues that this evidence strongly shows that concealed handgun laws are not beneficial, all but one of the estimates in his eight tables imply that the costs of crime fall with the passage of right-to-carry laws. The average estimate implies savings of \$1.84 billion per year. Simple dummy-variable specifications imply much smaller annual gains from right-to-carry laws: They show a gain of \$847 million using only the dummies versus an average benefit of \$2.2 billion for the other specifications. Including a time trend for each individual state reduces the benefits estimated from county-level data from \$2.1 billion to \$233 million, though it produces a much smaller reduction in the estimated benefit for state-level data. At least for the first five years after the adoption of the law, the spline estimates imply benefits that are almost twice as large as those of the hybrids. The only estimate in Donohue's Brookings paper that implies that crime rates would rise uses only a postpassage dummy variable combined with individual state time trends.

Returning to Table 1, the losses generated by Ayres and Donohue's table 13 are dominated by a few states. The table suggests that Kentucky's murder rate increased by an average of forty-two percent during the law's first five years, Louisiana's by thirty-four percent, and Tennessee's by thirty percent. Given that Ayres and Donohue estimated the five-year costs with only one full year of data for Kentucky, Louisiana, and South Carolina, some investigation seems warranted.³²

31. The year-by-year breakdown in the impact of the law reported in Figures 3a through 3i are the most detailed breakdown and they produce the largest benefit (\$2.1 billion) of all the weighted least squares estimates. Among the two extremes for the other weighted least squares results, their dummy estimates imply an average loss per year of \$354 million and their spline estimates imply an average benefit per year of \$784 million.

32. All three states adopted the law in 1996, though few permits were issued in any of the states during the first year. Louisiana issued only 160 permits before 1997. *Governor's Promises vs. Performance*, ADVOCATE (Baton Rouge), Jan. 12, 1997, at 8A.

TABLE 1: COST-BENEFIT ANALYSES OF AYRES AND DONOHUE'S COUNTY-LEVEL REGRESSIONS:
FIVE-YEAR AVERAGE ESTIMATE OF NET COSTS/BENEFITS

Model	Total	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny	
Table 10									
(4) Dummy	# crimes		-678	-1,225	-560	-1,160	53,743	19,126	297,129
	% crimes		-7.7%	-3.2%	-0.3%	-0.3%	10.8%	1.6%	9.4%
	\$ cost/benefit	-\$1,889.4	-\$2,096.2	-\$112.1	-\$4.7	-\$29.3	\$209.2	\$28.2	\$115.6
(5) Spline	# crimes		-713	-3,101	-20,142	-31,870	-11,943	-93,239	-104,311
	% crimes		-8.1%	-8.1%	-10.8%	-8.2%	-2.4%	-7.8%	-3.3%
	\$ cost/benefit	-\$3,687.3	-\$2,205.1	-\$283.8	-\$169.5	-\$804.6	-\$46.5	-\$137.2	-\$40.6
(6) Hybrid	# crimes		-317	-1,646	-16,226	-16,630	16,421	-59,769	6,322
	% crimes		-3.6%	-4.3%	-8.7%	-4.3%	3.3%	-5.0%	0.2%
	\$ cost/benefit	-\$1,708.7	-\$980.0	-\$150.7	-\$136.6	-\$419.9	\$63.9	-\$88.0	\$2.5
Table 11: Includes state trends									
(3) Dummy	# crimes		-18	995	0	27,459	17,417	5,977	126,438
	% crimes		-0.2%	2.6%	0.0%	7.1%	3.5%	0.5%	4.0%
	\$ cost/benefit	\$855.7	-\$54.4	\$91.1	\$0.0	\$693.3	\$67.8	\$8.8	\$49.2
(4) Hybrid	# crimes		-537	-574	-9,885	30,166	1,493	-44,229	110,633
	% crimes		-6.1%	-1.5%	-5.3%	7.8%	0.3%	-3.7%	3.5%
	\$ cost/benefit	-\$1,051.0	-\$1,660.6	-\$52.6	-\$83.2	\$761.6	\$5.8	-\$65.1	\$43.0
Table 12: State-specific estimate, individual state time trends									
Dummy	# crimes		76.8	665.0	-1,687.1	3,1522.3	3,0748.6	5,683.1	176,804.4
	% crimes		0.9%	1.7%	-0.9%	7.8%	4.7%	0.3%	4.2%
	\$ cost/benefit	\$1,277.0	\$237.7	\$60.9	-\$14.2	\$795.8	\$119.7	\$8.4	\$68.8
Table 13: State-specific estimate, individual state time trends									
Hybrid	# crimes		-30	111	506	36,415	37,110	13,526	191,889
	% crimes		-0.3%	0.3%	0.3%	9.1%	5.7%	0.8%	4.6%
	\$ cost/benefit	\$1,078.9	-\$93.9	\$10.2	\$4.3	\$919.4	\$144.4	\$19.9	\$74.6
Average predicted change									
	# crimes		-101	-217	-2,182	3,450	6,590	-6,951	36,587
	% crimes		-1.1%	-0.6%	-1.2%	0.9%	1.2%	-0.6%	1.0%
	\$ cost/benefit	-\$232.9	-\$311	-\$20	-\$18	\$87	\$26	-\$10	\$14

TABLE 2: COST-BENEFIT ANALYSES OF COUNTY- AND STATE-LEVEL REGRESSIONS IN DONOHUE'S BROOKINGS PAPER: FIVE-YEAR AVERAGE ESTIMATE OF NET COSTS/BENEFITS

Model		Total	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny
Table 1: County data, the same as Ayres and Donohue's Table 10									
(4) Dummy	# crimes		-684	-1,116	-703	-242	53,825	17,551	302,562
	% crimes		-7.8%	-2.9%	-0.4%	-0.1%	10.8%	1.5%	9.6%
	\$ cost/benefit	-\$1,875.7	-\$2,114.5	-\$102.2	-\$5.9	-\$6.1	\$209.5	\$25.8	\$117.7
(5) Spline	# crimes		-1,029	-2,021	-12,480	11,460	-2,920	-57,307	34,183
	% crimes		-11.7%	-5.3%	-6.7%	3.0%	-0.6%	-4.8%	1.1%
	\$ cost/benefit	-\$3,264.4	-\$3,181.3	-\$184.9	-\$105.0	\$289.3	-\$11.4	-\$84.4	\$13.3
(6) Hybrid	# crimes		-757	22	-936	40,697	40,781	11,116	272,286
	% crimes		-8.6%	0.1%	-0.5%	10.5%	8.2%	0.9%	8.6%
	\$ cost/benefit	-\$1,039.2	-\$2,341.8	\$2.0	-\$7.9	\$1,027.5	\$158.7	\$16.4	\$105.9
Table 2: State data									
(4) Dummy	# crimes		-398	-1,799	-13,603	-22,972	29,096	-50,133	21,255
	% crimes		-4.5%	-4.7%	-7.3%	-5.9%	5.8%	-4.2%	0.7%
	\$ cost/benefit	-\$2,043.2	-\$1,231.8	-\$164.6	-\$114.5	-\$580.0	\$113.2	-\$73.8	\$8.3
(5) Spline	# crimes		-802	-1,585	-17,437	5,329	-26,443	-52,117	-9,245
	% crimes		-9.1%	-4.1%	-9.3%	1.4%	-5.3%	-4.4%	-0.3%
	\$ cost/benefit	-\$2,822.1	-\$2,481.7	-\$145.0	-\$146.7	\$134.5	-\$102.9	-\$76.7	-\$3.6
(6) Hybrid	# crimes		-801	-1,593	-16,932	2,953	8,504	-39,656	46,642
	% crimes		-9.1%	-4.2%	-9.1%	0.8%	1.7%	-3.3%	1.5%
	\$ cost/benefit	-\$2,699.6	-\$2,478.8	-\$145.8	-\$142.5	\$74.5	\$33.1	-\$58.4	\$18.1
Table 3: County data, Includes state trends, the same as Ayres and Donohue's Table 11									
(4) Dummy	# crimes		3	1,049	606	28,136	20,154	4,536	130,920
	% crimes		0.0%	2.7%	0.3%	7.3%	4.1%	0.4%	4.1%
	\$ cost/benefit	\$955.9	\$8.5	\$96.0	\$5.1	\$710.4	\$78.4	\$6.7	\$50.9
(5) Hybrid	# crimes		-519	-561	-9,406	30,404	4,451	-44,290	117,267
	% crimes		-5.9%	-1.5%	-5.0%	7.9%	0.9%	-3.7%	3.7%
	\$ cost/benefit	-\$969.7	-\$1,604.6	-\$51.3	-\$79.2	\$767.6	\$17.3	-\$65.2	\$45.6

continued...

TABLE 2 (CONTINUED)

Model	Total	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny
Table 4: State data, includes state trends								
(3) Dummy	# crimes	-166	-441	-1,218	485	22,720	7,890	63,567
	% crimes	-1.9%	-1.2%	-0.7%	0.1%	4.6%	0.7%	2.0%
	\$ cost/benefit	-\$425.6	-\$512.0	-\$40.3	-\$10.2	\$12.2	\$88.4	\$24.7
(4) Hybrid	# crimes	-699	-1,599	-18,333	6,304	-14,087	-31,292	29,805
	% crimes	-7.9%	-4.2%	-9.8%	1.6%	-2.8%	-2.6%	0.9%
	\$ cost/benefit	-\$2,391.6	-\$2,160.8	-\$146.4	-\$154.3	\$159.2	-\$54.8	\$11.6
Table 5: County data								
Year	# crimes	-511	-2,921	-8,560	-15,818	9,256	-20,082	170,691
dummies	% crimes	-5.8%	-7.6%	-4.6%	-4.1%	1.9%	-1.7%	5.4%
	\$ cost/benefit	-\$2,244.8	-\$1,578.9	-\$267.4	-\$72.0	-\$399.4	\$36.0	\$66.4
Table 6: State Data								
Year	# crimes	-589	-2,374	-17,811	-19,492	-11,346	-79,493	-5,058
dummies	% crimes	-6.7%	-6.2%	-9.6%	-5.0%	-2.3%	-6.7%	-0.2%
	\$ cost/benefit	-\$2,843.6	-\$1,821.2	-\$217.3	-\$149.9	-\$44.2	-\$117.0	-\$2.0
Table 7: County data, includes state trends								
Year	# crimes	-454	-1,106	-5,856	36,238	-9,554	-64,909	227,272
dummies	% crimes	-5.2%	-2.9%	-3.1%	9.4%	-1.9%	-5.4%	7.2%
	\$ cost/benefit	-\$684.7	-\$1,404.7	-\$101.3	-\$49.3	\$914.9	-\$37.2	\$88.4
Table 8: State data, includes state trends								
Year	# crimes	-841	-3,032	-32,116	7,890	-56,281	-110,214	-90,087
dummies	% crimes	-9.6%	-7.9%	-17.2%	2.0%	-11.3%	-9.2%	-2.9%
	\$ cost/benefit	-3364.7	-2599.8	-277.5	-270.3	199.2	-219.0	-162.2
Average predicted change								
	# crimes	-589	-1,363	-11,056	7,955	4,868	-36,314	93,719
	% crimes	-6.7%	-3.6%	-5.9%	2.1%	1.0%	-3.0%	3.0%
	\$ cost/benefit	-1836.6	-1821.7	-124.7	-93.0	200.8	18.9	-53.5
Average \$cost/benefit predicted change by type								
Dummy		-847.1	-962.5	-52.8	-31.4	34.1	122.4	-7.4
Spline		-\$3,043.3	-\$2,831.5	-\$165.0	-\$125.9	\$ 211.9	-\$57.2	\$ 4.9
Hybrid		-\$1,775.0	-\$2,146.5	-\$85.4	-\$96.0	\$ 507.2	\$ 38.6	\$ 45.3
Year Dummies		-\$2,284.5	-\$1,851.3	-\$215.8	-\$135.3	\$ 55.8	-\$66.0	\$ 29.2

Although a forty-two percent increase in Kentucky's murder rate should be easily spotted, this is not the case. Figure 3 shows the actual change in Kentucky's murder rates during the 1990s, and compares it to the change in murder rates for other states in the Midwest and for the United States as a whole. While the United States and Midwest murder rates were either unchanged or falling from 1992 to 1995, Kentucky's murder rate was rising. Kentucky's murder rate fell when the law was just getting started in 1996, and continued declining after that. Both percentage declines were much greater than the declines over the same period for the rest of the Midwest or the United States as a whole. Nor do other factors imply that Kentucky should have had an even bigger drop had it not been for the detrimental impact of the law (for example, Kentucky's arrest rate declined by forty percent between 1995 and 1998, and continued declining after that).³³

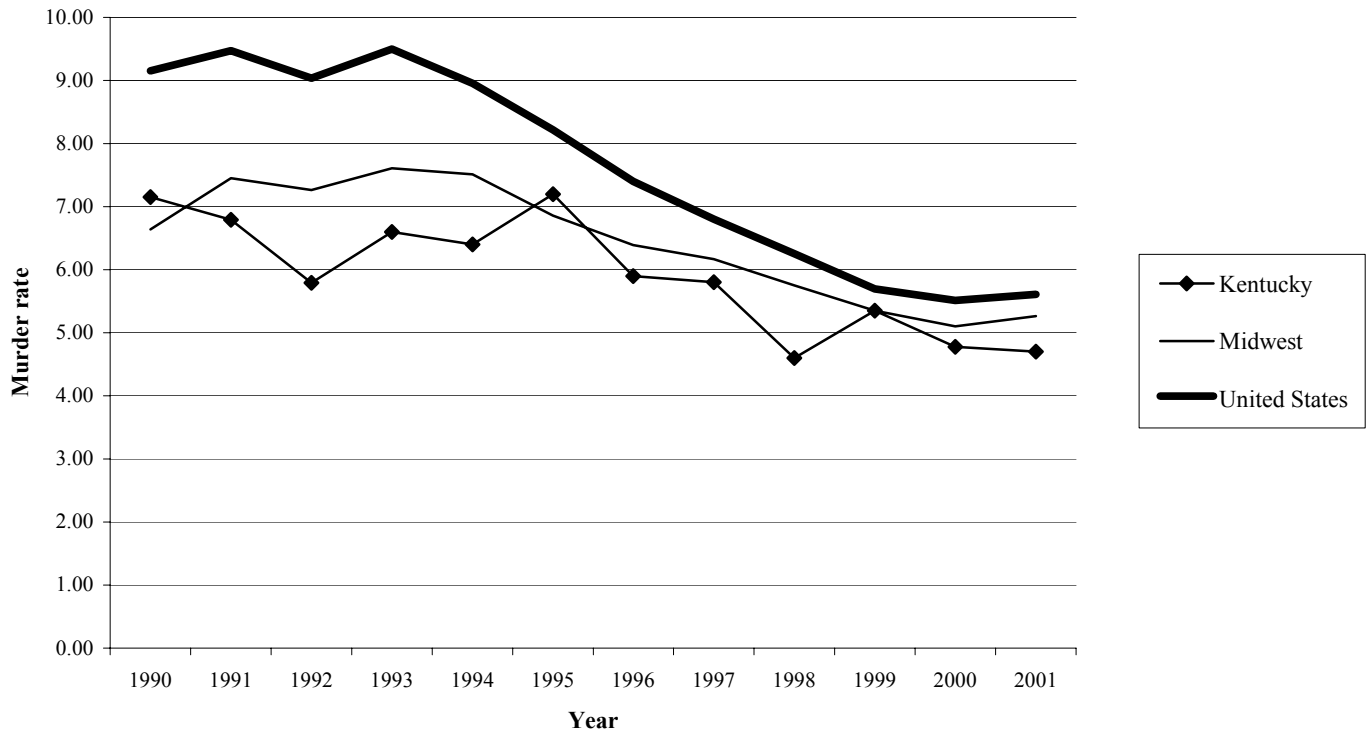
33. A similar breakdown for Louisiana, South Carolina, and Tennessee is available from the authors.

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FIGURE 3: EXAMINING THE LAST STATES TO ADOPT THE RIGHT-TO-CARRY LAW



II. COUNTY-LEVEL DATA FROM 1977 TO 2000

A. *Advantages and Disadvantages of Different Types of Data*

While most crime analysis has traditionally been done at the state level, disaggregated data has an important advantage in that both crime and arrest rates vary widely within states. In fact, the variation in both crime and arrest rates across states is almost always smaller than the average within-state variation across counties.³⁴ It is no more accurate to view all counties in the typical state as a homogenous unit than it is to view all states in the United States as one homogenous unit. For example, when a state's arrest rate rises, it may make a big difference whether that increase is taking place in the most or least crime-prone counties, or whether it is increasing a lot in one jurisdiction or across the entire state.

A simple example can show this potential "aggregation bias." Assume, for the sake of argument, that income is negatively correlated with property crimes (that is, a person with higher income commits fewer property crimes). Assume that Location A has a population of 1000 persons and Location B has a population of 2000 persons, with respective per capita incomes of \$50,000 and \$40,000 and property crimes of 100 and 200. Now assume that the per capita income at Location A increases to \$60,000 and crimes fall to eighty, and the per capita income at Location B decreases to \$36,000 and the number of crimes increases to 240. If we examined both locations separately, we would detect that increases in income lead to fewer property crimes and vice versa. However, if we instead aggregated both locations into a single location, we would observe that overall per capita income had increased (from \$43,334 to \$44,000) and that the number of property crimes committed had increased as well (from 300 to 320). Aggregating the data gives the false impression that increases in income lead to increases in the number of property crimes.

While there is a fair degree of similarity between state- and county-level data, as shown by Ayres and Donohue's yearly breakdown of the impact of right-to-carry laws, we will concentrate here on the county-level data, both because we believe that it provides a much more accurate measure of changes in crime rates and because of time and space constraints.

B. *Extending the Data to 2000*

There are seven different types of estimates that have been used to evaluate the impact of right-to-carry laws: a dummy variable for the law; before-and-

34. LOTT, *supra* note 6, at 21-35.

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after trends; a "hybrid" approach; the impact of the law on a year-by-year basis before and after the law; nonlinear before-and-after trends; some county-level data on the per capita number of permits issued; and the predicted number of permits based upon the characteristics of the right-to-carry laws and some limited information on permit issuance in some states.

We analyze the county-level data from 1977 to 2000 using the four different methods discussed in Ayres and Donohue's paper and in Donohue's Brookings paper. We provide a more precise breakdown of year-by-year impacts from the law in the Appendix, but for the text we follow the two-year interval approach used by Donohue so as to make our results more comparable. The regressions use all control variables employed in the second edition of *More Guns, Less Crime*.³⁵ In addition to arrest rates, we use: per capita income; population density; the execution rate for murder; per capita welfare payments; per capita unemployment insurance payments; retirement payments per person over age sixty-five; thirty-six different demographic measures by age, gender, and race; state-level poverty and unemployment rates; county and regional "year-fixed" effects.³⁶

Figures 4a through 4f graphically report the results for a postpassage dummy, spline, hybrid, and year-by-year impacts.³⁷ Two conclusions are readily apparent from these Figures. First, the year-by-year impacts of the law are very similar to those reported by Ayres and Donohue for the 1977 to 1997 period. The first six years during which the right-to-carry law is in effect are associated with about ten percent declines in murder and rape and an eight percent decline in robbery rates. The year-by-year breakdown in the Appendix shows that by the second full year of the law, all four violent crime categories have experienced large drops, with murder falling by 5% and robbery by 8.7%.³⁸

35. Lott, *supra* note 6.

36. Despite Ayres and Donohue's claim that "many of the explanatory variables are only measured on the state level," Ayres & Donohue, *supra* note 5, at 1259, with the exception of the state-level poverty and unemployment rates these are all county-level control variables. The regressions reported in this Response use crime-specific arrest rates. We repeated the analyses of murder, rape, robbery, and aggravated assault with the arrest rate of violent crime and obtained very similar estimates.

37. The other property crime rates are not included to save space, but are available from the authors.

38. For rapes and aggravated assaults, the small increases from year zero to year one seem at least in part to represent an upward trend that had been occurring for those crime rates over a period of eight years in the case of rape and five years for aggravated assault.

FIGURE 4A: VIOLENT CRIME: WEIGHTED LEAST SQUARES ESTIMATES USING COUNTY-LEVEL DATA FROM 1977 TO 2000



FIGURE 4B: MURDER: WEIGHTED LEAST SQUARES ESTIMATES USING COUNTY-LEVEL DATA FROM 1977 TO 2000

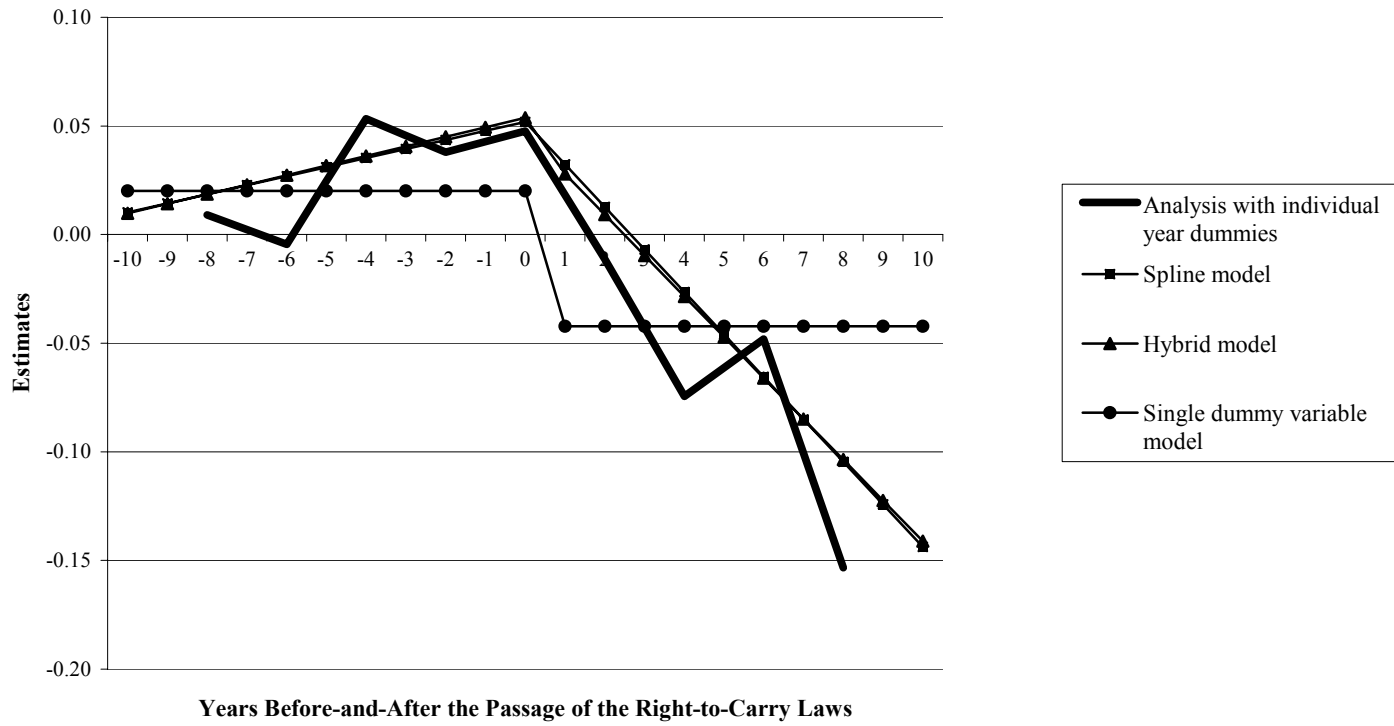


FIGURE 4C: RAPE: WEIGHTED LEAST SQUARES ESTIMATES USING COUNTY-LEVEL DATA FROM 1977 TO 2000

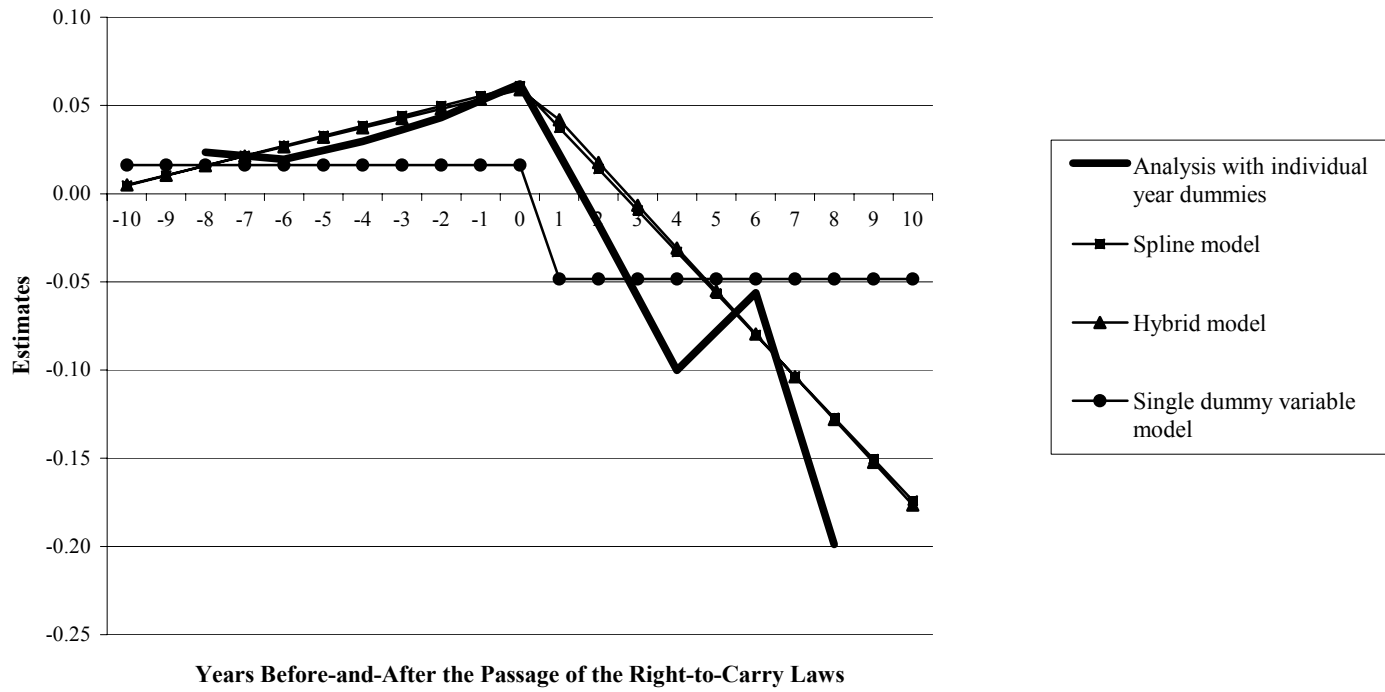


FIGURE 4D: ROBBERY: WEIGHTED LEAST SQUARES ESTIMATES USING COUNTY-LEVEL DATA FROM 1977 TO 2000

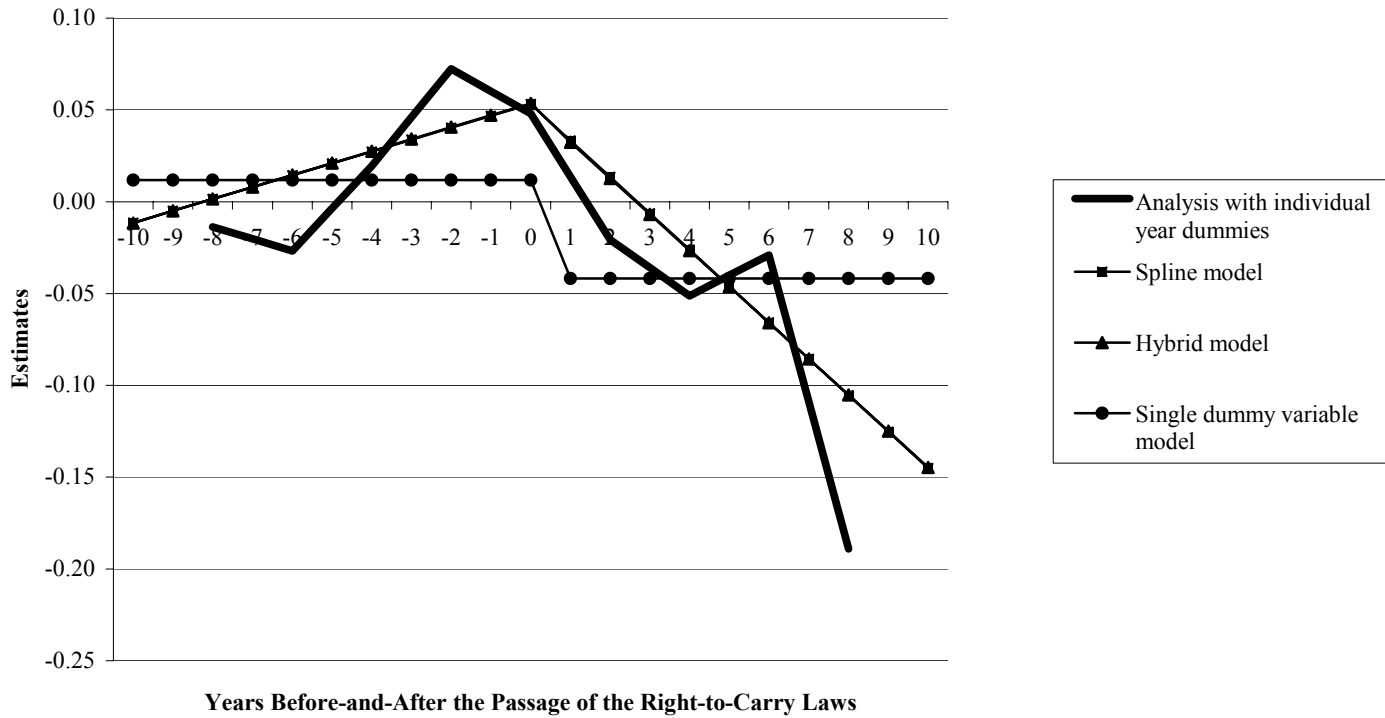
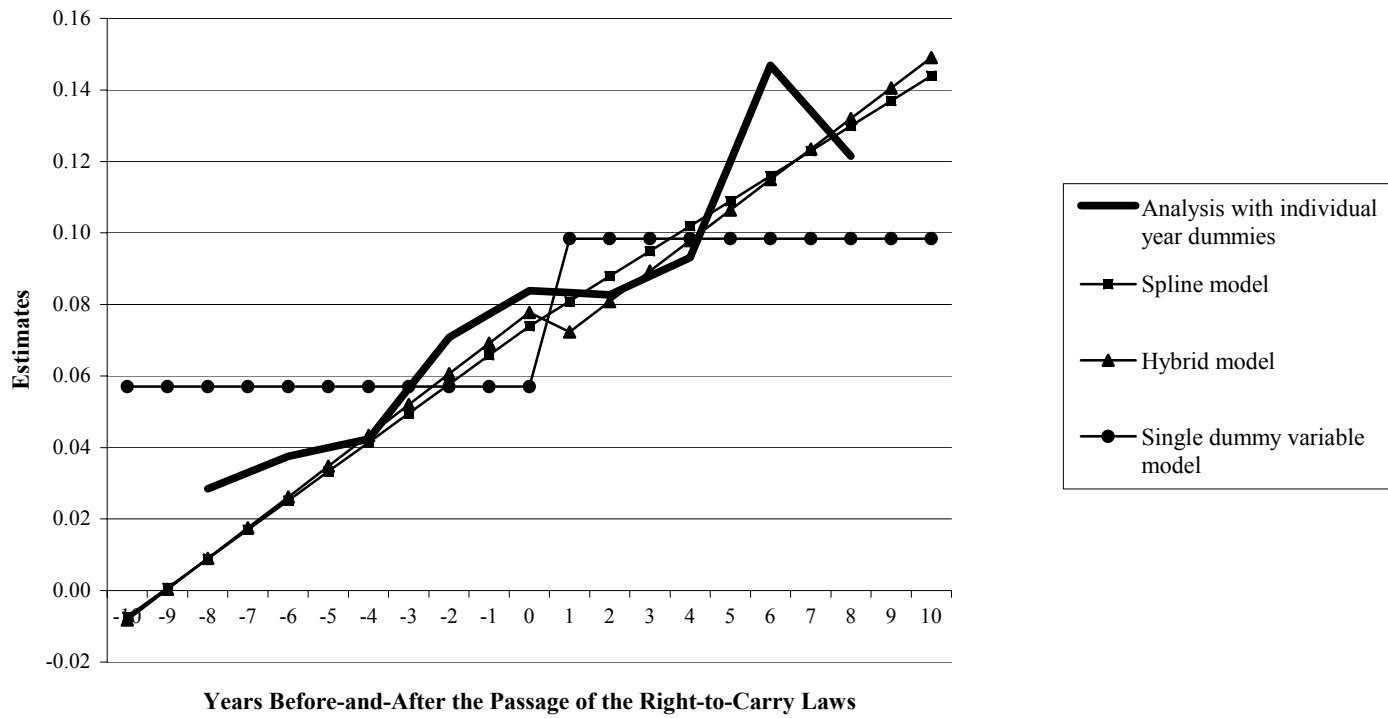


FIGURE 4E: AGGRAVATED ASSAULT: WEIGHTED LEAST SQUARES ESTIMATES USING COUNTY-LEVEL DATA FROM 1977 TO 2000



FIGURE 4F: PROPERTY CRIMES: WEIGHTED LEAST SQUARES ESTIMATES USING COUNTY-LEVEL DATA FROM 1977 TO 2000



Second, both the spline and hybrid models closely track the more disaggregated year-by-year estimates. In fact, for murder, rape, and especially the robbery estimates, the spline and hybrid estimates are virtually identical. The hybrid's postpassage dummy is essentially zero for those three crime categories.

Table 3a provides the exact results and significance levels behind these specifications, and reports the robust standard errors that adjust for clustering at the state level. The spline and the hybrid models indicate positive, but statistically insignificant, trends in violent crime rates prior to the right-to-carry law. After the law has been passed, however, violent crime rates decline. The change in trends is statistically significant at least at the ten-percent level for all individual violent crime categories for the spline estimates, implying that murder, rape, and robbery fall by over two percent per year during each additional year that right-to-carry laws are in effect. While the effect for murder is slightly larger than the 1.5% annual decline found by Lott using data from 1977 to 1996, the results for rape and robbery are somewhat smaller than the 3.2% and 2.9% annual declines found earlier by Lott.

The hybrid model estimates the change in before-and-after trends and gives identical results to those found with the spline model (up to the third decimal place). The changes in trends for rape and robbery are again significant at better than the ten-percent level. The impact of the law on murder rates is also statistically significant at least at the ten-percent level when the negative intercept shift is included in the *F*-test.³⁹ In none of these estimates is the postpassage dummy statistically significant by itself.⁴⁰ Whatever different results Ayres and Donohue obtained for the postpassage dummy with their hybrid approach, these effects disappear when the additional data are included.

39. The *F*-statistic equals 3.14 with a probability of 4.4%.

40. However, since both the postpassage dummy variable and the after law time trend are both measured at the same time, it is not really possible to claim that the initial crime rate rises or falls based upon just the value of the postpassage dummy, as Ayres and Donohue do. In this case, the net effect of adding both the postpassage dummy and the difference in before-and-after trends together implies that during the first full year that the right-to-carry law is in effect, murder falls by 3%, rape falls by 2.3%, robbery falls by 2.8%, and aggravated assault rises by 1%. By the second year of the law, aggravated assaults will also have fallen by 1.1%.

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TABLE 3A: COMPARISON OF THE THREE DUMMY SPECIFICATIONS USED BY AYRES AND DONOHUE
(OUR MODEL INCLUDES REGION-SPECIFIC YEAR DUMMIES AND USES 1977-2000 DATA)

	Violent crime	Murder	Rape	Robbery	Aggravated Assault	Property crimes	Auto Theft	Burglary	Larceny
Single dummy variable model									
Post-passage dummy	-2.8% (4.1%)	-6.2% (3.1%)	<u>-6.5%</u> (2.6%)	-5.4% (3.1%)	-1.6% (4.9%)	<u>4.1%</u> (2.6%)	<u>9.0%</u> (4.4%)	0.4% (2.2%)	<u>6.0%</u> (2.3%)
R^2	0.86	0.81	0.76	0.91	0.81	0.82	0.85	0.81	0.80
Spline model									
Trend before	0.2% (0.4%)	0.4% (0.5%)	0.6% (0.4%)	0.7% (0.7%)	0.6% (0.6%)	<u>0.8%</u> (0.3%)	<u>1.1%</u> (0.5%)	<u>0.9%</u> (0.5%)	<u>1.1%</u> (0.4%)
Trend after	-0.5% (0.6%)	-2.0% (0.9%)	<u>-2.4%</u> (0.8%)	-2.0% (0.8%)	-1.3% (1.0%)	0.7% (0.5%)	0.7% (0.5%)	-1.1% (0.7%)	0.2% (0.9%)
R^2	0.86	0.81	0.76	0.91	0.81	0.82	0.85	0.81	0.80
Difference between trends	-0.0067	-0.0237	-0.0291	-0.0263	-0.0190	-0.0011	-0.0039	-0.0199	-0.0093
F -test statistic	1.21	3.69	6.82	4.09	2.75	0.03	0.40	3.24	0.59
Prob > F	27.1%	5.5%	0.9%	4.3%	9.7%	86.6%	52.7%	7.2%	44.1%
Hybrid model									
Trend before	0.3% (0.4%)	0.4% (0.4%)	0.5% (0.4%)	0.7% (0.7%)	0.5% (0.5%)	<u>0.9%</u> (0.3%)	<u>0.9%</u> (0.5%)	0.5% (0.5%)	<u>1.0%</u> (0.3%)
Post-passage dummy	-2.7% (3.6%)	-0.7% (5.0%)	0.7% (3.8%)	-0.2% (6.0%)	3.1% (6.1%)	-1.4% (2.7%)	6.2% (7.8%)	3.7% (5.2%)	4.3% (4.1%)
Trend after	-0.2% (0.7%)	-1.9% (1.3%)	-2.4% (1.1%)	<u>-2.0%</u> (1.2%)	-1.7% (1.4%)	0.9% (0.6%)	0.1% (1.0%)	-1.5% (1.1%)	-0.3% (1.3%)
R^2	0.86	0.81	0.76	0.91	0.81	0.82	0.85	0.81	0.80
Difference between trends	-0.0047	-0.0232	-0.0297	-0.0262	-0.0211	-0.0001	-0.0083	-0.0225	-0.0123
F -test statistic	0.42	2.44	5.34	3.06	2.18	0.00	0.75	2.89	0.69
Prob > F	51.8%	11.9%	2.1%	8.0%	14.0%	99.2%	38.7%	8.9%	40.5%
Number of observations in all 3 models	62,702	37,060	49,606	49,625	62,459	65,705	63,435	65,470	65,462

Notes: Robust standard errors are shown in parentheses and clustering is assumed by state. Coefficients that are significantly different from zero at the 10% level are underlined. Coefficients that are significantly different from zero at the 5% level are displayed in bold. Coefficients that are significantly different from zero at the 1% level are both underlined and displayed in bold. We want to emphasize that "significantly different from zero" is only of interest for the analysis of the "single dummy variable model." In the other two models, what is interesting is whether the coefficients are significantly different from each other.

Table 3b reestimates the regressions along the lines of the dataset compiled by Wentong Zheng.⁴¹ The main difference is that we reduce the demographic variables to four: the percentage of the population that is black, and the percent of the population that is ten to nineteen, twenty to twenty-nine, and thirty to thirty-nine. Like Ayres and Donohue and our earlier estimates, we continue to use population, per capita income, state-level unemployment rates, and poverty rates. The other measures of income used earlier are dropped from this analysis. Despite our theoretical concerns discussed later, we now include the once-lagged per capita prison population. We did not have time to extend our data on the number of sworn police officers by county, so we continued to use the arrest rate as a measure of police effectiveness. We could not obtain a county-level measure of per capita alcohol consumption, so we did not include that variable. The fixed effects are the same as we used previously.

Given those alterations, the estimated change in before-and-after trends for both the spline and hybrid models remain extremely similar to those reported in Table 3a. The change in trends for both the spline and hybrid models is slightly smaller for murder, but a little bigger for rape, robbery, and aggravated assault. However, despite the point estimates remaining virtually unchanged, the significance levels are reduced. For the spline results, the change in trends is still statistically significant for murder, rape, and robbery at least at the ten-percent level. For the hybrid model, only the rape- and robbery-rate trend changes are significant at about or better than the ten-percent level. For murder, the joint *F*-test for both the postpassage dummy and the postpassage trend is statistically significant at least at the ten-percent level.⁴²

41. Bartley & Cohen, *supra* note 1, found that for the full sample, all their combinations of control variables indicated that right-to-carry laws caused the murder, rape, and robbery crime rate trends to fall.

42. The *F*-statistic equals 2.5 with a probability of 8.2%.

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TABLE 3B: LIMITED SET OF DEMOGRAPHICS AND LAGGED PER CAPITA PRISON POPULATION, 1977-2000 DATA

	Violent crime	Murder	Rape	Robbery	Aggravated Assault	Property crimes	Auto Theft	Burglary	Larceny
Single dummy variable model									
Post-passage dummy	-2.8%	<u>-6.8%</u>	-3.2%	-4.1%	-1.9%	4.8%	9.7%	0.4%	6.4%
	(4.0%)	(3.6%)	(3.7%)	(3.3%)	(4.7%)	(2.9%)	(4.0%)	(2.6%)	(3.1%)
R^2	0.86	0.81	0.75	0.92	0.81	0.82	0.88	0.81	0.80
Spline model									
Trend before	0.4%	0.3%	1.1%	0.9%	0.9%	0.9%	<u>1.0%</u>	<u>1.1%</u>	1.3%
	(0.5%)	(0.7%)	(0.5%)	(0.7%)	(0.7%)	(0.4%)	(0.5%)	(0.6%)	(0.6%)
Trend after	-0.6%	-1.9%	<u>-1.9%</u>	<u>-1.9%</u>	-1.2%	0.9%	1.0%	-1.0%	0.4%
	(0.7%)	(1.3%)	(1.0%)	(1.1%)	(1.1%)	(0.6%)	(0.7%)	(0.9%)	(1.2%)
R^2	0.86	0.81	0.76	0.92	0.81	0.82	0.88	0.81	0.80
Difference between trends	-0.0095	-0.0222	-0.0304	-0.0284	-0.0211	0.0003	0.0006	-0.0209	-0.0099
F -test statistic	1.50	2.69	5.12	3.31	2.34	0.00	0.00	2.36	0.38
Prob > F	22.0%	10.0%	2.4%	6.9%	12.59%	97.8%	95.6%	12.48%	53.67%
Hybrid model									
Trend before	0.5%	0.4%	1.1%	0.9%	0.9%	1.0%	0.8%	<u>1.0%</u>	1.2%
	(0.5%)	(0.6%)	(0.4%)	(0.6%)	(0.6%)	(0.4%)	(0.5%)	(0.6%)	(0.5%)
Post-passage dummy	-3.6%	-1.4%	1.8%	-1.1%	0.4%	-2.2%	6.3%	2.3%	2.9%
	(3.1%)	(4.3%)	(4.3%)	(5.9%)	(5.5%)	(2.6%)	(7.3%)	(5.2%)	(3.5%)
Trend after	-0.2%	-1.7%	-2.1%	-2.0%	-1.2%	<u>1.2%</u>	0.4%	-1.3%	0.0%
	(0.8%)	(1.6%)	(1.3%)	(1.5%)	(1.5%)	(0.7%)	(1.2%)	(1.3%)	(1.5%)
R^2	0.86	0.81	0.76	0.92	0.81	0.82	0.88	0.81	0.80
Difference between trends	-0.0068	-0.0211	-0.0317	-0.0291	-0.0213	0.0019	-0.0040	-0.0225	-0.0120
F -test statistic	0.64	1.08	4.25	2.62	1.70	0.04	0.08	2.03	0.43
Prob > F	42.3%	29.89%	3.9%	10.5%	19.3%	83.6%	77.1%	15.5%	51.1%
Number of observations in all 3 models	60,073	35,392	47,642	47,506	59,896	62,971	60,792	62,743	62,748

Notes: Robust standard errors are shown in parentheses and clustering is assumed by state. Coefficients that are significantly different from zero at the 10% level are underlined. Coefficients that are significantly different from zero at the 5% level are displayed in bold. Coefficients that are significantly different from zero at the 1% level are both underlined and displayed in bold. We want to emphasize that "significantly different from zero" is only of interest for the analysis of the "single dummy variable model." In the other two models, what is interesting is whether the coefficients are significantly different from each other.

One hypothesis advanced by Ayres and Donohue is that the impact of the law is different for early- and late-adopting states. They argue that the states first studied in the original sample from 1977 to 1992 produced much larger reductions in violent crime than those that came afterwards. To test this, Table 4 shows the before-and-after trends separately for those two groups of states. It turns out that there is nothing to support this hypothesis. The difference between the before-and-after trends is almost identical for the two groups of states. For murder, early adopters found their murder rates declining 2.2% faster after the law, while late adopters had a 2.4% faster decline. For rapes, early adopters saw a decline of 2.9% and late adopters a decline of 3.1%. For robberies, the difference was a little larger, but again in the opposite direction of what Ayres and Donohue note. Early adopters experienced a 2.1% faster annual decline and late adopters a 3% annual decline. Indeed, in all cases, violent crimes fell slightly faster for late adopting states. None of the differences are statistically significantly different from each other.

Overall, the results in Tables 3a through 4 imply that the costs of crime fall by between \$2.6 and \$3.1 billion per year for the first five years of the law. As to Ayres and Donohue's claims of model misspecification, these results provide no evidence of "positive main effects."⁴³ There is no evidence that "crime was substantially higher than the regression model would have otherwise predicted."⁴⁴

43. Donohue, *supra* note 7, at 301; *see also* Ayres & Donohue, *supra* note 5, at 1224, 1268.

44. Ayres & Donohue, *supra* note 5, at 1255.

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TABLE 4: EARLY AND LATE ADOPTERS, FULL SET OF DEMOGRAPHICS, 1977-2000 DATA

	Violent crime	Murder	Rape	Robbery	Aggravated Assault	Property crimes	Auto Theft	Burglary	Larceny
Early adopters									
Trend before	-0.6%	0.3%	0.5%	0.3%	-0.9%	0.8%	0.8%	<u>1.0%</u>	<u>1.2%</u>
	(0.8%)	(0.7%)	(0.5%)	(1.1%)	(1.0%)	(0.5%)	(0.8%)	(0.6%)	(0.6%)
Trend after	0.1%	-1.9%	<u>-2.3%</u>	-1.7%	-0.3%	0.7%	0.9%	-1.4%	0.0%
	(1.0%)	(1.1%)	(0.9%)	(1.1%)	(1.3%)	(0.5%)	(0.7%)	(0.9%)	(0.9%)
Late adopters									
Trend before	0.3%	0.5%	0.6%	0.8%	0.8%	0.8%	<u>1.1%</u>	0.6%	1.0%
	(0.4%)	(0.5%)	(0.5%)	(0.7%)	(0.6%)	(0.4%)	(0.6%)	(0.6%)	(0.4%)
Trend after	0.3%	-2.0%	<u>-2.5%</u>	-2.2%	0.6%	1.0%	1.6%	0.6%	0.6%
	(2.5%)	(1.9%)	(1.5%)	(1.8%)	(3.0%)	(1.5%)	(2.1%)	(1.4%)	(1.6%)
Number of observations	62,730	37,080	49,633	49,651	62,487	65,733	63,463	65,498	65,490
R ²	0.86	0.81	0.76	0.92	0.81	0.82	0.85	0.81	0.80
Early adopters									
Difference between trends	0.0071	-0.0221	-0.0286	-0.0205	0.0058	-0.0015	0.0003	-0.0237	-0.0114
F-test statistic	0.22	1.69	5.53	0.94	0.08	0.03	0.00	3.08	0.65
Prob > F	63.7%	9.7%	1.9%	33.3%	77.7%	87.4%	98.3%	7.9%	42.2%
Late adopters									
Difference between trends	-0.0003	-0.0243	-0.0314	-0.0301	-0.0023	0.0027	0.0055	0.0002	-0.0036
F-test statistic	0.00	1.30	3.10	2.64	0.00	0.03	0.05	0.00	0.04
Prob > F	99.1%	25.4%	7.8%	10.1%	94.1%	87.2%	82.5%	99.1%	84.9%

Notes: Early adopters are states that adopted the right-to-carry laws between 1977 and 1992; late adopters enacted the law after 1992. Robust standard errors are shown in parentheses and clustering is assumed by state. The same set of control variables are used as in Table 3a. Coefficients that are significantly different from zero at the 0.10 level are underlined. Coefficients that are significantly different from zero at the 0.05 level are displayed in bold. Coefficients that are significantly different from zero at the 0.01 level are both underlined and displayed in bold.

C. *Is the Adoption of a Right-to-Carry Law Endogenous?*

The least squares estimates of the four dummy models suggest that, for the five violent crime categories and for burglary, the adoption of a right-to-carry law reverses an upward trend in crime rates. A possible interpretation of this finding is that right-to-carry laws have generally been adopted in response to unusual increases in crime, and that the drop in crime after the law simply represents a reversion to the mean. If the adoption of a right-to-carry law is endogenous (that is, if there is a two-way causal relationship between crimes and the adoption of the law) then standard least squares methods yield inconsistent estimates of the dummy coefficients.⁴⁵

Probably the simplest test whether there are abnormal increases in crime during the years immediately before the adoption of a right-to-carry law that drive the results is to exclude these years from the analysis. To eliminate the possibility that the preadoption trend is driven by increases in the crime rate right before the adoption of the law, we excluded observations of the two years immediately before the adoption of the law as well as the very year of the adoption. The upper part of Table 5 shows the estimation results for the spline model of the reduced dataset, and the *F*-test probabilities that the before- and after-adoption trends differ from each other. The probabilities are only slightly larger than the corresponding *F*-test probabilities in Table 4a, which indicates that the differences in trends are not the result of abnormal increases in crime rates in the years immediately before the adoption of a right-to-carry law.⁴⁶

45. A standard solution to this endogeneity problem is to use a so-called "instrumental variable estimator." This estimator requires that the instruments (additional right-hand side variables) are correlated with the right-hand side endogenous variable (in this case, the right-to-carry dummy variable) but not correlated with the other right-hand side variables. Since it may be difficult to find such instruments, this method might not provide a practical solution to the endogeneity problem. Ayres and Donohue, *supra* note 5, at 1255-57, argue that no good instruments are available in this case, though they claimed that they were unable to obtain all the instruments used previously and decided to replicate the previous results using state- rather than county-level data. The NRA membership data has always been readily available to academics who have been willing to agree not to give out the data to others and not to report the data in such a way that it is possible to discern the membership rate within a particular state.

46. We also tried dropping out three years prior to the passage of the law, as well as the year of passage, and obtained very similar estimates. The *F*-statistics for the change in trends for murder, rape, and robbery all remained statistically significant at the 10% or better level.

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TABLE 5: TEST WHETHER THE ADOPTION OF SHALL-ISSUE LAWS IS ENDOGENOUS TO THE CRIME RATE

	Violent crime	Murder	Rape	Robbery	Aggravated Assault	Property crimes	Auto Theft	Burglary	Larceny
Model that excludes observations from 2 years and 1 year before and after the adoption of a shall-issue law, and the year of the adoption									
Trend before	-0.1%	0.2%	0.4%	0.3%	0.3%	0.8%	0.8%	0.8%	1.0%
	(0.4%)	(0.5%)	(0.5%)	(0.8%)	(0.6%)	(0.3%)	(0.6%)	(0.5%)	(0.4%)
Trend after	-0.2%	-1.7%	-2.1%	-1.3%	-1.0%	0.7%	<u>1.0%</u>	-0.9%	0.3%
	(0.8%)	(0.7%)	(0.8%)	(0.8%)	(1.1%)	(0.6%)	(0.6%)	(0.7%)	(0.8%)
Number of observations	55,766	32,612	43,842	44,034	55,613	58,639	56,599	58,422	58,421
R ²	0.86	0.81	0.76	0.91	0.81	0.81	0.84	0.81	0.80
Difference between trends	-0.0012	-0.0190	-0.0253	-0.0153	-0.0132	-0.0014	0.0022	-0.0169	-0.0068
F-test statistic	0.02	3.37	5.08	2.66	1.08	0.03	0.05	2.20	0.43
Prob > F	87.9%	6.7%	2.4%	10.2%	29.9%	85.5%	81.5%	13.8%	51.3%
Model that excludes observations from 3 years, 2 years, and 1 year before and after the adoption of a shall-issue law, and the year of the adoption									
Trend before	-0.0%	0.2%	0.4%	0.3%	0.3%	1.0%	0.9%	<u>1.0%</u>	-0.0%
	(0.5%)	(0.6%)	(0.5%)	(0.8%)	(0.7%)	(0.4%)	(0.7%)	(0.5%)	(0.5%)
Trend after	-0.2%	-1.6%	-2.1%	-1.2%	-1.1%	0.6%	0.9%	-1.1%	-0.2%
	(0.8%)	(0.8%)	(0.8%)	(0.8%)	(1.1%)	(0.6%)	(0.7%)	(0.8%)	(0.8%)
Number of observations	52,955	30,809	41,524	41,745	52,836	55,778	53,835	55,567	55,572
R ²	0.86	0.82	0.77	0.91	0.81	0.81	0.84	0.81	0.79
Difference between trends	-0.0008	-0.0178	-0.0253	-0.0147	-0.0146	-0.0043	0.00001	-0.0207	-0.01147
F-test statistic	0.01	2.48	4.91	2.36	0.93	0.30	0.00	3.27	1.09
Prob > F	93.1%	11.6%	2.7%	12.6%	33.4%	58.1%	99.9%	7.2%	29.6%

Notes: Robust standard errors are shown in parentheses and clustering is assumed by state. The same set of control variables are used as in Table 3a. Coefficients that are significantly different from zero at the 10% level are underlined. Coefficients that are significantly different from zero at the 5% level are displayed in bold. Coefficients that are significantly different from zero at the 1% level are both underlined and displayed in bold.

The exclusion of the immediate years before the adoption would have helped to correct an inflated upward trend, but it might also be necessary to correct an inflated downward trend that would be the result of a simple reversion to the mean. To test this possibility, we repeated the analysis without observations from the adoption year, the two years immediately before, and the two years after the adoption. The *F*-test probabilities in the lower part of Table 5 are again very similar to those of the complete model in Table 4a. This exercise suggests that it is unlikely that right-to-carry laws have generally been adopted at a time when crime rates have peaked, and that it is more likely that the decreases in the crime rates of murders, rapes, robberies, aggravated assaults, and burglaries are the result of the laws.

A reversion to the mean also seems to be ruled out by Figures 4a through 4e (or even, for that matter, the county-level estimates by Ayres and Donohue presented in Figures 2a through 2c), simply because the crime rates are not returning to their prelaw lows, but are actually going well below those values. This is not simply a reversion to normal prelaw levels. Economists have looked at a wide range of gun laws such as one-gun-a-month, assault weapons bans, background checks, and waiting periods. Yet they have not found any evidence of these laws reducing violent crime rates. If there is a reversion to the mean, the question is why would it only affect right-to-carry laws? The results here provide a possible explanation: There is no simple reversion to the mean as a result of an unusual event that is occurring prior to the adoption of right-to-carry laws.

Table 6 shows the state-by-state breakdown of the results for the county-level data using the spline estimates (Appendix Table 4 uses the state-level data). Sixty percent of the states show a drop in crime after the law using county-level data and sixty-three percent show a drop with state-level data. Even more importantly, breaking down the state-by-state results on a year-by-year basis also produces similar results.

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TABLE 6: STATE-BY-STATE BREAKDOWN: SPLINE MODEL, 1977-2000
COUNTY-LEVEL DATA, WITH REGIONAL YEAR FIXED EFFECTS

	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny
Maine	11.5% (0.0%)	-0.8% (70.7%)	6.7% (0.7%)	12.1% (0.0%)	3.7% (42.4%)	-5.3% (1.8%)	1.5% (42.1%)
Florida	-6.5% (0.0%)	3.6% (0.2%)	9.1% (0.0%)	5.0% (74.8%)	6.9% (0.0%)	5.0% (0.1%)	4.2% (0.0%)
Virginia	-3.4% (2.6%)	-0.3% (84.0%)	-0.2% (91.5%)	-0.6% (13.9%)	-3.2% (11.4%)	1.6% (32.4%)	0.0% (98.5%)
Georgia	-3.5% (4.1%)	-1.2% (53.3%)	-3.8% (13.2%)	-3.1% (13.5%)	-4.2% (9.0%)	-0.2% (93.3%)	-0.4% (80.1%)
Pennsylvania	-3.2% (1.6%)	1.0% (8.9%)	-4.4% (2.2%)	-2.3% (0.0%)	-0.4% (83.5%)	-0.1% (92.0%)	0.0% (99.2%)
West Virginia	2.1% (17.5%)	-4.7% (0.9%)	-4.1% (9.4%)	-11.5% (1.2%)	-2.2% (42.5%)	-3.5% (9.5%)	-2.2% (31.0%)
Idaho	-2.4% (19.8%)	-4.4% (0.0%)	-9.8% (0.0%)	-4.4% (0.0%)	-1.8% (38.1%)	-1.7% (30.4%)	-0.3% (80.6%)
Mississippi	-5.2% (0.0%)	-5.6% (0.1%)	-8.8% (0.0%)	-8.3% (0.0%)	-9.5% (0.0%)	-5.9% (0.1%)	-4.8% (0.4%)
Oregon	6.1% (0.0%)	0.6% (71.4%)	2.3% (3.0%)	-11.2% (0.0%)	4.3% (7.9%)	3.7% (0.1%)	-0.1% (87.0%)
Montana	-6.0% (8.2%)	-19.3% (0.0%)	-38.7% (0.0%)	-51.0% (0.0%)	-18.4% (0.0%)	-7.1% (0.1%)	-13.5% (0.0%)
Alaska	16.5% (0.0%)	0.0% (99.8%)	5.3% (4.2%)	10.4% (0.8%)	-9.9% (1.6%)	-7.7% (0.1%)	1.1% (64.9%)
Arizona	1.2% (39.1%)	-6.0% (0.0%)	5.7% (1.6%)	5.7% (69.6%)	1.0% (0.0%)	11.6% (1.7%)	3.7% (0.0%)
Tennessee	5.3% (0.0%)	1.8% (31.5%)	-2.8% (12.2%)	10.1% (0.0%)	1.6% (29.7%)	-0.2% (89.5%)	1.3% (47.3%)
Wyoming	-1.4% (50.3%)	-7.8% (0.1%)	-8.8% (1.8%)	3.3% (29.7%)	-15.5% (0.0%)	1.6% (49.0%)	-0.4% (83.0%)
Arkansas	6.5% (0.0%)	2.8% (16.2%)	-0.2% (90.6%)	7.4% (1.1%)	6.0% (0.4%)	4.8% (1.1%)	1.3% (54.2%)
Nevada	-2.4% (27.2%)	-1.2% (52.0%)	0.2% (95.8%)	-7.9% (0.2%)	5.8% (17.4%)	0.7% (78.4%)	1.9% (27.2%)
North Carolina	8.6% (0.0%)	7.0% (0.1%)	21.4% (0.0%)	-7.5% (0.7%)	10.2% (0.0%)	15.0% (0.0%)	6.8% (0.1%)
Oklahoma	3.6% (12.6%)	-0.4% (85.7%)	-11.0% (0.0%)	2.0% (49.3%)	-9.4% (0.0%)	-3.2% (11.1%)	-2.5% (27.8%)
Texas	-5.6% (0.6%)	-4.2% (4.4%)	-2.1% (24.3%)	3.1% (31.7%)	-0.3% (86.9%)	-3.1% (10.9%)	-1.0% (62.9%)
Utah	-0.7% (82.6%)	11.8% (0.0%)	-1.9% (71.5%)	-4.3% (25.0%)	-7.2% (16.1%)	-0.8% (81.5%)	-1.0% (71.3%)
Kentucky	-0.1% (94.5%)	-15.3% (0.0%)	-13.0% (0.0%)	8.5% (3.9%)	-14.8% (0.0%)	-11.0% (0.0%)	-17.3% (0.0%)
Louisiana	-0.2% (93.7%)	-8.7% (0.1%)	1.9% (40.9%)	-4.8% (18.8%)	3.2% (30.3%)	2.9% (26.3%)	-1.8% (45.0%)
South Carolina	-4.2% (3.4%)	4.7% (6.2%)	15.6% (0.0%)	-2.8% (39.6%)	5.7% (5.6%)	-0.8% (73.7%)	0.1% (97.4%)
Number of observations	37,080	49,633	49,651	62,487	63,463	65,498	65,490
R ²	0.81	0.76	0.91	0.81	0.85	0.81	0.80
Number significantly positive	6	5	7	5	6	5	3
Number insignificantly positive	3	4	2	5	4	4	8
Number significantly negative	8	9	8	8	7	6	3
Number insignificantly negative	6	5	6	5	6	8	9

Notes: The numbers are the differences between the pre-adoption and post-adoption trend dummies. The values in parentheses are the F-test probabilities that the preadoption and postadoption trends are identical. Differences that are significant at the 10% level are underlined. Differences that are significant at the 5% level are displayed in bold. Differences that are significant at the 1% level are both underlined and displayed in bold.

D. Poisson Estimates

A major problem with county-level data is what one should do with all the observations that have zero crime rates. Including arrest rates creates the problem of eliminating observations whenever a county's crime rate is zero. This occurs because the arrest rate is defined as the number of arrests divided by the crime rate, and it is not possible to divide by zero. On the other hand, with weighted least squares, omitting the arrest rate is not a useful suggestion either. Including all counties with zero crime rates will bias the estimated benefit of the concealed handgun law towards finding an increase in crime, because no matter how good the law is, it cannot lower the crime rate below zero. Although the crime rate cannot fall in those counties, there will be some occasions, even due to pure randomness, where the crime rate rises. This problem occurs with Ayres and Donohue's table 1 estimates.

This problem is most pronounced for murder, because about half of the counties had zero murders in 2000. Twenty-seven percent of the counties had no robberies that year and twenty-one percent had no rapes. Yet, virtually all counties experienced at least one aggravated assault and thus at least one violent crime. The standard approach to analyzing data with this kind of distribution is to use a Poisson regression model. We reestimated our earlier results for murder, rape, and robbery using the Poisson procedure. We also replaced the arrest rate for an individual crime with the arrest rate for all violent crimes to ensure that we did not have to eliminate all observations of zero crime.⁴⁷ Unfortunately, even this is a problem since STATA does not support the calculation of robust standard errors or clustering by state in its routine that absorbs the fixed effects (*xtpois*), and STATA's maximum likelihood routine (*poisson*) failed to converge when we incorporated the more than 3000 county dummies manually.

All results reported in Tables 7 and 8 are similar to those that we reported using weighted least squares. The year-by-year estimates in Table 7 show declines in all three crimes after the right-to-carry law was enacted. Between the year of passage and the eighth year after the law, murder rates fall by almost 17%, rape rates by 6.4%, and robbery rates by almost 16%. In only two cases is the crime rate after passage higher than the crime rate when the law was passed (the first year after the law for rape and the third year after the law for robbery).

47. While the correlation between the arrest rate for violent crime and the arrest rates for murder, rape, and robbery is below 50%, including the arrest rate for violent crime allows at least some county-level measure of law enforcement activity and permits us to include virtually all the counties in the United States.

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TABLE 7: POISSON ESTIMATES:
ALL SOCIOECONOMIC VARIABLES AND THE ARREST RATE OF VIOLENT CRIME

	Murder	Rape	Robbery
Before (-10)	<u>-20.5%</u> (2.9%)	3.6% (1.7%)	<u>2.5%</u> (0.9%)
Before (-9)	<u>-22.4%</u> (3.0%)	4.4% (1.7%)	<u>7.5%</u> (0.9%)
Before (-8)	<u>-24.1%</u> (3.0%)	3.5% (1.7%)	<u>8.6%</u> (0.9%)
Before (-7)	<u>-23.2%</u> (3.0%)	4.4% (1.7%)	<u>14.8%</u> (0.9%)
Before (-6)	<u>-24.0%</u> (3.0%)	1.4% (1.7%)	<u>9.1%</u> (0.9%)
Before (-5)	<u>-20.9%</u> (3.0%)	3.8% (1.7%)	<u>6.4%</u> (0.9%)
Before (-4)	<u>-18.5%</u> (3.0%)	<u>2.9%</u> (1.7%)	<u>8.0%</u> (0.9%)
Before (-3)	<u>-19.7%</u> (3.0%)	1.3% (1.7%)	<u>6.8%</u> (0.9%)
Before (-2)	<u>-20.6%</u> (3.1%)	<u>9.5%</u> (1.7%)	<u>8.9%</u> (0.9%)
Before (-1)	<u>-21.3%</u> (3.1%)	<u>6.2%</u> (1.7%)	<u>12.2%</u> (0.9%)
Zero	<u>-18.7%</u> (3.1%)	4.4% (1.7%)	<u>12.7%</u> (0.9%)
After (1)	<u>-22.9%</u> (3.2%)	<u>6.0%</u> (1.8%)	<u>7.6%</u> (0.9%)
After (2)	<u>-21.8%</u> (3.2%)	2.4% (1.8%)	<u>10.7%</u> (0.9%)
After (3)	<u>-19.9%</u> (3.2%)	2.4% (1.8%)	<u>13.3%</u> (0.9%)
After (4)	<u>-27.2%</u> (3.2%)	<u>-3.5%</u> (1.8%)	<u>5.6%</u> (0.9%)
After (5)	<u>-29.2%</u> (3.3%)	-1.6% (1.8%)	<u>8.2%</u> (0.9%)
After (6)	<u>-29.4%</u> (3.5%)	4.0% (1.9%)	<u>6.5%</u> (1.0%)
After (7)	<u>-33.2%</u> (3.7%)	0.2% (1.9%)	<u>7.5%</u> (1.0%)
After (8)	<u>-35.4%</u> (3.8%)	-2.0% (2.0%)	<u>3.0%</u> (1.0%)
Number of observations	61,219	62,446	62,023
LogLikelihood	-93,040	-192,545	-439,305

Notes: Nonrobust standard errors are shown in parentheses. Coefficients that are significantly different from zero at the 10% level are underlined. Coefficients that are significantly different from zero at the 5% level are displayed in bold. Coefficients that are significantly different from zero at the 1% level are both underlined and displayed in bold.

TABLE 8: POISSON ESTIMATES:
ALL SOCIOECONOMIC VARIABLES AND THE ARREST RATE OF VIOLENT CRIME

	Murder	Rape	Robbery
Single dummy variable model			
Post-passage dummy	<u>-4.5%</u> (0.8%)	<u>-3.5%</u> (0.4%)	<u>-1.2%</u> (0.2%)
LogLikelihood	-93,147	-192,847	-441,442
Spline model			
Before trend	<u>0.2%</u> (0.1%)	<u>0.2%</u> (0.1%)	<u>0.5%</u> (0.0%)
After trend	<u>-1.2%</u> (0.2%)	<u>-0.9%</u> (0.1%)	<u>-0.5%</u> (0.0%)
LogLikelihood	-93,128	-192,810	-441,123
Difference between trends	-0.0145	-0.0103	-0.01
χ^2 test statistic	64.79	148.84	586.95
Prob > χ^2	0.0%	0.0%	0.0%
Hybrid model			
Before trend	<u>0.3%</u> (0.1%)	<u>0.2%</u> (0.1%)	<u>0.5%</u> (0.0%)
Post-passage dummy	-1.3% (1.1%)	<u>-1.6%</u> (0.5%)	<u>-0.8%</u> (0.2%)
After trend	<u>-1.1%</u> (0.2%)	<u>-0.7%</u> (0.1%)	<u>-0.4%</u> (0.1%)
LogLikelihood	-93,127	-192,805	-441,117
Difference between trends	-0.0131	-0.009	-0.01
χ^2 test statistic	38.21	80.26	373.16
Prob > χ^2	0.0%	0.0%	0.0%
Number of observations in all 3 models	61,219	62,446	62,023

Notes: Nonrobust standard errors are shown in parentheses. Coefficients that are significantly different from zero at the 10% level are underlined. Coefficients that are significantly different from zero at the 5% level are displayed in bold. Coefficients that are significantly different from zero at the 1% level are both underlined and displayed in bold.

The spline and hybrid estimates imply similar but smaller effects than previously reported (Table 8). Crime rates are rising before the law and falling afterwards. To the extent that there is a postpassage dummy effect, it implies that the crime rate fell by more than would be indicated by the simple trend, though the intercept shift makes the change in trends smaller. For both the spline and hybrid regressions, murder declines at an annual rate of about 1.3% to 1.5%, and rape and robbery decline at an annual rate of about 1%.

III. EVALUATING SOME GENERAL CLAIMS MADE BY AYRES AND DONOHUE

A. *Has Previous Work Acknowledged Both the Costs and Benefits of Guns?*

[Lott and Mustard] never acknowledge cases on the other side of the ledger where the presence of guns almost certainly led to killings.⁴⁸

For example, the nightmare scenario for those asserting the value of defensive use of guns is not mentioned: the case of the Japanese exchange student, Yoshihiro Hattori, who was on his way to a Halloween party in October 1992 when he mistakenly approached the wrong house and was shot to death by the homeowner, Rodney Peairs.⁴⁹

This is Ayres and Donohue's first criticism and they frequently revisit the claim that Lott and Mustard ignore the costs of guns. Yet Lott reports exactly this story about the Japanese college student on the first page of his book and refers to it as showing "how defensive gun use can go tragically wrong."⁵⁰ Lott and Mustard's original paper also describes this same incident on page two.⁵¹ Lott's book also explicitly notes many gun crimes and repeatedly discusses how one must analyze the "net effect" of guns.⁵²

48. Ayres & Donohue, *supra* note 5, at 1200.

49. *Id.* at 1200 n.12.

50. LOTT, *supra* note 6, at 1.

51. Ayres and Donohue's discussion also conveniently leaves out that the homeowner who killed the student, while found civilly liable, was acquitted of manslaughter because he believed that he was acting in self defense. The gun was fired only after the Japanese man continued moving towards the homeowner after being yelled at to "freeze." See Leslie Zganjar, *Homeowner Who Killed Japanese Student Feared for Life*, ASSOCIATED PRESS, Sept. 13, 1994.

52. See, e.g., LOTT, *supra* note 6, at 194-95, 223; see also John R. Lott, Jr. & William R. Landes, Multiple Victim Public Shootings 1 (Univ. of Chi., Working Paper, 2000) (listing many examples of multiple victim public shootings), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=272929#Paper%20Download.

B. *Possible Bad Effects of Concealed Handgun Laws*

Ayres and Donohue raise questions of how concealed handguns could contribute to crime. But this concern is far from new and they ignore existing empirical evidence.

First, even if the adoption of a shall-issue law increased the riskiness of criminal activity and thereby dampened the number of criminals, it might also *increase* the number of criminals who decided to carry weapons . . . and also might *increase* the speed at which a criminal decides to shoot or disable potential victims (as the presence of armed victims increases the cost of hesitation once a criminal engagement has been launched). Therefore, the number of murders and aggravated assaults can rise if criminals respond to shall-issue laws by packing more heat and shooting quicker.⁵³

True, the rate at which criminals are carrying guns is not known. Nonetheless, whether more murders are committed with guns after right-to-carry laws are passed has indeed been evaluated. David Olson and Michael Maltz use county-level data from the *Supplemental Homicide Report* to show that the entire drop in homicides after right-to-carry laws are adopted is due to a decline in firearm homicides.⁵⁴ They claim that while overall homicides declined by 6.5%, firearm homicides fell by twenty-one percent. (Nonfirearm homicides actually rose by 9.8%, though this increase was not statistically significant.) Earlier work by Lott and Mustard using state-level data also found that firearm homicides declined by more than other homicides, but the difference was not statistically significant.⁵⁵ No statistical evidence supports Ayres and Donohue's concern. "This [example] suggests that those who are able to secure handgun permits are not always model citizens"⁵⁶

True, yet the rate at which permits are incorrectly provided is extremely low and mistakes are usually quickly corrected. In Florida, for example, 820,759 permits were issued between October 1, 1987 and October 31, 2002, and only 477 (a rate of 0.058%) were later revoked for a crime prior to licensure.⁵⁷ Revoked licenses for any type of violation after licensure are also very rare: Over the last twenty-five years, the permanent revocation rate for any reason, usually nothing that involves the gun, was only 0.14%.⁵⁸ Similar information for other states can be found in Lott's book.⁵⁹ "[P]utting more guns in the hands of the law-abiding population necessarily means that more

53. Ayres & Donohue, *supra* note 5, at 1204.

54. See Olson & Maltz, *supra* note 1.

55. See John R. Lott, Jr. & David B. Mustard, *Crime, Deterrence, and Right-to-Carry Concealed Handguns*, 26 J. LEGAL STUD. 1, 1-68 (1997).

56. Ayres & Donohue, *supra* note 5, at 1205.

57. The most current numbers can be obtained from <http://licgweb.doacs.state.fl.us/news/reports.html>.

58. These are revocations net of reinstatements for Florida.

59. LOTT, *supra* note 6, at 219-22.

guns will end up in the hands of criminals. . . . [T]he presence of more guns can actually serve as a stimulus to burglary and theft."⁶⁰

From what information is available, the vast majority of permit holders appear to have already owned a gun prior to getting a permit. A Texas poll showed that ninety-seven percent of first-time applicants for concealed-handgun permits already owned a handgun.⁶¹ While this appears to be higher than experienced in other states, any comparable rates imply that right-to-carry laws do not result in appreciably higher rates of homes with guns.

While a gun in a home might make burglary more attractive, it also makes the crime riskier for criminals. State-level survey data on gun ownership imply that states with the biggest increases in gun ownership have seen the biggest relative drops in burglary.⁶² Using magazine sales data as a proxy for gun ownership produces generally similar results. Of the seven largest gun magazines, only one (*Guns & Ammo*) implies any positive relationship between magazine sales and burglaries. Even then the evidence is mixed, with sales two years preceding the crime positively related to burglaries, but sales one year prior to the crime negatively and insignificantly related.⁶³

C. *Other Concerns: The Risks to Police, Accidents, and the Problem with Untrained Permit Holders*⁶⁴

On the risks to police, David Mustard finds that police officers are murdered at a lower rate after concealed handgun laws are passed, and that the longer the laws are in effect, the greater the decline.⁶⁵ The Olson and Maltz evidence mentioned earlier implies fewer criminals carrying guns.

As for accidental gun deaths and gun suicides, two studies have examined these issues. Both studies find that the passage of right-to-carry laws does not affect either death rate. That result holds when examining all people as well as those under age twenty.⁶⁶ While the Lott and Landes study finds that a few

60. Ayres & Donohue, *supra* note 5, at 1205 (footnote omitted).

61. *NRA Poll: Salespeople No. 1 for Permit Applications*, DALLAS MORNING NEWS, Apr. 19, 1996, at 32A.

62. LOTT, *supra* note 6, at 114.

63. See JOHN R. LOTT, JR., *THE BIAS AGAINST GUNS* app. 2 (2003).

64. See Ayres & Donohue, *supra* note 5, at 1203-05.

65. Mustard, *supra* note 1. Ayres and Donohue mention Mustard's paper in the context of a case where a permit holder came to the aid of a police officer in Arizona, but they do not discuss Mustard's general results. Ayres & Donohue, *supra* note 5, at 1288 n.116.

66. See LOTT, *supra* note 6, at 110-13; Lott & Whitley, *supra* note 1; John R. Lott, Jr. & John E. Whitley, *Safe-Storage Gun Laws: Accidental Deaths, Suicides, and Crime* (Yale Law Sch., Working Paper, 1999), available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=228534#Paper%20Download.

hours of training result in a greater reduction in multiple-victim public shootings, the other Lott research finds no benefits for other types of crime.⁶⁷

D. *Are There “Initial Jumps in Crime”?*

In sum, the foundation of the Lott thesis essentially is captured in regressions 1 (dummy variable model) and 2 (spline model) of Table 10. . . . Importantly, both the dummy variable and spline models are essentially rejected by the data by virtue of the large and statistically significant positive effects on both terms in the hybrid models (Table 10, lines 3 and 6)—particularly for the full dataset. But the hybrid model’s prediction of initial jumps in crime followed by subsequent declines in response to the adoption of a shall-issue law raises our concern about model misspecification⁶⁸

Let us first address Ayres and Donohue’s claim that the hybrid estimates in their table 10 show “initial jumps in crime.” In addition to the problems discussed in Part I of fitting a straight line to a nonlinear shape, virtually none of these initial increases are actually statistically significant. The confusion apparently arises because Ayres and Donohue concentrate solely on the significance of the postpassage dummy itself. For example, take their hybrid estimates for murder using the 1977 to 1997 sample in line six of their table 10. The postpassage dummy for murder equals 6.9% and is statistically significant at the 5% level. However, for the first year that the law is in effect, the net effect on the crime rate is the sum of the 6.9% dummy plus the –3.5% postlaw crime trend. The net effect in the first year is 3.4%, but with a standard error of 2.98, which is less than 1.2 standard deviations from zero. In fact, none of Ayres and Donohue’s hybrid estimates for murder, rape, or robbery in their tables 10 and 11 imply a net effect that is much more than one standard deviation away from zero. This is not particularly surprising since even the earliest nonlinear results provided by Lott found this pattern for aggravated assaults.⁶⁹

As our work in Part II has shown, Ayres and Donohue’s claim about the “initial jump in crime” is very sensitive to the timeframe chosen. In none of the specifications examining the 1977 to 2000 data is the postpassage dummy statistically significant, and for overall violent crime, murder, and robbery, the estimate even becomes negative. In addition, since all the estimates reported in Part II indicate that the trend in violent crime rates fell after the adoption of the law, the net effect during the first year after the law is negative and statistically significant for murder, rape, and robbery.

As to the claim that a dummy variable or a linear trend will “essentially” capture Lott’s thesis (or even a dummy variable with a quadratic trend), in Part

67. LOTT, *supra* note 6, at 176-81; Lott & Landes, *supra* note 52.

68. Ayres & Donohue, *supra* note 5, at 1267.

69. See, e.g., LOTT, *supra* note 6, at 79 fig.4.9.

I we noted that dummy variables and trends are a convenient way to summarize data, but they might produce misleading impressions that can be avoided with more detailed nonlinear specifications. Among many other reasonable specifications, Lott's past work has reported the most general types of alternative specifications: breaking down the impact of the law on a year-by-year basis before and after the law; using nonlinear before-and-after trends; introducing some county-level data on the per capita number of permits issued; and employing data on the predicted number of permits, based upon the characteristics of the right-to-carry laws and some limited state information on permit issuance.

E. *Can Cocaine Use Explain the Results?*

But an alternative explanation is that the crack cocaine problem drove up crime [T]he regression would identify a relationship between higher crime and the failure to adopt a shall-issue law when the real cause would have been the influence of crack⁷⁰

One of Ayres and Donohue's greatest concerns is the apparent failure of previous research to account for the differential geographic impact of cocaine on crime. Indeed, as they argue, if the accessibility of cocaine/crack was primarily a problem in non-right-to-carry areas, those states might experience a relative increase in crime. However, despite their claims to the contrary, cocaine/crack has been addressed in even the earliest economics research on concealed handgun laws.

It is difficult to directly measure the violence caused by cocaine/crack, but we can measure the relative accessibility of cocaine in different markets. For example, Lott's book (and the Lott and Mustard paper) reported that including price data for cocaine⁷¹ did not alter the results. Using yearly county-level pricing data (as opposed to short-run changes in prices) has the advantage of picking up cost—but not demand—differences between counties, thus measuring the differences in availability across counties.⁷²

Research conducted by Steve Bronars and John Lott examined the crime rates for neighboring counties located within either fifty or 100 miles of each

70. Ayres & Donohue, *supra* note 5, at 1217 (footnote omitted).

71. *See, e.g.*, LOTT, *supra* note 6, at 54 n.8.

72. Even though Lott gave Ayres and Donohue the cocaine price data from 1977 to 1992, they have never reported using it. While simply using the price does not allow one to perfectly disentangle local differences in demand and supply, arbitrage basically assures that except for short periods of time, the differences in prices between these local markets will equal differences in selling costs. If the total cost of selling cocaine was the same in two different cities, any price differentials resulting from sudden shifts in demand would result in distributors sending cocaine to the city with the higher price until the price had fallen enough so that the prices between the two cities were equal.

other and situated on either side of a state border.⁷³ When the counties adopting the law experienced a drop in violent crime, neighboring counties directly on the other side of the border without right-to-carry laws experienced an *increase*. However, that is not all. The size of the spillover is larger if the neighboring counties are closely matched to each other in terms of population density. In other words, criminals in more urban areas (as measured by population density) are more likely to move across the border if the neighboring county is also urban. Ayres and Donohue argue that different parts of the country may have experienced differential impacts from the crack epidemic. Yet, if there are two urban counties next to each other, how can the crack cocaine hypothesis explain why one urban county faces a crime increase from drugs, when the neighboring urban county is experiencing a drop? Such isolation would be particularly surprising as criminals can easily move between these counties.

The second edition of *More Guns, Less Crime* uses region-specific fixed-year effects for five regions so as to account for factors that might influence crime rates differently on a year-to-year basis in different parts of the country. Thus, the coefficients measuring the impact of right-to-carry laws are measuring any change in crime rates relative to other counties in that region of the country (the Northeast, South, Midwest, Rocky Mountains, and Pacific). Since Ayres and Donohue acknowledge the usefulness of this approach, we used that approach consistently throughout the regressions reported earlier in this Response.

Ayres and Donohue argue that the states that tend to adopt right-to-carry laws also “tend to be Republican, have high NRA membership, and have low crime rates” and are thus less typical of the states where crack was a problem.⁷⁴ There still exist high-crime counties within these “low crime” states that do not fit that overall state profile. Indeed, it is the most densely populated, high-crime counties that experience the biggest drops in violent crime. More importantly, using the data up through 2000 produces similar results. Since the use of cocaine and other drugs appears to have gradually spread to rural states but subsided in urban areas where the problem originated, the differential trend that Ayres and Donohue are concerned about may even have been the opposite of what they conjecture.

73. Since Bronars has been unwilling to share the data collected on geographic locations, we have re-collected some of this information. See Bronars & Lott, *supra* note 1.

74. Ayres & Donohue, *supra* note 5, at 1287. Ayres and Donohue claim their own results show the opposite: States that adopted the law had “substantially higher” crime rates prior to adoption. *Id.* at 1255.

F. *Measurement Error in County-Level Data*

Maltz and Targonski consider the quality of UCR county-level data to be so poor that they dismiss Lott's work on that basis alone (at least if the data extend beyond 1992).⁷⁵

Ayres and Donohue incorrectly describe Maltz and Targonski's conclusions. Maltz and Targonski point out that not all police agencies report their crime data.⁷⁶ Both county- and state-level data are affected, since both rely on aggregating this lower-level data into larger units, and both thus experience measurement error. The missing-data problems are obviously more prevalent for the smallest reporting units in the least populated portions of states. However, the problem is actually not that extensive. Lott and Whitley showed that in a sample that weighs observations by population (essentially what we do in the regressions), only 6.8% of the total possible population came from counties with thirty-percent underreporting or greater.⁷⁷ In addition, given that rural jurisdictions were the most likely not to experience any murders or rapes, missing data is not equivalent to misreporting the crime rate.

Further, Maltz and Targonski had nothing to say about data after 1992 since they only examined data from 1977 to 1992. There is no discussion of a post-1992 break in the quality of data. Instead, what Maltz and Targonski actually say is that starting with 1994, the UCR data began showing the percentage of the county that did not report its crime data.⁷⁸ Nor do Maltz and Targonski provide any evidence that state-level data are more dependable than county-level data.⁷⁹ Aggregating county-level data to the state-level might lead to the type of aggregation bias that we discuss in Part II.

G. *Is the U.S. Murder Rate "Exceptional"?*

[T]he United States is exceptional in only one aspect of its crime problem—its high rate of lethal violence—[so] it might at first appear that guns must be a part of the problem.⁸⁰

In 2000, the United States had a murder rate of 5.5 per 100,000 people, Brazil 26.3, and Russia 23.0.⁸¹ Russia has had a ban on handguns since the

75. Ayres & Donohue, *supra* note 5, at 1259-60.

76. Michael D. Maltz & Joseph Targonski, *A Note on the Use of County-Level UCR Data*, 18 J. QUANTITATIVE CRIMINOLOGY 297, 297-318 (2002).

77. John R. Lott, Jr. & John Whitley, *Measurement Error in County-Level UCR Data: A Response*, 19 J. QUANTITATIVE CRIMINOLOGY (forthcoming 2003).

78. Maltz & Targonski, *supra* note 76, at 309.

79. The imputation rules for the state and county data differ somewhat, but given that the missing values are overwhelmingly in the lowest-crime counties, the different rules do not appear to make a large difference.

80. Ayres & Donohue, *supra* note 5, at 1203.

81. *Dateline (Brazil): Violent Deaths in Brazil Second Only to Columbia, UNESCO*

communist revolution and Brazil has had extremely stringent gun regulations since the 1930s. Most Eastern European nations have a homicide rate multiple times higher than the United States's.⁸² On the other extreme, countries such as Israel and Switzerland have high gun ownership rates and low homicide rates.⁸³ Jeff Miron recently examined homicide rates across forty-four countries and found that the countries with the strictest gun control laws tended to have higher homicide rates.⁸⁴

H. *Should Philadelphia Be Treated Differently from the Rest of Pennsylvania After 1989?*

Philadelphia is treated as a separate jurisdiction, because the law became effective in the city of Philadelphia at a different time than for the rest of Pennsylvania.⁸⁵

In fact, Philadelphia was only "partially exempted."⁸⁶ Permit holders in the suburbs or anywhere else in the state were allowed to bring their concealed handguns into Philadelphia, whether for work or shopping. Before the 1989 law, Pennsylvania had been a "may issue" state and Philadelphia continued to operate under those rules, but Philadelphia also became much more liberal in issuing permits after the passage of the state right-to-carry law, possibly to head off the law being extended to include the city. From 1989 to 1994, while concealed handgun permits in the state increased by 34.5%, the number of concealed handgun permits in Philadelphia increased by over 70%. Indeed, Philadelphia and three surrounding counties (Montgomery, Bucks, and Delaware) all ranked among the top seven counties in terms of the percent increase in issued permits between those two years.⁸⁷ Montgomery ranked first in the state with a 253% increase and Bucks second with a 241% increase. By the time that the rules were changed for Philadelphia, however, most of the demand in these counties had been satiated.

There are many other inaccurate claims, such as that Lott and Mustard argue that Maine and Virginia were not right-to-carry states; that their original research discussed clustering; and that there should be a substitution between overall violent crime and property crime. Yet space constraints prevent more detailed discussions.

Study Shows (NBC television broadcast, May 4, 2002).

82. LOTT, *supra* note 63, ch. 4; Jeffrey A. Miron, *Violence, Guns, and Drugs: A Cross-Country Analysis*, 44 J.L. & ECON. 615, 615-34 (2001).

83. LOTT, *supra* note 63.

84. Miron, *supra* note 82, at 624.

85. Ayres & Donohue, *supra* note 5, at 1271 n.96.

86. LOTT, *supra* note 6, at 152.

87. Interview with Dr. Alan S. Krug, Member, Pennsylvania Governor's Sportsmen's Advisory Council (Fall 2002).

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CONCLUSION

Analyzing county-level data for the entire United States from 1977 to 2000, we find annual reductions in murder rates between 1.5% and 2.3% for each additional year that a right-to-carry law is in effect. For the first five years that such a law is in effect, the total benefit from reduced crimes usually ranges between about \$2 and \$3 billion per year. The results are very similar to earlier estimates using county-level data from 1977 to 1996.

We appreciate the continuing effort that Ayres and Donohue have made in discussing the impact of right-to-carry laws on crime rates. Yet we believe that both the new evidence provided by them as well as our new results show consistently that right-to-carry laws reduce crime and save lives. Unfortunately, a few simple mistakes lead Ayres and Donohue to incorrectly claim that crime rates significantly increase after right-to-carry laws are initially adopted and to misinterpret the significance of their own estimates that examined the year-to-year impact of the law.

Their claims about significant "positive main effects" from right-to-carry laws are not supported by their own results. There is also no evidence that the state-level year-to-year estimates imply that crime rates were significantly higher than what would have been predicted prior to the passage of the laws. Our own evidence from the 1977 to 2000 period rejects these claims even more strongly.

Perhaps the most surprising conclusion is that applying their very own method of evaluating the costs and benefits implies large benefits from right-to-carry laws. This holds true not only when one studies the many different specifications in their paper, but also when one applies this method to their other contemporaneous work.

APPENDIX TABLE 1: REPORTING THE RESULTS ON VIOLENT CRIME RATES FROM STUDIES CRITICAL OF RIGHT-TO-CARRY LAWS (USING THE NATIONAL COEFFICIENTS FROM THE MOST CRITICAL STUDIES THAT EXAMINED THE CHANGE IN CRIME RATES BEFORE-AND-AFTER THE PASSAGE OF RIGHT-TO-CARRY LAWS)

Study	Tables in the study	Positive Effect	Zero Effect	Negative Effect***
Black & Nagin	Tables 1 & 2 (National Effects)	1	8	12
Duggan	Table 12	1	15*	14*
Ludwig	Tables 4 and 5	0	19	0
Ayres & Donohue	Table 1	0	13 (16)**	30 (27)**
Totals		2	55 (58)	56 (53)

* Duggan's study has typos mislabeling the statistical significance of two of his results. See Mark Duggan, *More Guns, More Crime*, 109 J. POL. ECON. 1086, 1110 (2001) (column 2 of table 12 and the results for rape and aggravated assault). For rape a coefficient of -.052 and a standard error of .0232 produce a t-statistic of 2.24. For aggravated assault a coefficient of -.0699 and a standard error of .0277 produce a t-statistic of 2.52.

** Because of downward rounding to 1.6, it is not possible to tell whether the t-statistics reported in Ayres and Donohue are statistically significant at the 10% level. The values in parentheses assume that a t-statistic of 1.6 is not significant at the 10% level, while the first values assume that a t-statistic rounded off to 1.6 is significant at that level. See Ian Ayres & John J. Donohue III, *Nondiscretionary Concealed Weapons Laws: A Case Study of Statistics, Standards of Proof, and Public Policy*, 1 AM. L. & ECON. REV. 436 (1999) (reviewing JOHN LOTT, *MORE GUNS, LESS CRIME* (1st ed. 1998)).

*** Some of these negative significant coefficients are a result of the authors replicating Lott's earlier work. If these were removed the numbers for negative significant coefficients would be as follows: Black and Nagin, 8; Duggan, 9; Ayres and Donohue, 25 (22); and Totals 42 (39). See Dan A. Black & Daniel S. Nagin, *Do Right-to-Carry Laws Deter Violent Crime?*, 27 J. LEGAL STUD. 209 (1998); Jens Ludwig, *Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data*, 18 Int'l Rev. L. & Econ. 239 (1998).

Appendix Table 1 lists the results for the four papers using national data that examine the before-and-after law changes in crime rates that were critical of Lott's work. Out of 113 coefficients reported by these critics, only 2 coefficients imply a statistically significant increase in crime after the passage of the law, 55 imply no statistically significant change, and 56 a statistically significant decline in crime. In other words, half the time Lott's results are confirmed, and in only 2% of all cases are the results reversed—and these are fairly dubious regressions.

It is also possible to provide a listing for Black and Nagin's state-by-state breakdown for the four violent crime categories. At the 10% level, 3 coefficients imply a statistically significant increase, 22 no significant change, and 15 a statistically significant decline. Of course as mentioned in the introduction to the second section of Lott's book, examining only simple before-and-after averages can be quite misleading and all these critical estimates report only these estimates.

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APPENDIX TABLE 2: ANALYSIS WITH YEAR-SPECIFIC TREND DUMMIES
(OUR MODEL INCLUDES REGION-SPECIFIC YEAR DUMMIES
AND USES 1977-2000 DATA)

	Violent crime	Murder	Rape	Robbery	Assault	Property crimes	Auto Theft	Burglary	Larceny
Before (-10)	-2.1% (2.2%)	3.5% (3.1%)	6.4% (3.2%)	0.5% (3.9%)	3.7% (3.6%)	1.2% (2.5%)	1.9% (3.5%)	4.5% (2.8%)	-2.1% (2.2%)
Before (-9)	0.1% (4.0%)	-1.1% (2.3%)	<u>7.6%</u> (4.3%)	7.8% (9.0%)	5.1% (4.6%)	1.6% (2.6%)	12.7% (14.2%)	4.0% (3.2%)	0.1% (4.0%)
Before (-8)	0.7% (3.5%)	4.4% (2.8%)	10.4% (4.5%)	5.0% (5.0%)	9.5% (4.8%)	3.0% (3.3%)	0.4% (4.1%)	7.4% (3.8%)	0.7% (3.5%)
Before (-7)	-0.7% (4.3%)	5.0% (4.2%)	8.2% (6.2%)	4.7% (6.7%)	8.8% (5.8%)	-0.2% (5.4%)	-4.0% (5.9%)	6.3% (6.1%)	-0.7% (4.3%)
Before (-6)	0.0% (3.4%)	3.3% (5.6%)	8.3% (6.5%)	3.7% (6.4%)	8.8% (5.8%)	1.6% (3.4%)	-0.8% (4.2%)	8.2% (5.1%)	0.0% (3.4%)
Before (-5)	-1.7% (2.9%)	5.0% (5.4%)	10.4% (6.4%)	3.4% (5.7%)	5.5% (5.3%)	1.4% (2.7%)	2.4% (3.5%)	<u>7.4%</u> (4.4%)	-1.7% (2.9%)
Before (-4)	-2.1% (3.1%)	11.2% (5.0%)	11.8% (6.0%)	5.6% (6.1%)	5.1% (6.5%)	1.1% (2.7%)	3.4% (4.7%)	<u>8.5%</u> (4.5%)	-2.1% (3.1%)
Before (-3)	-0.4% (3.3%)	<u>9.6%</u> (5.7%)	10.9% (6.8%)	<u>12.0%</u> (6.8%)	5.7% (6.3%)	2.6% (3.6%)	5.3% (4.4%)	9.6% (6.2%)	-0.4% (3.3%)
Before (-2)	3.9% (3.2%)	9.9% (6.8%)	14.7% (6.1%)	14.6% (7.3%)	<u>11.3%</u> (6.7%)	4.7% (3.2%)	6.7% (4.6%)	12.2% (5.9%)	3.9% (3.2%)
Before (-1)	1.4% (4.4%)	8.7% (7.1%)	13.4% (6.6%)	17.6% (8.6%)	8.2% (7.4%)	4.9% (3.7%)	<u>9.2%</u> (5.1%)	<u>13.3%</u> (7.1%)	1.4% (4.4%)
Zero	3.6% (4.7%)	11.3% (9.8%)	15.5% (7.7%)	15.9% (10.5%)	13.6% (8.8%)	5.9% (4.2%)	<u>13.5%</u> (6.9%)	<u>15.1%</u> (8.8%)	3.6% (4.7%)
After (1)	0.9% (4.6%)	11.1% (10.5%)	<u>19.3%</u> (10.7%)	13.4% (10.2%)	14.9% (10.5%)	4.7% (3.4%)	<u>15.9%</u> (8.2%)	<u>17.0%</u> (9.3%)	0.9% (4.6%)
After (2)	-1.9% (4.5%)	6.2% (10.5%)	<u>11.8%</u> (7.0%)	7.2% (7.4%)	8.9% (8.4%)	3.6% (3.5%)	9.3% (6.3%)	<u>12.0%</u> (6.5%)	-1.9% (4.5%)
After (3)	-1.3% (4.3%)	6.8% (6.7%)	8.6% (5.4%)	9.0% (8.4%)	7.2% (7.8%)	4.3% (3.8%)	11.3% (7.2%)	<u>11.4%</u> (6.8%)	-1.3% (4.3%)
After (4)	-4.9% (4.5%)	0.5% (6.9%)	-0.9% (5.2%)	4.2% (7.8%)	4.2% (7.2%)	2.3% (3.7%)	9.0% (7.3%)	7.1% (6.6%)	-4.9% (4.5%)
After (5)	-1.8% (5.3%)	0.1% (8.0%)	5.5% (5.5%)	7.6% (7.5%)	7.0% (8.7%)	7.1% (3.3%)	<u>15.2%</u> (8.1%)	<u>11.9%</u> (6.4%)	-1.8% (5.3%)
After (6)	2.0% (7.1%)	8.1% (7.5%)	8.1% (7.9%)	8.7% (8.1%)	14.9% (10.0%)	11.2% (4.5%)	22.3% (8.9%)	17.0% (8.3%)	2.1% (7.1%)
After (7)	-1.1% (5.2%)	-1.2% (9.7%)	9.1% (9.0%)	11.8% (8.4%)	15.2% (9.5%)	9.5% (4.0%)	26.0% (11.0%)	<u>16.5%</u> (9.3%)	-1.1% (5.2%)
After (8)	-1.8% (3.7%)	4.8% (9.2%)	9.3% (7.8%)	8.0% (7.3%)	9.3% (9.3%)	6.9% (3.0%)	20.5% (8.3%)	<u>15.2%</u> (7.9%)	-1.8% (3.7%)
Number of observations	62,702	37,060	49,606	49,625	62,459	65,705	63,435	65,470	65,462
R ²	0.86	0.81	0.76	0.91	0.81	0.82	0.85	0.81	0.80

Notes: Robust standard errors are shown in parentheses and the estimates are clustered by state. The same set of control variables are used as in Table 3a. Coefficients that are significantly different from zero at the 10% level are underlined. Coefficients that are significantly different from zero at the 5% level are displayed in bold. Coefficients that are significantly different from zero at the 1% level are both underlined and displayed in bold. We want to emphasize that "significantly different from zero" is only of secondary interest for this analysis; what is interesting is whether the coefficients are significantly different from each other.

APPENDIX TABLE 3: LIMITED DEMOGRAPHIC VARIABLES AND
ADDING LAGGED PRISON POPULATION; 1977-2000

	Violent crime	Murder	Rape	Robbery	Assault	Property crimes	Auto Theft	Burglary	Larceny
Before (-10)	-0.7% (2.2%)	5.5% (3.8%)	<u>7.9%</u> (4.8%)	2.6% (3.3%)	6.4% (4.4%)	3.5% (2.4%)	5.1% (3.3%)	7.1% (3.6%)	8.3% (5.1%)
Before (-9)	2.1% (4.1%)	2.1% (4.4%)	9.6% (6.4%)	11.5% (8.1%)	8.7% (5.6%)	<u>4.8%</u> (2.8%)	17.0% (13.4%)	<u>7.8%</u> (4.5%)	<u>10.2%</u> (5.9%)
Before (-8)	2.1% (3.6%)	7.3% (4.5%)	<u>11.3%</u> (6.1%)	8.0% (5.2%)	12.9% (5.7%)	<u>5.8%</u> (3.4%)	4.8% (4.5%)	10.6% (5.0%)	<u>12.4%</u> (6.6%)
Before (-7)	0.0% (4.8%)	7.1% (7.0%)	8.9% (8.3%)	6.8% (8.4%)	11.7% (7.3%)	2.0% (5.6%)	-0.2% (7.3%)	9.0% (7.7%)	8.9% (9.3%)
Before (-6)	1.0% (3.8%)	5.1% (8.6%)	9.4% (8.6%)	5.8% (8.5%)	<u>12.3%</u> (7.4%)	4.0% (3.7%)	3.1% (5.9%)	10.9% (6.9%)	11.0% (8.2%)
Before (-5)	-0.4% (3.1%)	7.0% (8.2%)	11.9% (8.2%)	5.3% (7.0%)	9.5% (6.8%)	3.9% (2.8%)	6.1% (4.8%)	<u>10.3%</u> (5.7%)	<u>10.7%</u> (7.3%)
Before (-4)	-1.2% (3.2%)	12.9% (7.9%)	<u>12.9%</u> (7.8%)	6.8% (6.8%)	9.5% (8.0%)	3.4% (2.8%)	7.2% (5.3%)	<u>10.9%</u> (6.0%)	10.1% (7.7%)
Before (-3)	0.6% (3.4%)	11.3% (8.7%)	12.6% (8.5%)	<u>13.8%</u> (7.9%)	10.3% (7.9%)	5.2% (3.5%)	<u>9.8%</u> (5.4%)	<u>12.4%</u> (7.5%)	12.7% (8.5%)
Before (-2)	4.4% (3.4%)	12.0% (10.1%)	<u>15.6%</u> (8.5%)	<u>16.5%</u> (9.2%)	<u>15.3%</u> (8.3%)	<u>6.7%</u> (3.6%)	<u>10.7%</u> (5.9%)	<u>14.5%</u> (8.3%)	<u>15.2%</u> (9.2%)
Before (-1)	2.0% (4.4%)	11.1% (10.7%)	<u>14.8%</u> (9.0%)	<u>20.1%</u> (10.8%)	11.8% (9.3%)	<u>7.2%</u> (3.9%)	13.6% (7.0%)	<u>15.9%</u> (9.5%)	16.5% (10.2%)
Zero	4.4% (4.4%)	13.6% (13.4%)	<u>16.7%</u> (10.1%)	18.8% (12.8%)	17.3% (10.7%)	8.2% (4.1%)	18.0% (9.3%)	18.0% (11.1%)	<u>19.7%</u> (11.2%)
After (1)	1.6% (4.3%)	13.4% (13.6%)	20.9% (12.8%)	15.7% (12.7%)	18.4% (12.1%)	<u>6.6%</u> (3.4%)	19.8% (10.0%)	19.2% (11.7%)	20.7% (12.2%)
After (2)	-1.6% (4.7%)	6.3% (9.9%)	12.9% (8.5%)	8.4% (9.2%)	11.9% (9.7%)	5.0% (3.6%)	<u>13.1%</u> (8.0%)	13.0% (8.6%)	<u>15.4%</u> (8.9%)
After (3)	-1.4% (4.5%)	6.8% (10.1%)	<u>12.3%</u> (7.0%)	8.8% (11.2%)	11.0% (9.9%)	5.1% (3.7%)	13.5% (9.3%)	11.8% (9.4%)	<u>15.0%</u> (8.9%)
After (4)	-2.1% (6.0%)	7.3% (8.6%)	3.1% (8.7%)	9.3% (9.5%)	12.1% (10.1%)	8.5% (3.4%)	<u>21.1%</u> (7.9%)	12.5% (9.0%)	<u>21.6%</u> (8.6%)
After (5)	-1.7% (4.4%)	3.7% (12.1%)	8.5% (9.3%)	12.7% (9.7%)	13.1% (10.7%)	10.6% (3.6%)	26.6% (10.3%)	13.7% (10.1%)	24.5% (8.4%)
After (6)	-3.2% (4.6%)	9.5% (9.5%)	10.8% (9.3%)	9.2% (11.0%)	14.3% (11.1%)	10.3% (3.4%)	27.1% (10.8%)	<u>15.6%</u> (11.5%)	24.8% (9.2%)
After (7)	-2.3% (5.6%)	1.7% (10.9%)	12.0% (10.2%)	13.0% (9.9%)	18.0% (11.3%)	11.5% (4.8%)	32.8% (12.0%)	17.7% (11.4%)	28.3% (9.7%)
After (8)	<u>-6.1%</u> (3.7%)	3.2% (10.0%)	8.2% (10.1%)	3.6% (10.2%)	9.8% (11.3%)	5.6% (4.1%)	22.2% (10.0%)	12.8% (10.7%)	<u>19.7%</u> (10.4%)
Number of observations	57,726	34,452	45,546	45,863	57,529	60,587	58,531	60,392	60,374
R ²	0.87	0.80	0.76	0.92	0.81	0.83	0.85	0.81	0.82

Notes: Robust standard errors are shown in parentheses and the estimates are clustered by state. The same set of control variables are used as in Table 3b. Coefficients that are significantly different from zero at the 10% level are underlined. Coefficients that are significantly different from zero at the 5% level are displayed in bold. Coefficients that are significantly different from zero at the 1% level are both underlined and displayed in bold.

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APPENDIX TABLE 4: SPLINE MODEL, 1977-2000 STATE LEVEL DATA
REGIONAL YEAR FIXED EFFECTS

	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny
Maine	<u>5.3%</u> (8.4%)	<u>7.2%</u> (0.3%)	8.0% (2.4%)	8.5% (0.2%)	<u>6.1%</u> (6.7%)	<u>6.3%</u> (0.2%)	3.7% (1.1%)
Florida	-6.6% (0.0%)	-3.0% (4.0%)	-11.3% (0.0%)	-0.3% (83.1%)	-10.9% (3.1%)	-4.0% (0.1%)	-2.1% (3.0%)
Virginia	-1.5% (29.6%)	3.1% (1.4%)	<u>-3.0%</u> (5.8%)	2.0% (16.7%)	-0.6% (76.0%)	-0.5% (66.4%)	-2.2% (4.1%)
Georgia	-2.5% (17.8%)	-4.5% (0.5%)	-8.7% (0.0%)	-5.2% (0.0%)	-0.4% (84.7%)	-5.0% (0.0%)	-2.8% (0.5%)
Pennsylvania	0.7% (62.2%)	2.7% (3.5%)	<u>3.4%</u> (8.1%)	5.1% (0.0%)	0.6% (78.8%)	<u>2.2%</u> (6.0%)	3.5% (0.0%)
West Virginia	-4.7% (16.9%)	-1.9% (33.8%)	-3.3% (18.7%)	10.6% (0.0%)	4.9% (20.8%)	1.9% (30.0%)	1.3% (40.9%)
Idaho	-2.1% (59.4%)	-1.4% (58.3%)	0.3% (91.9%)	-0.3% (93.2%)	-0.1% (97.1%)	-1.2% (49.8%)	-1.7% (23.0%)
Mississippi	11.2% (0.0%)	7.2% (0.0%)	12.6% (0.0%)	16.7% (0.0%)	16.8% (0.0%)	8.8% (0.0%)	8.6% (0.0%)
Oregon	-1.1% (64.9%)	4.2% (0.1%)	-3.1% (19.9%)	1.5% (52.3%)	<u>-4.1%</u> (9.8%)	-0.3% (90.0%)	3.5% (26.2%)
Montana	1.2% (80.2%)	7.5% (1.2%)	0.8% (82.6%)	15.6% (0.0%)	-5.9% (3.8%)	0.5% (83.5%)	2.7% (0.7%)
Alaska	20.1% (0.1%)	21.3% (0.0%)	5.1% (40.8%)	12.7% (2.9%)	-4.5% (50.2%)	7.1% (13.2%)	2.6% (44.7%)
Arizona	-9.5% (0.3%)	-8.2% (4.0)	-9.4% (0.4%)	-1.2% (76.5%)	-0.2% (94.7%)	-5.1% (1.5%)	-4.8% (3.5%)
Tennessee	-1.2% (59.3%)	1.6% (36.6%)	-3.0% (14.0%)	0.1% (95.3%)	0.6% (89.2%)	0.3% (88.4%)	1.5% (31.1%)
Wyoming	4.7% (42.1%)	-4.8% (19.8%)	<u>8.6%</u> (7.2%)	-2.4% (50.9%)	8.5% (16.0%)	-1.8% (64.6%)	0.1% (96.7%)
Arkansas	Dropped	Dropped	Dropped	Dropped	Dropped	Dropped	Dropped
Nevada	-10.4% (0.7%)	-8.9% (0.6%)	-12.3% (0.1%)	-3.1% (51.1%)	-4.4% (47.0%)	-0.1% (97.6%)	-6.0% (1.1%)
North Carolina	-7.2% (0.1%)	-3.4% (13.9%)	-4.9% (2.7%)	-2.5% (18.3%)	8.2% (6.4%)	-5.0% (0.2%)	-2.5% (3.9%)
Oklahoma	-1.4% (79.3%)	-3.1% (33.7%)	-6.2% (13.5%)	1.6% (70.1%)	-0.6% (91.7%)	-4.8% (12.7%)	-1.7% (48.8%)
Texas	-11.0% (0.1%)	-7.6% (0.2%)	-5.4% (10.3%)	-6.6% (2.3%)	-2.8% (62.8%)	-5.6% (3.6%)	-3.0% (12.2%)
Utah	-13.3% (1.7%)	-17.9% (0.0%)	-14.8% (0.8%)	-1.7% (78.9%)	2.7% (66.5%)	-6.3% (10.6%)	-7.0% (0.9%)
Kentucky	-4.3% (27.7%)	-8.1% (1.0%)	<u>-8.0%</u> (6.3%)	-19.0% (2.5%)	8.7% (21.0%)	-4.3% (10.9%)	-3.1% (16.7%)
Louisiana	-1.4% (73.5%)	0.2% (95.6%)	-1.6% (69.0%)	1.4% (65.4%)	6.8% (17.1%)	1.9% (54.2%)	1.8% (54.8%)
South Carolina	-9.9% (0.1%)	-6.5% (5.8%)	-6.9% (6.2%)	-9.4% (0.7%)	-2.9% (54.7%)	-7.6% (2.2%)	-4.3% (15.0%)
Number of observations	1,187	1,182	1,189	1,189	1,141	1,141	1,141
R^2	0.930	0.904	0.950	0.937	0.892	0.925	0.977
Number significantly positive	2	7	4	6	2	3	4
Number insignificantly positive	4	2	3	5	8	5	6
Number significantly negative	6	8	8	4	3	5	6
Number insignificantly negative	10	5	7	7	9	9	6

Notes: The numbers are the differences between the pre-adoption and post-adoption trend dummies. The values in parentheses are the F-test probabilities that the pre-adoption and post-adoption trends are identical. Differences that are significant at the 10% level are underlined. Differences that are significant at the 5% level are displayed in bold. Differences that are significant at the 1% level are both underlined and displayed in bold.