Confirming More Guns, Less Crime

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Corrected

Abstract

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 percent 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 billion and $3 billion per year.

Ayres and Donohue have simply misread their own results. Their own most generalized 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.

I. Introduction

Quite a few empirical papers have now 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] 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. But they have misread their own results. The most detailed results that they report -- follow the change in crime rates on a year-by-year basis before-and-after right-to-carry laws are adopted -- clearly show large drops in violent crime that occur immediately after the laws are adopted. Their hybrid results that show 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. But 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 re-examine the evidence, extend the data set 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 here inaccurately describes the literature and also fails to address previous critiques of their work. For example, Ayres and Donohue claim that "When we added five years of county data and seven years of state data, allowing us to test an additional 14 jurisdictions that adopted shall-issue laws, the Lott and Mustard findings proved not to be robust." All their tables report results for "Lott's 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.[5] Ayres and Donohue's work thus extends the county level data by ***one year***, from 20 to 21 years.

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[1] For a summary see John R. Lott, Jr., Introduction, 44 J Law & Econ. 605-614 (October 2001). Individual papers that show a benefit from the law include: William A. Bartley & Mark A. Cohen, The Effect of Concealed Weapons Laws: An Extreme Bound Analysis," Econ. Inquiry 258-265 (April 1998); Stephen G. Bronars & John R. Lott, Jr., Criminal Deterrence, Geographic Spillovers, and Right-to-carry laws, Am. Econ. Rev. 475-479 (May 1998); David B. Mustard, The Impact of Gun Laws on Police Deaths, 44 J Law & Econ. 635-658 (October 2001), John R. Lott, Jr. & John E. Whitley, Safe-Storage Gun Laws: Accidental Deaths, Suicides, and Crime, 44 J Law & Econ. 659-689 (October 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 Law & Econ. 747-770 (October 2001); Florenz Plassmann & Nicolaus Tideman, Does Right to Carry Concealed Handguns Deter Countable Crimes? Only a Count Analysis Can Say, 44 J Law & Econ. 771-798 (October 2001); Tomas B. Marvell, The Impact of Banning Juvenile Gun Possession, 44 JLaw & Econ. 691-714 (October 2001); and Carlisle Moody, Testing for the Effects of Concealed Weapons Laws, 44 JLaw & Econ. 799-813 (October 2001).

[2] In fact, these critics provided a great deal of supportive evidence. See Appendix Table 1.

[3] For an earlier discussion on Lott's research see Ian Ayres & John Donohue, Nondiscretionary Concealed Weapons Law: A Case Study of Statistics, Standards of Proof, and Public Policy, 1 Am. Law & Econ. Rev. 436 (1999) and John R. Lott, Jr., More Guns, Less Crime: A Response by Ayres and Donohue, Yale Law School Working Paper (September 1, 1999) (http://papers.ssrn.com/paper.taf?abstract_id=248328).

[4] John Donohue & Steven Levitt, The Impact of Legalizing Abortion on Crime Rates, 116 (2) Q. J Econ. 379-420 (May 2001).

[5] In one footnote (Ian Ayres & John Donohue, Shooting Down the More Guns, Less Crime Hypothesis, Stanf. Law Rev. at 34, n. 74) they acknowledge that these additional data were used but they claim Lott "only reports results for this data set from tests of the trend specification." Yet, in Lott's book Figures 9.1 to 9.5 provide information on the nonlinear before-

The following section of our paper 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 Section III, 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, Section IV provides a partial response correcting some inaccurate claims made by Ayres and Donohue.

II) 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. In their current paper, Ayres and Donohue only provide this breakdown for state level data from 1977 to 1999. Donohue's Brookings paper presents the year-by-year changes for county level data from 1977 to 1997.[6] Their state level data shows the crime rates in the first year of the law, the second year, and so on, but 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.[7] Also one of the county estimates includes a separate state time trend for each state.

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. 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.[8]

A) Is there a "Robbery Effect"?

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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. Other work that has examined the data through 1996 includes: Mustard, *supra* note 1, and Lott & Whitley, *supra* note 1.

[6] John Donohue, "Divining The Impact of State Laws Permitting Citizens to Carry Concealed Handguns," in: Evaluating Gun Policy: Effects on Crime and Violence, edited by Jens Ludwig and Philip Cook, Brookings Institution: Washington, DC (February 2003). Identical results were reported in Table 5 in Ian Ayres & John Donohue, Shooting Down More Guns, Less Crime, Stanford University Working Paper (May 2002), and Table 9 in Donohue's Brookings paper.

[7] 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 constraint of making all years have to 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 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 and that very little weight should be placed on those estimates, whether they are falling or increasing.

[8] Jon Vernick and Lisa Hepburn, Description and Analysis of State and Federal Laws Affecting Firearm Manufacture, Sale, Possession, and Use, 1970-1999, Johns Hopkins Center for Gun Policy Research Working Paper (December 21, 2001), see Table 5.

> "Robbery is a good place to start our inquiry because it is committed in public more than other crime, and should be the crime most likely to decline if the Lott and Mustard story of deterrence has any plausibility." (p. 11)
>
> "the failure of the model to show a drop in robbery, casts doubt on the causal story that they advance." (p. 22)

Ayres and Donohue have consistently argued over several papers that robbery is the key result upon which the deterrence by right-to-carry laws is based.[9] In contrast, Lott has argued many times that there is no a priori reason to believe that the benefits are larger for robbery than other violent crimes.[10] But putting that debate aside, the robbery results presented by Ayres and Donohue present a very clear, consistent story (Figure 2a). 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. However, after that, robbery rates in right-to-carry states fell relative to non-right-to-carry states for the next 9 years, and then remained fairly constant through year 17. 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.[11]

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 8 and 12 percent respectively. It is difficult to see how anyone could look at these year-by-year results and accept their claims that "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 average in evaluating the impact of the law, yet they do not consistently apply that insight when discussing the evidence.

B) Murder Rates

Figure 2b 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 percent the drop in murder rates during the first three years of the law as showing "relatively little movement."[12] 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, had had the law in effect for more than 13 years. The increase during years 14, 15, 16, and 17 thus solely reflect 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.[13] The increase between years 13 and 14 is also more apparent than real. The real "increase" is

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[9] Ayres & Donohue, *supra* note 3. See also Ayres & Donohue, *supra* note 6, at 13, 14, and 28.

[10] John R. Lott, Jr, More Guns Less Crimes, University of Chicago Press: Chicago, Illinois (2000), 133-4, 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. 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" (italics in the original).

[11] See Lott, *supra* note 10, pp. 172-174.

[12] Ayres and Donohue, *supra* note 5, at 27.

[13] 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 percent of the state's population

actually not due to any sudden change in Maine's crime rates, but due to the fact that other states are included in calculating the crime rate for year 13, while only Maine is used for year 14.

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.[14] 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 10 percent.

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 assaults fell almost continually after the laws were enacted (Figures 2c and 2d). Even choosing for comparison the sixth year after the law where rape and aggravated assault rates have slightly risen back up, still leaves rapes 9 percent below their peak and aggravated assaults 3 percent below theirs.

The county level estimates with individual state trends provide a mixed picture. With the exception of one single year, rape rates are rising before the law and falling 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 8 or more years. Yet, since the aggravated assault rate was rising for years prior to the law at least as fast as it was after the law 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 10 percent for the first six years after the adoption of the law, and remain below the pre-law levels for at least 12 years. Only when Maine becomes the sole remaining state in the sample does the rape rates rise, and it rises above the pre-law levels for just one year (by 7 percent). Rape rates then plunge by over 25 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 pre-law levels. There is only one year out of the seventeen years after the law has been passed that the rape rate exceeds any of the values during the twelve years before the law. The state level aggravated assault data show only a temporary beneficial effect, with an initial decline in rape that is eventually eliminated. 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.

D) Critiques of Year-by-Year Breakdown of Law's Impact

The debate over simple dummies, splines, or hybrids becomes irrelevant when one has examined the year-by-year breakdown. All those approaches are simple ways to summarize the crime patterns and

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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 city police chief, Frank Maoroso.

[14] 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.

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.

Yet, Ayres and Donohue have obviously looked at these estimates from their papers and come to the exact opposite conclusions. Donohue has even taken the year-by-year estimated impact of the law to imply that right-to-carry laws increased crime. In his Brookings paper, Donohue writes (p.20): "For the 1977-97 period [using the results from Donohue's Table 5], the effect for the '2 or 3 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)." Indeed, this is true for his Table 5,[15] but irrelevant. The question isn't whether these coefficients are different from zero, but whether they have changed relative to other coefficients. The patterns for robbery, murder, and rape clearly show that the longer the law is in effect, the greater the drop in crime.

Nor is it relevant, as Donohue suggests, to compare the crime levels before-and-after the law (p. 20).[16] When crime rates are rising before the law and falling afterwards, there might be very little change in the before-and-after means even though, as the diagram for something like 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.[17]

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 down."[18] Their county level results indicate that by the second and third years after the law has been adopted, all violent crime rates are below the values that they had in the last 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 their claim (regarding the county level data) that "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."[19] Yet, their year-by-year estimates shown in our Figures 2a-2d indicate that no such "positive main effect" is occurring.

A claim might be made that the hybrid is mis-specified solely because they are fitting a straight line to a nonlinear relationship. Take Figure 1, where the crime rate is falling at an increasing rate after the

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[15] In addition to being completely irrelevant, it is still a selective reading of his results. In Table 6, only one of nine coefficients indicates a positive and statistically significant effect for the second and third years after the law. For Tables 7 and 8, the numbers are 4 out of 9 and 3 out of 9, respectively.

[16] Comparing years –1 and –2 before the law with years 2 and 3 afterward show 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.

[17] Donohue, *supra* note 6, at 20, writes "Lott mentions . . . the so-called inverted V hypothesis. While 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)." To test this one must compare the before and after trends, and their own estimates 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.

[18] Ayres & Donohue, *supra* note 5, at 34.

[19] Donohue, *supra* note 6, at 10. Ayres & Donohue, *supra* note 5, at 34, write: "the main effect of the shall-issue laws is positive but over time this effect gets overwhelmed." See also Ayres & Donohue, *supra* note 5, at 18.

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.[20] 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.

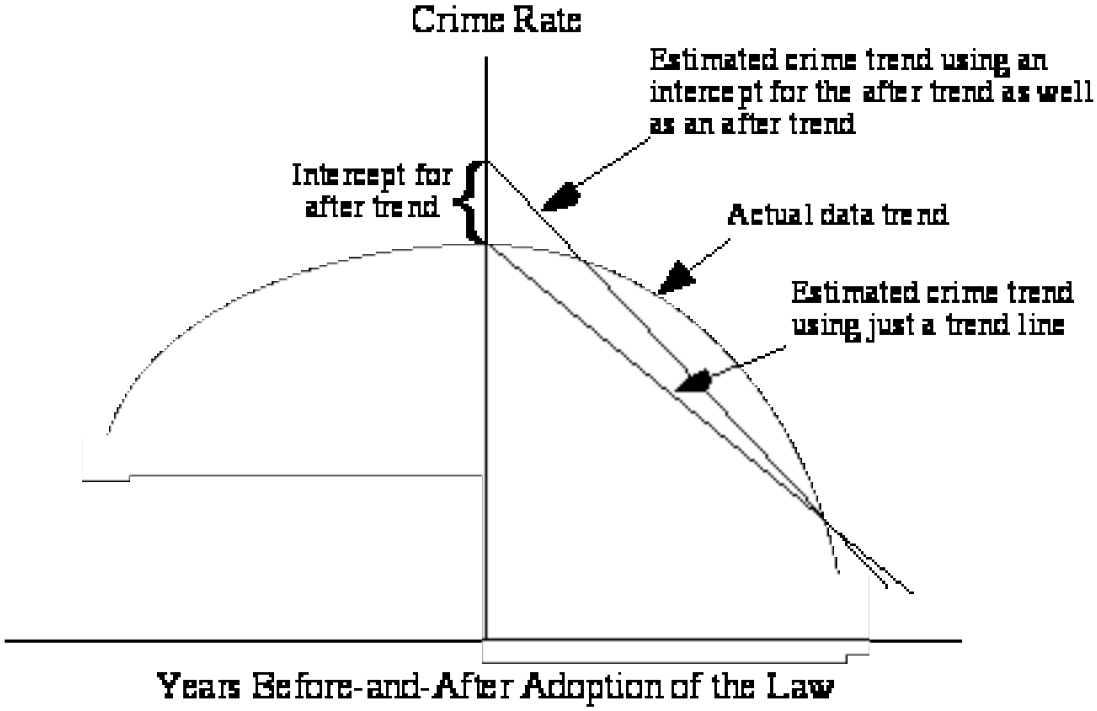

Figure 1: Showing what happens when you fit an intercept shift and a trend to a nonlinear trend

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-3i, Ayres and Donohue note that: "...one can see that in 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 variables). This raises concerns about the reliability of the regression model,..."[21] 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-3i do not report standard errors, this information is reported in Donohue's Table 6 for the 1977 to 1997 time frame.[22] The crime rates for violent crimes, rape, robbery, and aggravated

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[20] 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 is observed. Comparing the results in lines 5 and 6 show that the positive intercept shift is associated with the trend line becoming more steeply negative.

[21] Ayres & Donohue, *supra* note 5, at 27.

[22] The differences in point estimate values between Donohue's Table 6 (Donohue, *supra* note 6) and Ayres and Donohue's Figures 3a-3i simply arise because they include all the possible year dummies for the figures and only a portion of them for Donohue's Table 6. Adding two years to the data set is not the crucial difference. The difference is that in Donohue's

assault were never statistically significantly different from zero for at least 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 1, 2, 5, or 6.

We will review our concerns with Ayres and Donohue's tests and how they interpret them in Section IV, 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 over predicts the costs (taking Ayres and Donohue's own results at face value), 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. Tables 10 and 11 in Ayres and Donohue and Tables 1 and 3 in Donohue represent the same specifications.[23] Donohue's separate estimates do include estimated year-by-year effects of the law in addition to the dummy, spline, and hybrid specifications.[24]

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.[25] 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 over predicting crime rates in early years, which is exacerbated by 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.

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. This entire discussion makes it very difficult to put much weight on whether the year-by-year dummies are different from zero.

[23] 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.

[24] Another caveat is worth noting: the same sets of specifications (dummy, spline, and hybrid) are not reported 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.

[25] 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 don't make these calculations for the regressions estimating the changing year-by-year impact of the law. Even though Ayres and Donohue include a pre-law crime rate trend (as used in the spline or hybrid models), no pre-law 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 2d).

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 adopt ion of the law, there is an average annualized benefit from right-to-carry laws across all the specifications of $766 million.[26]

Table 2 does the same breakdown for all tables listed in Donohue's Brookings paper, using both the county and state level data for 1977 to 1997 data. 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 a saving 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 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 tables that implies that crime rates would rise uses only a post-passage 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 42 percent of the law's first five years, Louisiana's by 34 percent, and Tennessee's by 30 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.[27]

Although a 42 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 US 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 40 percent between 1995 and 1998 and continued declining after that. A similar breakdown for Louisiana, South Carolina, and Tennessee is available from the authors.

III. County Level Data from 1977 to 2000

[26] The year-by-year breakdown in the impact of the law reported in Figures 3a-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.

[27] 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, The Advocate, Baton Rouge, La. (January 12, 1997) at 8A.

A) Advantages and Disadvantages of Different Types of Data

While most crime analysis has traditionally been done at the state level, disaggregated data have 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.[28] 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 the argument, that income is negatively correlated with property crimes (that is, a person with higher income commits fewer property crimes). Assume that Location 1 has a population of 1,000 persons and Location 2 has a population of 2,000 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 1 increases to $60,000 and crimes fall to 80, and the per capita income at Location 2 decreases to $36,000 and the number of crimes increases to 240. If we examined both locations separately, then we would detect that increases in income lead to fewer property crimes and vice versa. But 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) while the number of property crimes committed has increased as well (from 300 to 320). Aggregating the data together erroneously 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 six 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-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 will analyze the county level data from 1977 to 2000 using the four different methods discussed by Ayres and Donohue as well as Donohue's Brookings paper. A more precise breakdown of year-by-year impacts from the law is provided 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. In addition to arrest rates, we use: per capita income; population density; arrest rates; the execution rate for murder; per capita welfare payments; per capita unemployment insurance payments; retirement payments per person over

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[28] Lott, *supra* note 10, Chp. 2.

age 65; 36 different demographic measures by age, gender, and race; state level poverty and unemployment rates; county and regional year fixed effects.[29]

Figures 4a through 4e graphically report the results for a post-passage dummy, spline, hybrid, and year-by-year impacts. (The property crime rates are lumped together to save space but are available from the authors.) 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 that the right-to-carry law is in effect are associated with about 10 percent declines in murder and rape and an 8 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 in crime, with murder falling by 5 percent and robbery by 8.7 percent.[30]

Second, both the spline and hybrid models closely track the more disaggregated year-by-year estimates. In fact, for murder, rape, and especially robbery estimates, the spline and hybrid estimates are virtually identical. The hybrid's post-passage law 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.[31] The spline and the hybrid models indicate positive, but statistically insignificant, trends in violent crime rates prior to the right-to-carry law. However, after the law has been passed, violent crime rates are declining. The change in trends is statistically significant at least at the 10 percent level for all individual violent crime categories for the spline estimates, implying that murder, rape, and robbery fall by over 1.5 percent per year during each additional year that right-to-carry laws are in effect. While the effect for murder is the same as the 1.5 percent annual decline found by Lott using data from 1977 to 1996, the results for rape and robbery are smaller than the 3.2 and 2.9 percent annual declines found earlier by Lott.

The hybrid model estimates the change in before-and-after trends and gives identical results to those found by the spline model (for up to the third decimal place). The change in trends for rape and robbery are again significant at better than the 10 percent level. The impact of the law on murder rates is also statistically significant at least at the 10 percent level when the negative intercept shift is included in the F-test.[32] Whatever different results Ayres and Donohue obtained for the post-passage dummy with their hybrid approach, these effects disappear when the additional data are included.

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[29] Despite Ayres & Donohue, *supra* note 5, at 30, claim that "many of the explanatory variables are only measured on the state level," with the exception of the state level poverty and unemployment rates these are all county level control variables.

[30] 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.

[31] As we will discuss later, using weighted least squares is not the ideal estimation technique. Other methods can be used for calculating the standard errors. Clustering is also a possibility, and doing so does affect the results. Yet, it is not clear that applying STATA's clustering command to all counties within a state provides an adequate solution to the problem of cross-correlation.□While it is possible that error terms are correlated across jurisdictions within a state, the more important correlations may be among neighboring jurisdictions across state lines. Ayres and Donohue also do not report the results with clustering.

[32] However, since both the post-passage 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 post-passage dummy, as Ayres and Donohue do. In this case, the net effect of adding both the post-passage dummy and the difference in before-

Table 3b re-estimates the regressions along the lines of the data set compiled by Wentong Zheng.[33] 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 10 to 19, 20 to 29, and 30 to 39. Like Ayres and Donohue and our earlier estimates, we continue to use the population, the per capita income, the state level unemployment 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 the 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 10 percent level. For the hybrid model, only the rape and robbery rate trend changes are significant at about or better than the 10 percent level. For murder, the joint F-test for both the post-passage dummy and the after law trend is statistically significant at least at the 10 percent level.[34]

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 rate declining 0.8 percent faster after the law, while late adopters had a 1.8 percent faster decline. For rapes, early adopters saw a decline of 1.4 percent and late adopters a decline of 2.4 percent. For robberies, the difference was again in the opposite direction of what Ayres and Donohue note. Early adopters experienced a .5 percent faster annual decline and late adopters a 2.1 percent 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 Table 3a through 5 imply that the costs of crime fall by between about $2.5 and $3 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."[35] There is no evidence that "crime was substantially higher than the regression model would have otherwise predicted." [36]

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and-after trends together implies that during the first full year that the right-to-carry law is in effect, murder falls by 3 percent, rape falls by 2.3 percent, robbery falls by 2.8 percent, and aggravated assault rises by 1 percent. By the second year of the law, aggravated assaults will also have fallen by 1.1 percent.

[33] 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.

[34] The F-statistic equals 2.5 with a probability of 8.2 percent.

[35] Donohue, *supra* note 6, at 10. See also Ayres and Donohue, *supra* note 5, at 18, 34.

[36] Ayres and Donohue, *supra* note 5, at 27.

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.[37]

Probably the simplest test whether there are abnormal increases in crime immediately before the adoption of a right-to-carry law. To eliminate the possibility that the before-adoption 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 data set, 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.[38]

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 reversal to the mean. To test this possibility, we repeated the analysis without observations from the adoption year and 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 regression to the mean also seems to be ruled out by Figures 4a to 4e (or even, for that matter, the county level estimates from Ayres and Donohue presented in Figures 2a to 2c), simply because the crime rates are not returning to their pre-law lows, but actually going well below those values. This is not simply a reversion to normal pre-law 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

[37] 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 non-correlated with the other right-hand side variables. Because 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 28-30, argue that no good instruments are available in this case, though 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.

[38] 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 percent or better level.

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.

D) Poisson Estimates

A major problem with county level data is what to 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 50 percent of the counties had zero murders in 2000. 27 percent of counties had no robberies that year and 21 percent had no rapes. Yet, virtually all counties experience at least one aggravated assault and thus at least one violent crime. The standard approach to analyze data with these kind of distribution is to use a Poisson regression model. We re-estimated 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.[39] Unfortunately, even here there is a problem since STATA does not support the calculation of robust standard errors in its routine that absorbs the fixed effects (xtpois).

All results reported in Tables 6 and 7 are similar to those that we reported using weighted least squares. The year-by-year estimates in Table 6 show declines in all three crimes after the right-to-carry law is enacted. Between the year of passage and the eighth year after the law, murder rates have fallen by almost 17 percent, rape rates by 6.4 percent, and robbery rates by almost 16 percent. 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 third year after the law for robbery.

The spline and hybrid estimates imply similar but smaller effects than previously reported (Table 7). Crime rates are rising before the law and falling afterwards. To the extent that there is a post-passage 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 percent and rape and robbery declining at an annual rate of about 1 percent.

IV. 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. For example, the nightmare scenario for

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

> those asserting the value of defensive use of guns is not mentioned: the case of the Japanese exchange student, Yoshihiro Hattori, on his way to a Halloween party in October of 1992 who mistakenly approached the wrong house and was shot to death by the owner Rodney Peairs."
>
> Ayres and Donohue, p. 6.

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 page 1 of his book and refers to it as showing "how defensive gun use can go tragically wrong." Lott and Mustard's original paper also describes this same incident on page 2.[40] Lott's book also explicitly notes many gun crimes and repeatedly discusses how one must analyze the "net effect" of guns.[41]

2) 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.

> A) "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 might also 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." (p. 9)

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.[42] They claim that while overall homicides declined by 6.5 percent, firearm homicides fell by 21 percent. (Non-firearm homicides actually rose by 9.8 percent, 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 significantly.[43] No statistical evidence supports Ayres and Donohue's concern.

> B) "This [example] suggests that those who are able to secure handgun permits are not always model citizens." (p. 10)

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[40] 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 they 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." Leslie Zganjar, Homeowner who killed Japanese student feared for life, Associate Press (September 13, 1994).

[41] For example, Lott, *supra* note 10, at 194, 195, 223. See also the first paragraph in John R. Lott, Jr & William R. Landes, Multiple Victim Public Shootings, University of Chicago Working Paper (2000) (http://papers.ssrn.com/sol3/papers.cfm?abstract_id=272929), which lists many examples of multiple victim public shootings.

[42] Olson & Maltz, *supra* note 1.

[43] John R. Lott, Jr. & David B. Mustard, Crime, Deterrence, and Right-to-Carry Concealed Handguns, 26 J Legal Stud. 1-68 (January 1997).

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 were later revoked for a crime prior to licensure, a rate of .058 percent.[44] Revoked licenses for any type of violations after licensure are also very rare: over the last 25 years, the permanent revocation rate for any reason, usually nothing that involves the gun, was only 0.14 percent.[45] Similar information for other states can be found in Lott's book.[46]

C) Increased gun ownership means more guns available for theft and thus that will lead to more crime with guns. (p. 10)

For 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 97 percent of first-time applicants for concealed-handgun permits already owned a handgun.[47] 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 rate of homes with guns.

While guns in a home might make burglary more attractive, it also makes it 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.[48] 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.[49]

D) Other Concerns: The Risks to Police, Accidents, and the Problem with Untrained Permit Holders[50]

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.[51] The Olson and Maltz evidence mentioned earlier implies fewer criminals carrying guns.

As for accidental gun deaths and gun suicides, two studies have examined this issue. Both studies find that the passage of right-to-carry laws does not affect either death rate. That result holds when

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[44] The most current numbers can be obtained from http://licgweb.dos.state.fl.us/stats/cw_monthly.html.
[45] These are revocations net of reinstatements for Florida.
[46] Lott, *supra* note 10, at 219-222.
[47] NRA poll: Salespeople No. 1 for permit applications, Dallas Morning News (April 19, 1996) at 32A.
[48] Lott, *supra* note 10, at 114.
[49] John R. Lott, Jr. The Bias Against Guns, Regnery: Washington, DC (2003).
[50] Ayres & Donohue, *supra* note 5, at 10 and 11.
[51] 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.

examining all people as well as those under age twenty.[52] While the Lott and Landes study finds that a few hours of training results in a greater reduction in multiple victim public shootings, the other Lott research finds no benefits for other types of crime.[53]

3) 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 (lines 3 and 6) – particularly for the full data set. 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 raise concerns about model mis-specification . . . ." (pp. 36-7).

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 Section II 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 post-passage dummy itself. For example, take their hybrid estimates for murder using the 1977 to 1997 sample in row 6 of Table 10. The post-passage dummy for murder equals 6.9 percent and is statistically significant at the 5 percent level. But for the first year that the law is in effect, the net effect on the crime rate is the sum of the 6.9 percent dummy plus the -3.5 percent post-law crime trend. The net effect in the first year is 3.4 percent, but with a standard error of 2.9, which is only 1.2 standard deviations from zero. In fact, none of Ayres and Donohue's hybrid estimates for murder, rape, or robbery in Tables 10 or 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 non-linear results provided by Lott found this pattern for aggravated assaults.[54]

As our own work in Section III has shown, Ayres and Donohue's claim about the "initial jump in crime" is very sensitive to the time frame chosen. In none of the specifications examining the 1977 to 2000 data is the post-passage dummy statistically significant and for overall violent crime, murder, and robbery the estimate is now even negative. In addition, since all the estimates reported in Section III indicate that the trend violent crime rates fell after the adoption of the law, the net effect during the first year after the laws 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, Section II in our paper has already 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

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[52] Lott, *supra* note 10, at 110-113, and John R. Lott, Jr. & John E. Whitley, Safe-Storage Gun Laws: Accidental Deaths, Suicides, and Crime, Yale Law School Working paper (1999) (http://papers.ssrn.com/sol3/papers.cfm?abstract_id=228534).
Lott & Whitley, *supra* note 1.
[53] Lott & Landes, *supra* note 41, and Lott, *supra* note 10, at 176-181.
[54] For example, Lott, *supra* note 10, at 79, Figure 4.9.

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.

4) Can Cocaine Use Explain the Results?

> "But an alternative explanation is that the crack cocaine problem drove up crime . . . the 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 crack. . . ." (p. 14)

One of Ayres and Donohue greatest concerns is the apparent failure of previous research to account for the differential geographical 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, crack cocaine has been addressed in even the earliest economics research on concealed handgun laws.[55]

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 cocaine (e.g., p. 201, fn. 8) did not alter the results.[56] Using yearly county-level pricing data (as opposed to short-run changes in prices) has the advantage of picking up cost and not demand differences between counties, thus measuring the differences in availability across counties.[57]

Research conducted by Steve Bronars and John Lott examined the crime rates for neighboring counties located within either 50 or 100 miles of each other and situated on either side of a state border.[58] When the counties adopting the law experienced a drop in violent crime, neighboring counties without right-to-carry laws directly on the other side of the border experienced an ***increase***. But 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

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[55] Ayres and Donohue ignore a previous comment on their review of Lott's book that also made all these points. See Lott, *supra* note 3.

[56] While they don't mention the use by Lott and Mustard of this variables, Ayres & Donohue, *supra* note 5, at 51, even imply a relationship between crack cocaine prices and crime when they mention "the greater than average price declines in the 1990s."

[57] 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.

[58] Because Bronars has been unwilling to share the data collected on geographic locations, we have recollected some of this information. Bronars & Lott, *supra* note 1.

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). Because Ayres and Donohue acknowledge the usefulness of this approach, we used that approach consistently throughout the regressions reported earlier in this paper.

Ayres and Donohue argue that the states that tend to adopt right-to-carry laws also "tend to be Republican and have high NRA membership and low crime rates" and are thus less typical of the states where crack was a problem (p. 46).[59] 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.

5) 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 extends beyond 1992). (p. 31)

Ayres and Donohue incorrectly describe Maltz and Targonski's conclusions. Maltz and Targonski point out that not all police agencies report their crime data.[60] 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. But 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 30% under-reporting or greater.[61] 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. And there is no discussion of a post 1992 break in the quality of data. Instead,

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[59] Ayres and Donohue claim their own results show the opposite: states that adopted the law had "substantially higher" crimes rate prior to adoption (p. 27).
[60] Maltz, Michael & Targonski, Joseph, A Note on the Use of County-Level UCR Data, 18 J Quantitative Criminology 297-318 (September 2002).
[61] John R. Lott, Jr. & John Whitley, Measurement Error in County-Level UCR Data: A Response, J Quantitative Criminology, forthcoming. 2003.

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. Nor do Maltz and Targonski provide any evidence that state level data are more dependable than county level data.[62] The aggregation bias discussed in Section III is also relevant.

6) Is the US Murder Rate "Exceptional"?

> "the United States is exceptional in only one aspect of its crime problem – its high rate of lethal violence – it might at first appear that guns must be part of the problem." (p. 8)

Despite the common perception, the United States does not have one of the higher lethal violence rates in the world. In fact, the United States is not even close. In 2000, the US had a murder rate of 5.5 per 100,000 people, Brazil 26.3 and Russia 23.[63] Russia has had a ban on handguns since the 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 U.S.'s.[64] On the other extreme, countries such as Israel and Switzerland have high gun ownership rates and low homicide rates.[65] Jeff Miron recently examined homicide rates across 44 countries and found that the countries with the strictest gun control laws tended to have higher homicide rates.[66]

7) Should Philadelphia be treated differently than 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." (p. 38)

In fact, Philadelphia was only "partially exempted."[67] 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 percent, the number of concealed handgun permits in Philadelphia increased by over 70 percent. 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.[68] Montgomery ranked first in the state with a 253 percent increase, Bucks second with a 241 percent

[62] 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.
[63] Violent Deaths in Brazil Second Only to Columbia, UNESCO Study Shows, Associated Press (Dateline: Brasilia, Brazil) (May 4, 2002).
[64] Jeffrey Miron, Violence, Guns, and Drugs: A Cross-Country Analysis, 44 J Law & Econ. 615-634 (October 2001) and Lott, *supra* note 49, at Chapter 4.
[65] Lott, *supra* note 49.
[66] Miron, supra note 64, at 624.
[67] Lott, *supra* note 10, at 152.
[68] Source Dr. Alan S. Krug, member of Pennsylvania Governor's Sportsmen's Advisory Council.

increase. However, by the time that the rules were changed for Philadelphia, most of the demand in these counties had been satiated.

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

V. 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 percent 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 range between about $2 billion 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 the new results provided here 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 increases initially after right-to-carry laws were adopted and to misinterpret the significance of their own estimates that examined the year-by-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-by-year estimates imply that crime rates were significantly greater should 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 how 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.

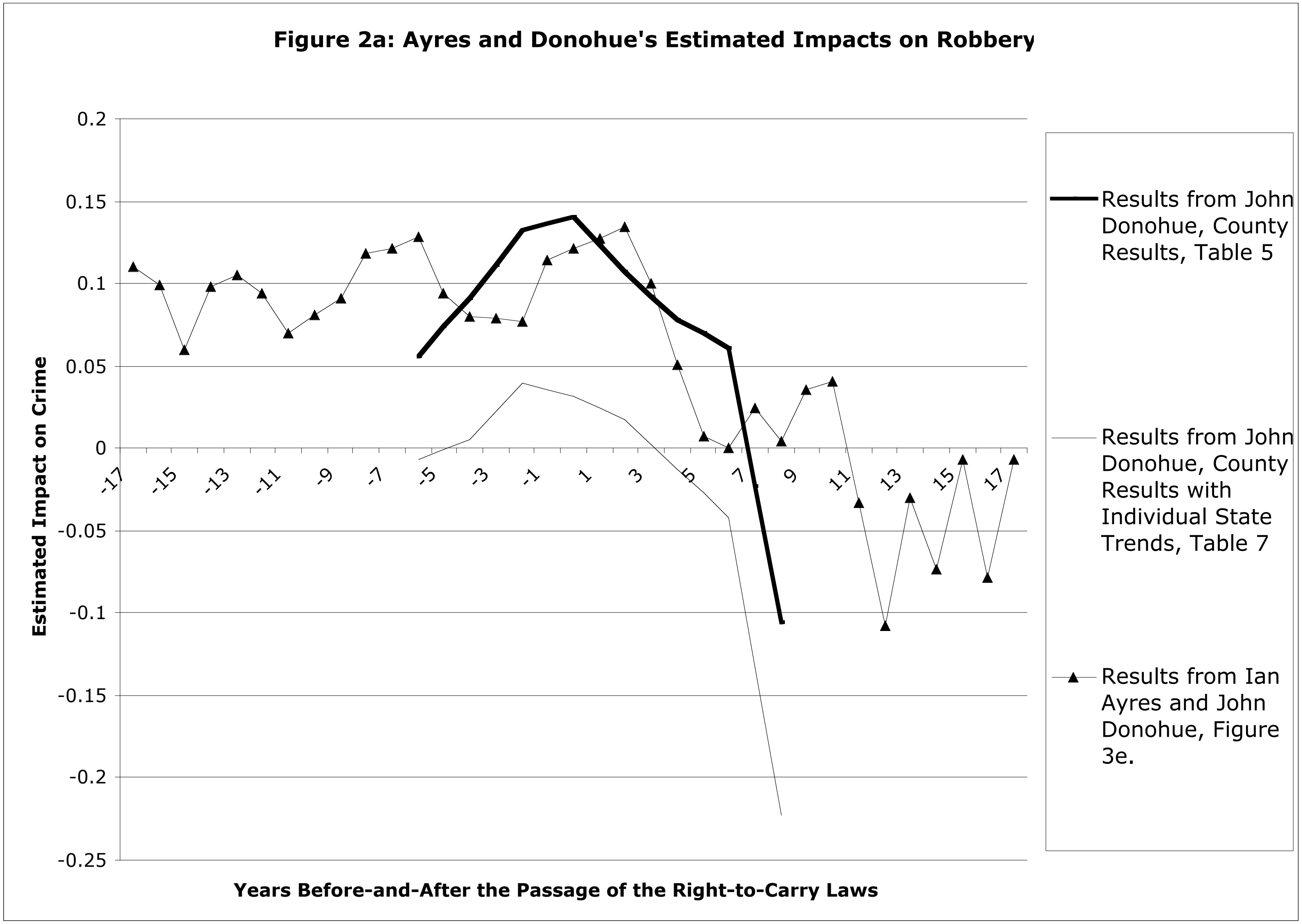

Figure 2a: Ayres and Donohue's Estimated Impacts on Robbery
Estimated Impact on Crime
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
-0.25
-17
-15
-13
-11
-9
-7
-5
-3
-1
1
3
5
7
9
11
13
15
17
Years Before-and-After the Passage of the Right-to-Carry Laws
Results from John Donohue, County Results, Table 5
Results from John Donohue, County Results with Individual State Trends, Table 7
Results from Ian Ayres and John Donohue, Figure 3e.

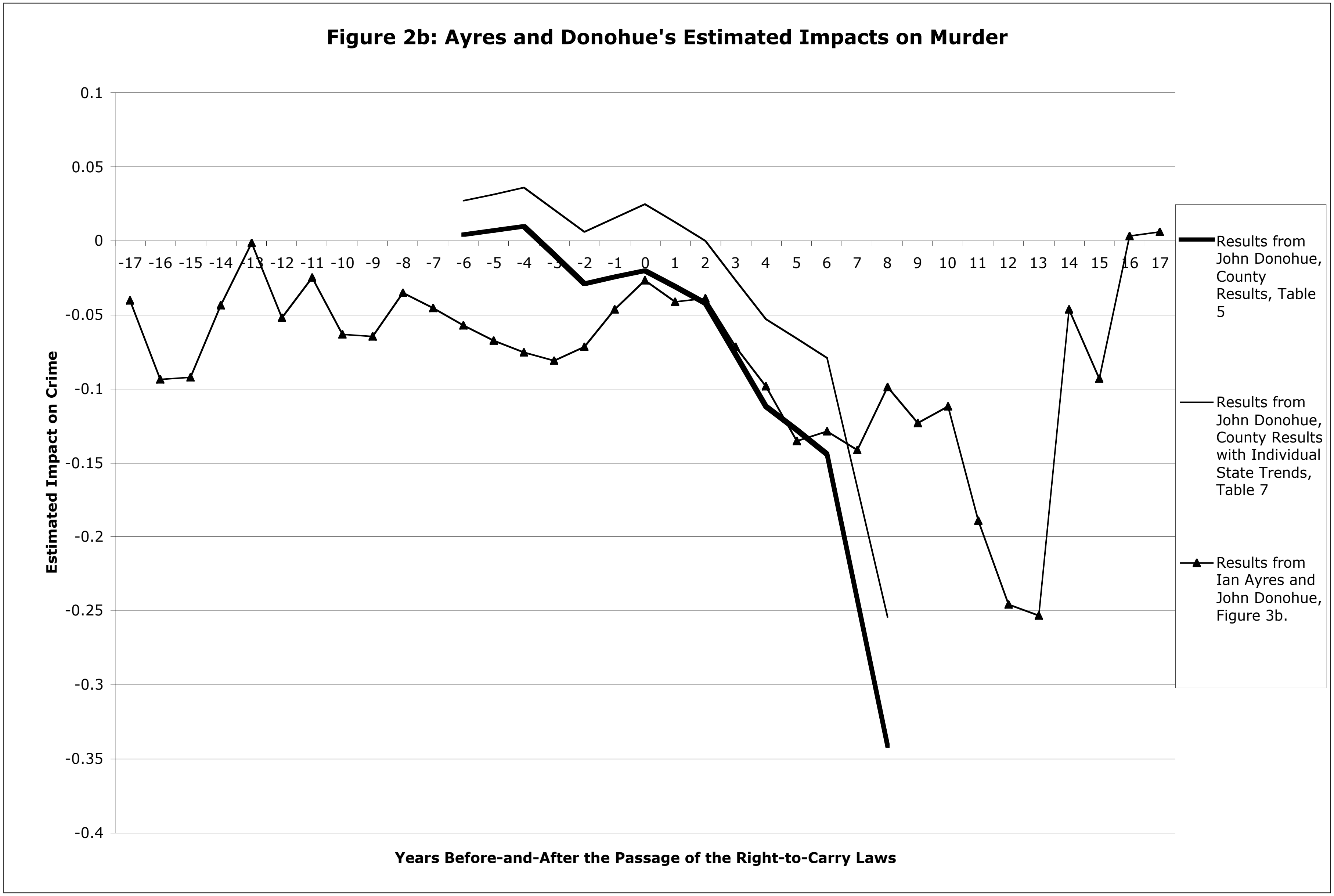
Figure 2b: Ayres and Donohue's Estimated Impacts on Murder
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
-0.25
-0.3
-0.35
-0.4
Estimated Impact on Crime
-17 -16 -15 -14 -13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
Years Before-and-After the Passage of the Right-to-Carry Laws
Results from John Donohue, County Results, Table 5
Results from John Donohue, County Results with Individual State Trends, Table 7
Results from Ian Ayres and John Donohue, Figure 3b.

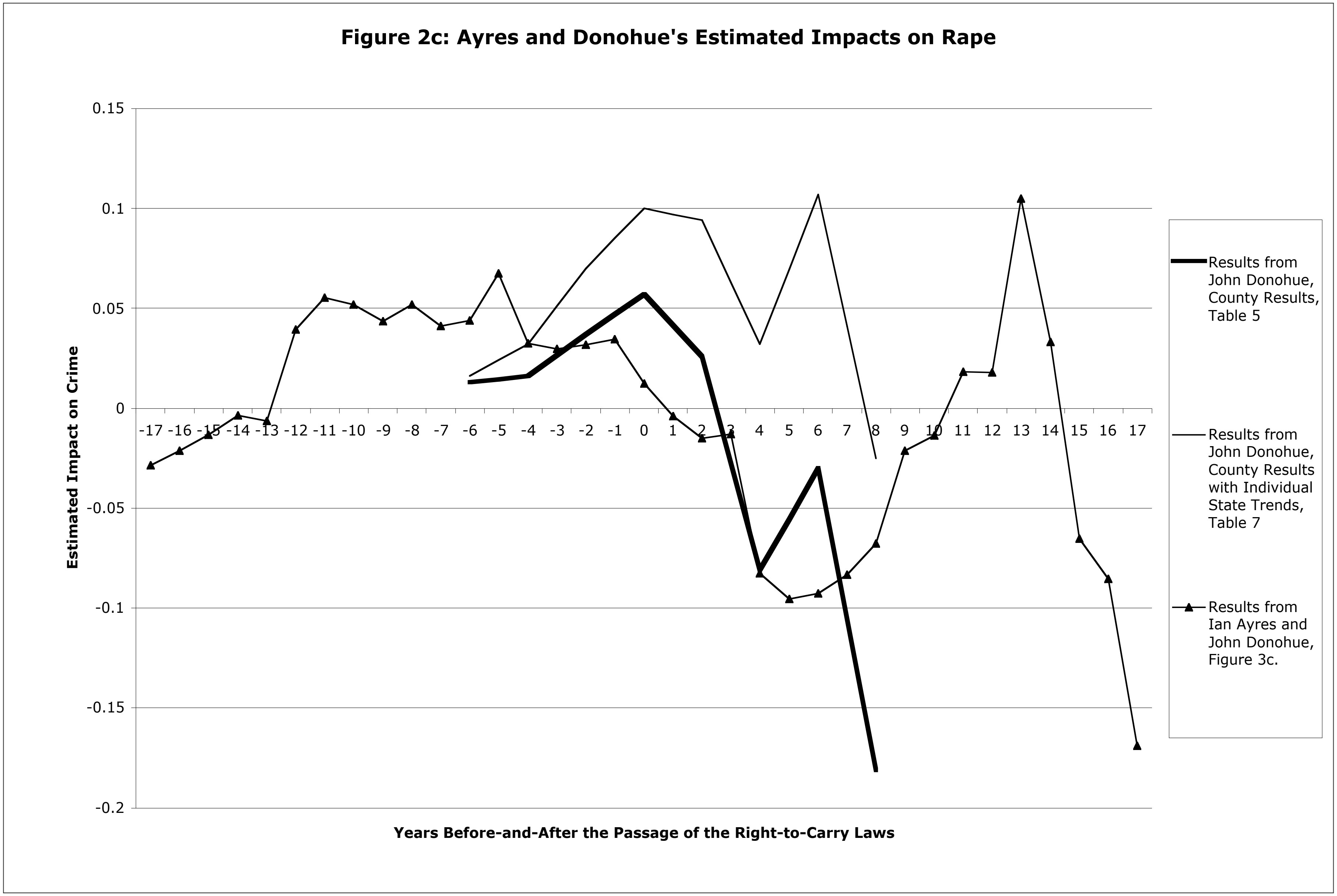

Figure 2c: Ayres and Donohue's Estimated Impacts on Rape
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
Estimated Impact on Crime
-17 -16 -15 -14 -13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
Years Before-and-After the Passage of the Right-to-Carry Laws
Results from John Donohue, County Results, Table 5
Results from John Donohue, County Results with Individual State Trends, Table 7
Results from Ian Ayres and John Donohue, Figure 3c.

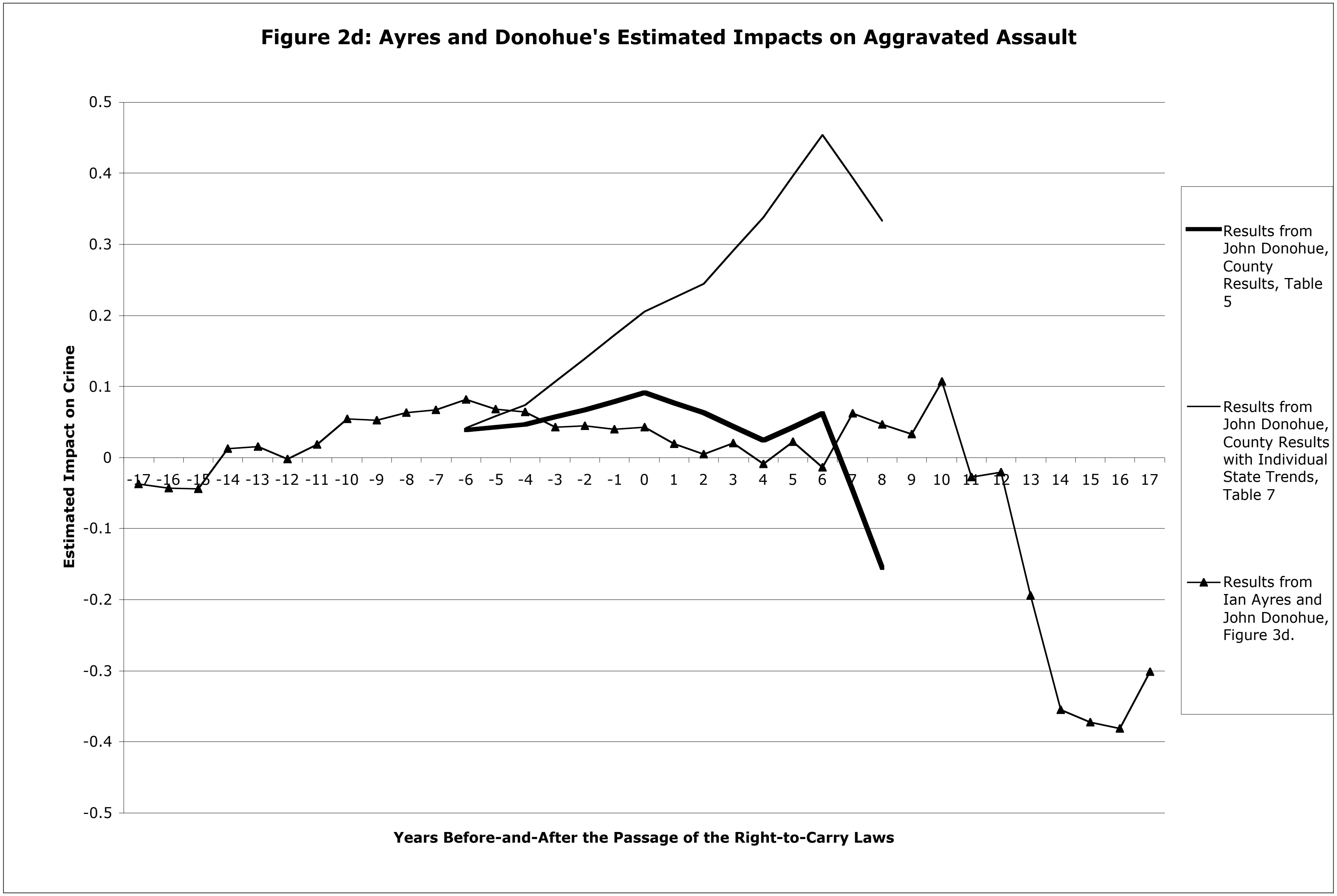
Figure 2d: Ayres and Donohue's Estimated Impacts on Aggravated Assault
Estimated Impact on Crime
0.5
0.4
0.3
0.2
0.1
0
-0.1
-0.2
-0.3
-0.4
-0.5
-17 -16 -15 -14 -13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17
Years Before-and-After the Passage of the Right-to-Carry Laws
Results from John Donohue, County Results, Table 5
Results from John Donohue, County Results with Individual State Trends, Table 7
Results from Ian Ayres and John Donohue, Figure 3d.

Table 1 Cost-Benefit Analyses of Ayres and Donohue's County-Level Regressions: Five year average estimate of Net Costs/Benefits

Table	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	-1687.1	31522.3	30748.6	5683.1	176804.4
		% 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

Table	Model		Total	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny
Table 1	(4) Dummy	# crimes		-684	-1,116	-703	-242	53,825	17,551	302,562
County data,		% crimes		-7.8%	-2.9%	-0.4%	-0.1%	10.8%	1.5%	9.6%
The same as		$ cost/benefit	-$1,875.7	-$2,114.5	-$102.2	-$5.9	-$6.1	$209.5	$25.8	$117.7
Ayres and	(5) Spline	# crimes		-1,029	-2,021	-12,480	11,460	-2,920	-57,307	34,183
Donohue's		% crimes		-11.7%	-5.3%	-6.7%	3.0%	-0.6%	-4.8%	1.1%
Table 10		$ 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	(4) Dummy	# crimes		-398	-1,799	-13,603	-22,972	29,096	-50,133	21,255
State Data		% 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	(4) Dummy	# crimes		3	1,049	606	28,136	20,154	4,536	130,920
County data,		% crimes		0.0%	2.7%	0.3%	7.3%	4.1%	0.4%	4.1%
Includes state		$ cost/benefit	$955.9	$8.5	$96.0	$5.1	$710.4	$78.4	$6.7	$50.9
trends, the	(5) Hybrid	# crimes		-519	-561	-9,406	30,404	4,451	-44,290	117,267
same as		% crimes		-5.9%	-1.5%	-5.0%	7.9%	0.9%	-3.7%	3.7%
Ayres and Donohue's Table 11		$ cost/benefit	-$969.7	-$1,604.6	-$51.3	-$79.2	$767.6	$17.3	-$65.2	$45.6
Table 4	(4) Dummy	# crimes		-166	-441	-1,218	485	22,720	7,890	63,567
State Data,		% crimes		-1.9%	-1.2%	-0.7%	0.1%	4.6%	0.7%	2.0%
Includes state		$ cost/benefit	-$425.6	-$512.0	-$40.3	-$10.2	$12.2	$88.4	$11.6	$24.7
trends	(5) 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	-$46.1	$11.6

Table 2 (Continued): Cost-Benefit Analyses of County and State Level Regressions in Donohue's Brookings Paper: Five year average estimate of Net Costs/Benefits

Table	Model		Total	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny
Table 5	Year	# crimes		-511	-2,921	-8,560	-15,818	9,256	-20,082	170,691
County Data	dummies	% crimes		-5.8%	-7.6%	-4.6%	-4.1%	1.9%	-1.7%	5.4%
		$ cost/benefit	-$2,244.8	-$1,579	-$267	-$72	-$399	$36	-$30	$66
Table 6 State	Year	# crimes		-589	-2,374	-17,811	-19,492	-11,346	-79,493	-5,058
Data	dummies	% crimes		-6.7%	-6.2%	-9.6%	-5.0%	-2.3%	-6.7%	-0.2%
		$ cost/benefit	-$2,843.6	-$1,821	-$217	-$150	-$492	-$44	-$117	-$2
Table 7	Year	# crimes		-454	-1,106	-5,856	36,238	-9,554	-64,909	227,272
County Data	dummies	% crimes		-5.2%	-2.9%	-3.1%	9.4%	-1.9%	-5.4%	7.2%
Includes state trends		$ cost/benefit	-$684.7	-$1,405	-$101	-$49	$915	-$37	-$96	$88
Table 8	Year	# crimes		-841	-3,032	-32,116	7,890	-56,281	-110,214	-90,087
State Data	dummies	% crimes		-9.6%	-7.9%	-17.2%	2.0%	-11.3%	-9.2%	-2.9%
Includes state trends		$ cost/benefit	-$3,364.7	-$2,600	-$278	-$270	$199	-$219	-$162	-$35
Average		# crimes		-589	-1,363	-11,056	7,955	4,868	-36,314	93,719
predicted		% crimes		-6.7%	-3.6%	-5.9%	2.1%	1.0%	-3.0%	3.0%
change		$ cost/benefit	-$1,837	-$1,822	-$125	-$93	$201	$19	-$54	$36
Average $Cost/Benefit	Dummy		-$847	-$962	-$53	-$31	$34	$122	-$7	$50
Predicted	Spline		-$3,043.3	-$2,831.5	-$164.95	-$125.85	$ 211.9	-$57.2	-$80.6	$ 4.9
Change by	Hybrid		-$1,775.0	-$2,146.5	-$85.38	-$95.98	$ 507.2	$ 38.6	-$38.3	$ 45.3
Type	Year Dummies		-$2,284.5	-$1,851.3	-$215.8	-$135.3	$ 55.8	-$66.0	-$101.3	$ 29.2

Figure 4a: Murder: Weighted Least Squares Estimates Using County Level Data from 1977 to 2000

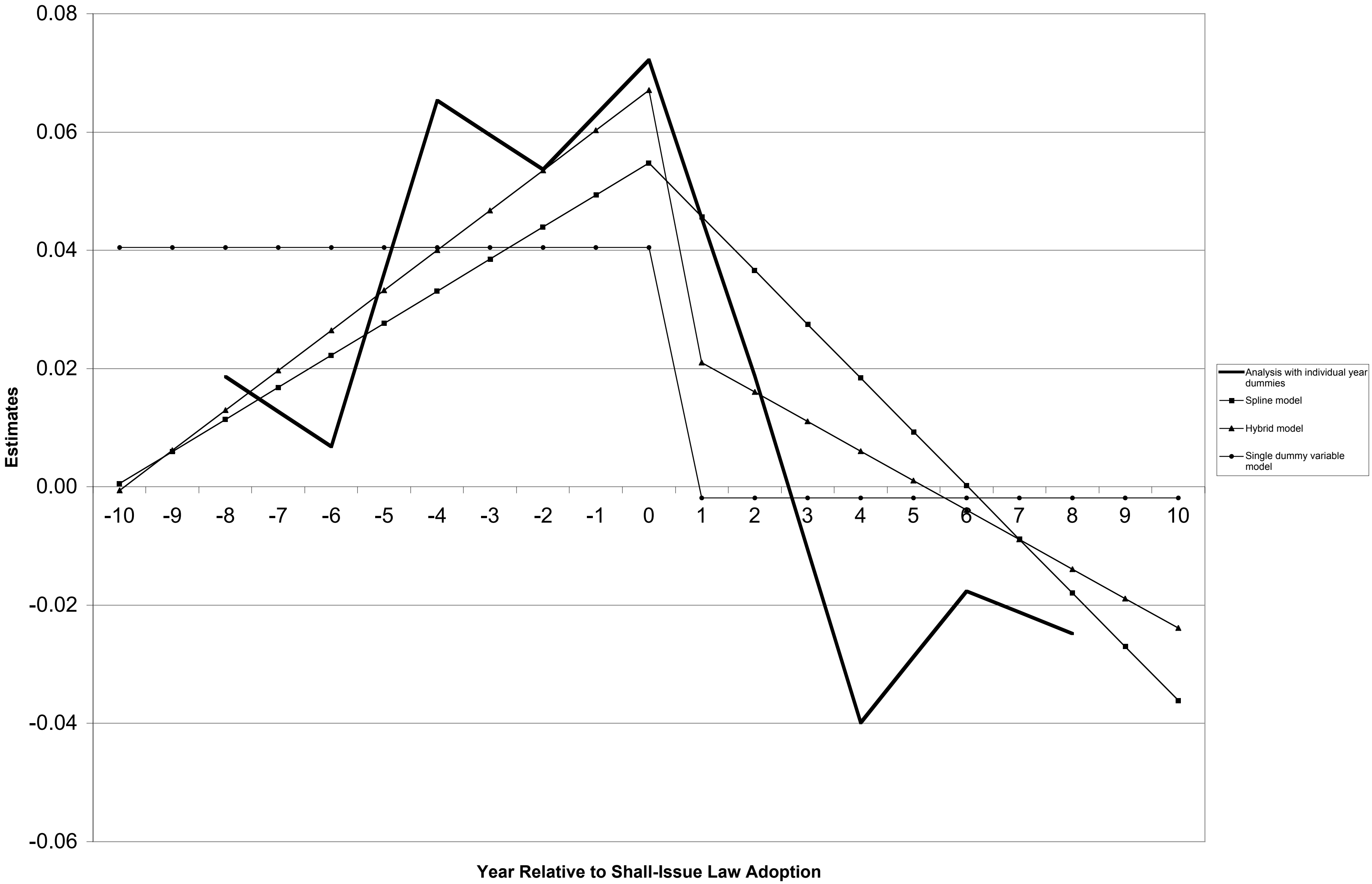

Figure 4b: Rape: Weighted Least Squares Estimates Using County Level Data from 1977 to 2000

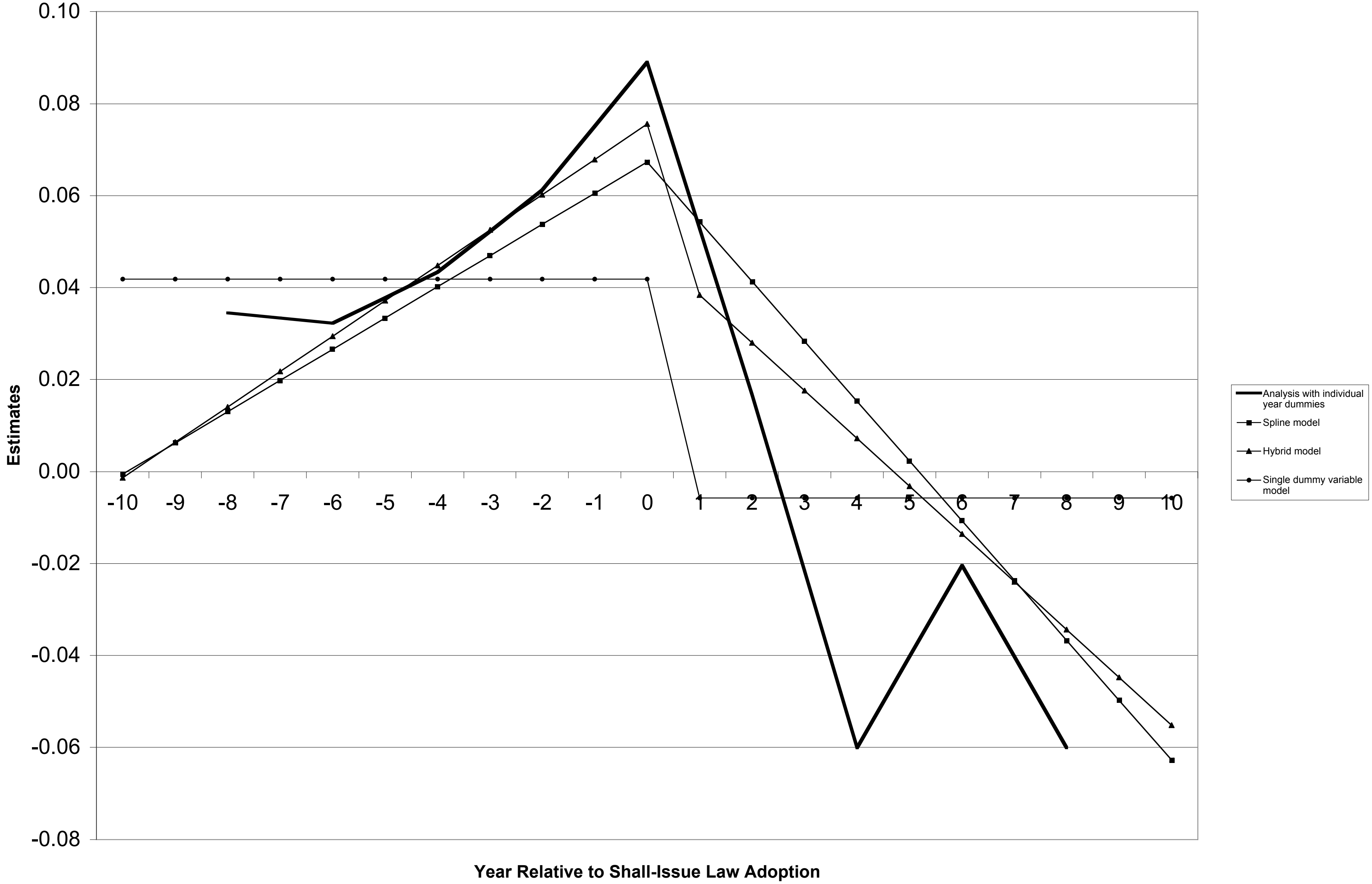

Figure 4c: Robbery: Weighted Least Squares Estimates Using County Level Data from 1977 to 2000

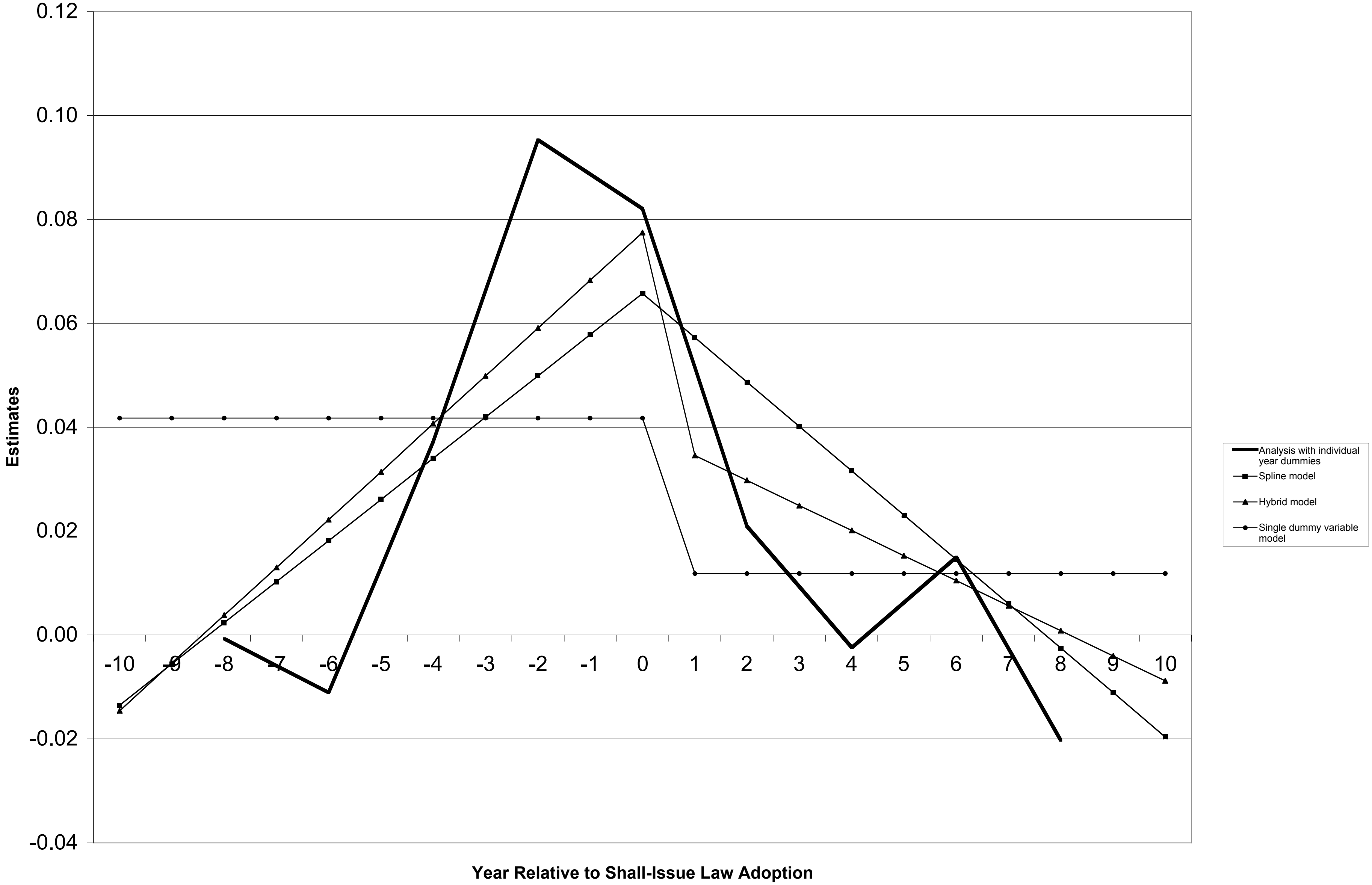

Figure 4d: Aggravated Assault: Weighted Least Squares Estimates Using County Level Data from 1977 to 2000

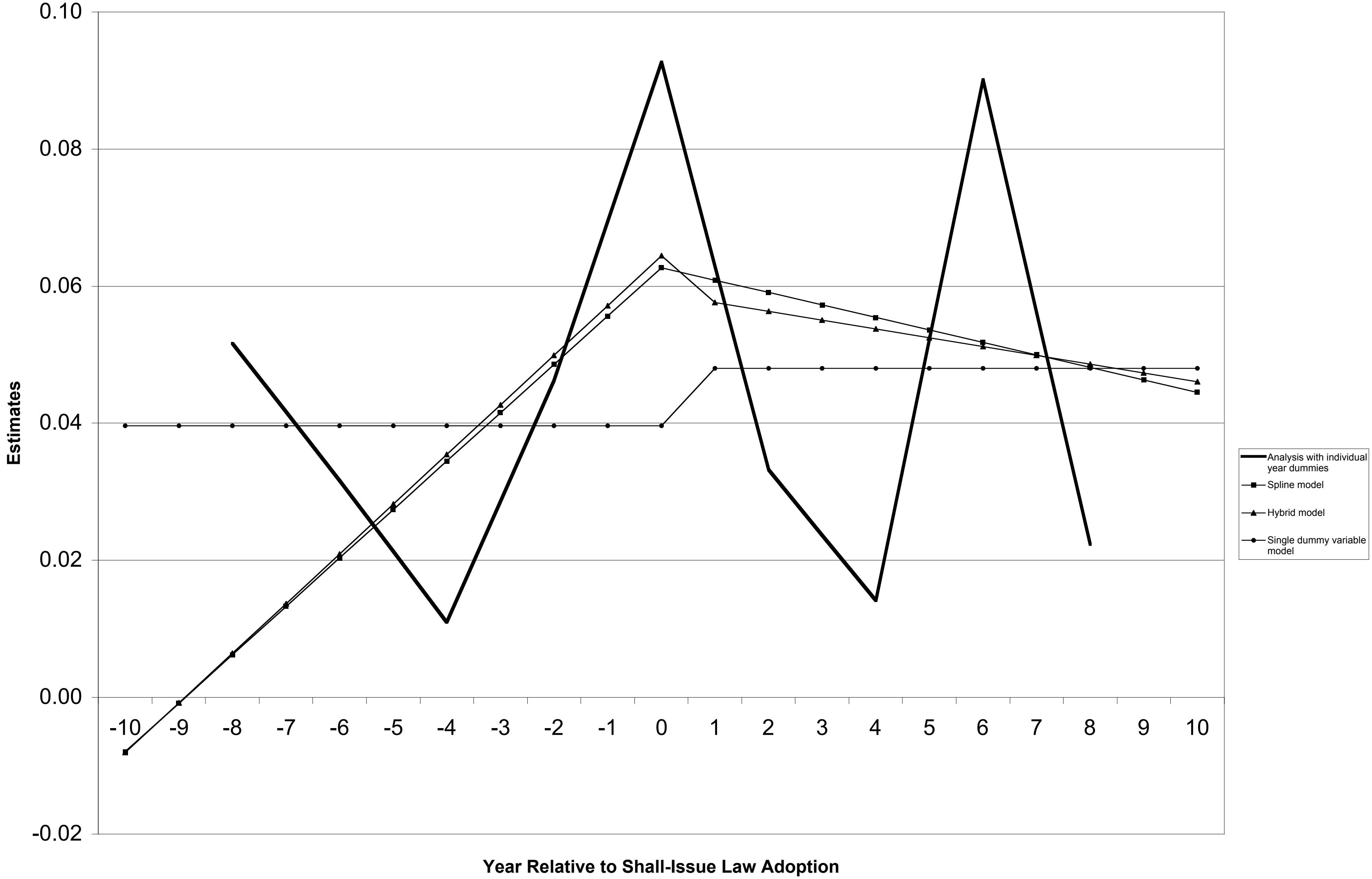

Figure 4e: Property Crimes: Weighted Least Squares Estimates Using County Level Data from 1977 to 2000

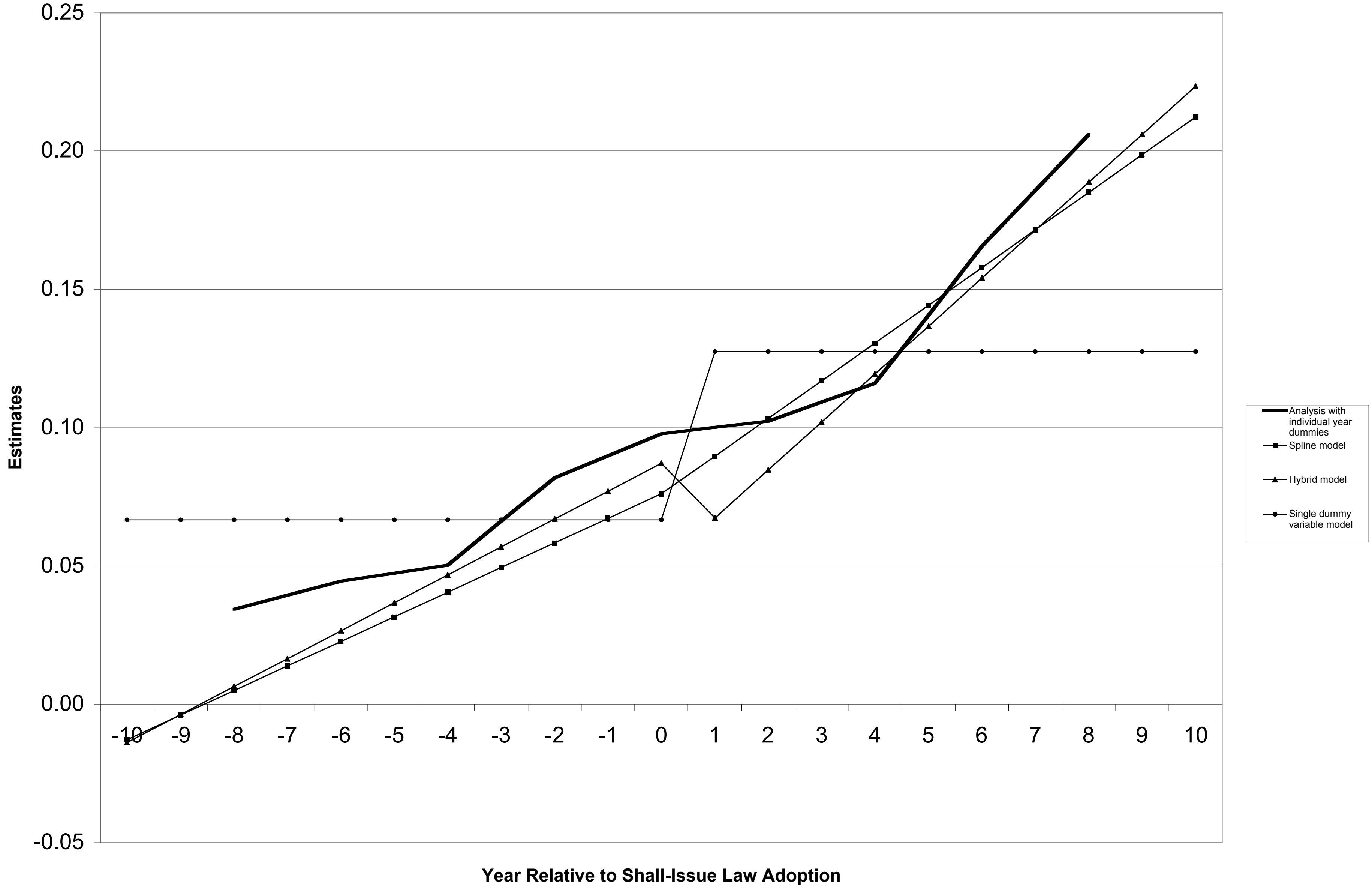

Table 3a: Ayres: Comparison of the three dummy specifications used by Ayres and Donohue (Our model includes region-specific year dummies and uses 1977 – 2000 data)

	Violent crime	Murder	Rape	Robbery	Aggravated Assault	Property crimes	Auto Theft	Burglary	Larceny
	Single dummy variable model								
Post-passage dummy	**-2.8%**	**-6.2%**	**-6.5%**	**-5.4%**	-1.6%	**4.1%**	**9.0%**	0.4%	**6.0%**
	(1.1%)	(1.5%)	(1.2%)	(1.3%)	(1.2%)	(0.9%)	(1.5%)	(1.0%)	(0.9%)
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%**	**0.6%**	**0.6%**	**0.6%**	**0.8%**	**1.1%**	**0.9%**	**1.1%**
	(0.1%)	(0.2%)	(0.1%)	(0.2%)	(0.2%)	(0.1%)	(0.2%)	(0.1%)	(0.1%)
Trend after	**-0.5%**	**-2.0%**	**-2.3%**	**-2.0%**	**-1.3%**	**0.7%**	**0.7%**	**-1.1%**	0.2%
	(0.2%)	(0.3%)	(0.3%)	(0.3%)	(0.3%)	(0.2%)	(0.3%)	(0.2%)	(0.2%)
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	-0.0092
F-test statistic	10.19	34.00	82.57	55.57	29.33	0.41	1.21	53.61	10.73
Prob > *F*	0.1%	0.0%	0.0%	0.0%	0.0%	52.2%	29.0%	0.0%	0.1%
	Hybrid model								
Trend before	0.2%	**0.4%**	**0.5%**	**0.7%**	**0.5%**	**0.9%**	**0.9%**	**0.7%**	**1.0%**
	(0.1%)	(0.2%)	(0.1%)	(0.2%)	(0.2%)	(0.1%)	(0.2%)	(0.1%)	(0.1%)
Post-passage dummy	**-2.6%**	-0.7%	0.7%	-0.2%	3.1%	-1.4%	**6.2%**	**3.7%**	**4.3%**
	(1.2%)	(2.5%)	(2.0%)	(2.0%)	(2.1%)	(1.0%)	(2.0%)	(1.5%)	(1.6%)
Trend after	-0.2%	**-1.9%**	**-2.4%**	**-2.0%**	**-1.7%**	**0.9%**	0.1%	**-1.5%**	-0.3%
	(0.3%)	(0.5%)	(0.4%)	(0.4%)	(0.4%)	(0.2%)	(0.4%)	(0.3%)	(0.4%)
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	-0.0263	-0.0211	-0.0001	-0.0083	-0.0225	-0.0123
F-test statistic	4.01	21.32	53.77	36.32	23.66	0.00	3.55	43.34	11.81
Prob > *F*	4.5%	0.0%	0.0%	0.0%	0.0%	97.1%	5.9%	0.0%	0.1%
	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

Note: Robust 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.

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: Limited set of demographics and Lagged Per Capita Prison Population, 1977 – 2000 data

	Violent crime	Murder	Rape	Robbery	Assault	Property crimes	Auto Theft	Burglary	Larceny
Post-passage dummy	**-2.8%**	**-6.7%**	**-3.2%**	**-4.1%**	-1.9%	**4.8%**	**9.7%**	0.4%	**6.4%**
	(1.0%)	(1.6%)	(1.7%)	(1.2%)	(1.2%)	(1.0%)	(1.3%)	(1.0%)	(1.0%)
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%**	**1.0%**	**1.1%**	**1.3%**
	(0.1%)	(0.2%)	(0.2%)	(0.2%)	(0.2%)	(0.1%)	(0.2%)	(0.1%)	(0.1%)
Trend after	**-0.6%**	**-1.9%**	**-1.9%**	**-1.9%**	**-1.2%**	**0.9%**	**1.0%**	**-1.0%**	0.4%
	(0.2%)	(.4%)	(.3%)	(.3%)	(.3%)	(0.2%)	(0.3%)	(0.2%)	(.3%)
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	0.0002	0.0006	-0.0209	-0.0099
F-test statistic	22.2	22.39	74.20	57.53	32.22	0.02	0.02	48.02	9.90
Prob > *F*	0%	0%	0%	0%	0%	89.88%	88.48%	0%	.17%
	Hybrid model								
Trend before	**0.5%**	0.4%	**1.1%**	0.9%	**0.9%**	**1.0%**	**0.8%**	**1.0%**	**1.2%**
	(0.1%)	(0.2%)	(0.2%)	(0.2%)	(0.2%)	(0.1%)	(0.2%)	(0.1%)	(0.1%)
Post-passage dummy	**-3.6%**	-1.4%	1.8%	1.1%	0.4%	**-2.2%**	**6.3%**	2.3%	2.9%
	(1.2%)	(2.5%)	(2.0%)	(1.9%)	(2.0%)	(1.0%)	(1.9%)	(1.5%)	(1.5%)
Trend after	-0.2%	**-1.7%**	**-2.1%**	**-2.0%**	**-1.2%**	**1.2%**	0.4%	**-1.3%**	0.0%
	(0.2%)	(.5%)	(.4%)	(.4%)	(.4%)	(0.2%)	(.4%)	(.4%)	(.4%)
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	8.94	13.76	52.06	40.43	22.08	0.81	0.76	37.55	9.59
Prob > F	.28%	.02%	0%	0%	0%	36.78%	38.34%	0%	.20%
	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

Note: Robust 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.

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 4: Early and late adopters, full set of demographics, 1977 – 2000 data:
Early adopters are states that adopted the right-to-carry laws between 1977 and 1992, and late adopters enacted the law after 1992

	Violent crime	Murder	Rape	Robbery	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%**	**1.0%**	**1.2%**
	(0.2%)	(0.3%)	(0.2%)	(.3%)	(.2%)	(0.1%)	(0.3%)	(0.2%)	(0.2%)
Trend after	0.1%	**-1.9%**	**-2.3%**	**-1.7%**	-0.3%	**0.7%**	**0.9%**	**-1.4%**	0.0%
	(.2%)	(.4%)	(0.3%)	(.3%)	(.3%)	(0.2%)	(0.4%)	(0.3%)	(0.3%)
Late adopters									
Trend before	**0.3%**	**0.5%**	**0.6%**	**0.8%**	**0.8%**	**0.8%**	**1.1%**	**0.6%**	**1.0%**
	(0.1%)	(0.2%)	(0.2%)	(0.2%)	(0.2%)	(0.1%)	(0.2%)	(0.1%)	(0.1%)
Trend after	0.3%	**-2.0%**	**-2.5%**	**-2.2%**	0.6%	1.0%	**1.6%**	0.6%	0.6%
	(.6%)	(.8%)	(.6%)	(.7%)	(.7%)	(.5%)	(.8%)	(.6%)	(.5%)
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	0.91	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	4.33	12.87	40.36	16.35	1.47	0.27	0.00	42.70	9.45
Prob > F	3.74%	.03%	0%	.01%	22.54%	60.08%	95.94%	0%	.21%
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	7.22	19.66	16.08	.07	0.23	0.38	0.00	0.37
Prob > F	96.21%	.72%	0%	.01%	78.68%	62.95%	53.52%	97.68%	54.10%

Note: Robust standard errors are shown in parentheses. The same set of control variables are used as used 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.

Table 5: Test whether the adoption of shall-issue laws in 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%**
	(0.2%)	(0.2%)	(0.2%)	(0.2%)	(0.2%)	(0.1%)	(0.2%)	(0.1%)	(0.1%)
Trend after	-0.2%	**-1.7%**	**-2.1%**	**-1.3%**	**-1.0%**	**0.7%**	**1.0%**	**-0.9%**	0.3%
	(0.2%)	(0.4%)	(0.3%)	(0.3%)	(.3%)	(0.2%)	(0.3%)	(0.2%)	(0.2%)
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	.18	16.79	48.11	16.40	13.00	0.38	0.28	37.70	5.71
Prob > F	66.95%	0%	0%	.01%	.03%	53.72%	59.70%	0%	1.69%
	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	-0.1%	0.2%	0.4%	0.3%	0.3%	**1.0%**	**0.9%**	**1.0%**	**1.2%**
	(0.2%)	(0.3%)	(0.2%)	(0.2%)	(0.2%)	(0.1%)	(0.3%)	(0.2%)	(0.2%)
Trend after	-0.2%	**-1.6%**	**-2.1%**	**-1.2%**	**-1.1%**	**0.6%**	**0.9%**	**-1.1%**	0.1%
	(0.2%)	(0.4%)	(0.3%)	(0.3%)	(3.1%)	(0.2%)	(0.4%)	(0.2%)	(0.2%)
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	-0.01465	-0.0145	-0.0043	-0.0000	-0.0207	-0.0115
F-test statistic	0.06	10.99	35.27	11.20	11.78	2.68	0.00	42.90	12.32
Prob > F	80.01%	.09%	0%	.08%	.06%	10.19%	99.77%	0%	.04%

Note: Robust standard errors are shown in parentheses. The same set of control variables are used as used 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.

Table 6: State-by-State Breakdown: Spline Model, 1977-2000 <u>county level data</u>, with regional year fixed effects

	Murder	Rape	Robbery	Assault	Auto Theft	Burglary	Larceny
Maine	**<u>12.5%</u>**	-0.40%	**8.0%**	**<u>13.7%</u>**	5.6%	**-4.3%**	**<u>3.1%</u>**
	(9.2)	(0.0)	(5.5)	(23.2)	(2.3)	(11.5)	(9.5)
Florida	**<u>6.7%</u>**	**3.5%**	**<u>8.9%</u>**	**<u>4.8%</u>**	**<u>6.6%</u>**	**<u>5.0%</u>**	**<u>4.2%</u>**
	(12.0)	(4.8)	(24.7)	(9.9)	(11.8)	(14.2)	(9.7)
Virginia	<u>-3.4%</u>	-0.40	-0.2%	-0.7%	-3.2%	1.4%	-0.1%
	(3.2)	(0.1)	(0.0)	(0.2)	(1.9)	(2.6)	(0.0)
Georgia	**-3.8%**	-1.3%	**-3.9%**	**-3.3%**	**<u>-4.3%</u>**	-0.4%	-0.6%
	(4.4)	(0.5)	(6.7)	(4.7)	(6.9)	(0.1)	(0.2)
Pennsylvania	**<u>-5.6%</u>**	-1.8%	**<u>-7.7%</u>**	**<u>-6.3%</u>**	**-5.1%**	**<u>-2.6%</u>**	**<u>-4.1%</u>**
	(9.9)	(2.2)	(24.3)	(30.8)	(6.8)	(9.9)	(26.1)
West Virginia	2.2%	<u>-4.8%</u>	<u>-11.6%</u>	**<u>-11.4%</u>**	-2.0%	**<u>-3.2%</u>**	-2.0%
	(0.6)	(3.5)	(3.5)	(20.0)	(1.4)	(7.0)	(2.5)
Idaho	-2.8%	<u>-5.8%</u>	**-10.2%**	**-5.0%**	-2.4%	-2.0%	-0.8%
	(0.4)	(3.7)	(12.3)	(5.7)	(1.2)	(1.9)	(0.4)
Mississippi	**-5.4%**	**-5.6%**	**<u>-8.9%</u>**	**<u>-8.5%</u>**	**<u>-9.7%</u>**	**<u>-6.0%</u>**	**-5.0%**
	(5.0)	(7.0)	(16.6)	(9.0)	(14.3)	(9.4)	(5.6)
Oregon	**5.8%**	0.7%	2.7%	**<u>-11.1%</u>**	**5.0%**	**<u>3.8%</u>**	-0.1%
	(4.0)	(0.2)	(1.9)	(17.3)	(8.1)	(8.4)	(0.0)
Montana	-6.0%	-18.9%	**-38.5%**	**<u>-51.0%</u>**	**-17.9%**	-6.8%	<u>-13.3%</u>
	(0.2)	(1.9)	(4.9)	(22.1)	(5.6)	(1.2)	(2.9)
Alaska	14.4%	1.2%	7.0%	**10.6%**	<u>-8.0%</u>	<u>-8.7%</u>	1.1%
	(2.1)	(0.1)	(1.8)	(5.3)	(3.8)	(2.8)	(0.1)
Arizona	1.8%	**-5.4%**	**<u>6.4%</u>**	1.6%	**12.4%**	**4.2%**	**<u>6.5%</u>**
	(0.4)	(6.6)	(8.1)	(0.5)	(8.2)	(5.1)	(12.8)
Tennessee	**5.2%**	1.7%	-2.8%	**<u>10.2%</u>**	1.7%	-0.3%	1.4%
	(6.2)	(1.0)	(2.7)	(24.5)	(0.6)	(0.0)	(0.9)
Wyoming	-1.7%	**-8.1%**	**-9.2%**	2.8%	**<u>-16.0%</u>**	1.5%	-0.9%
	(0.1)	(5.2)	(4.9)	(0.9)	(35.2)	(0.6)	(0.3)
Arkansas	**6.2%**	2.2%	0.8%	**<u>6.8%</u>**	**<u>5.1%</u>**	**<u>4.2%</u>**	0.7%
	(6.5)	(1.4)	(0.2)	(9.0)	(11.8)	(15.7)	(0.5)
Nevada	-1.2%	-0.4%	0.9%	<u>-7.4%</u>	**6.4%**	1.9%	2.7%
	(0.2)	(0.0)	(0.1)	(3.3)	(4.2)	(0.7)	(1.3)
North Carolina	**<u>-8.6%</u>**	**<u>7.1%</u>**	**<u>21.4%</u>**	**<u>-7.4%</u>**	**<u>10.2%</u>**	**<u>15.1%</u>**	**<u>6.9%</u>**
	(31.0)	(23.8)	(208.5)	(35.5)	(51.5)	(199.1)	(49.0)
Oklahoma	2.8%	-0.6%	**<u>-11.0%</u>**	1.8%	**<u>-9.2%</u>**	**<u>-3.7%</u>**	**-2.7%**
	(1.0)	(0.1)	(32.2)	(1.2)	(21.2)	(9.2)	(5.0)
Texas	**-5.2%**	**<u>-3.6%</u>**	-1.6%	**3.6%**	-0.5%	**-3.7%**	-0.4%
	(12.3)	(7.0)	(1.4)	(5.3)	(0.1)	(4.4)	(0.1)
Utah	-0.1%	**<u>11.7%</u>**	-2.0%	<u>-4.7%</u>	**-8.1%**	-0.3%	-1.3%
	(0.0)	(20.8)	(0.6)	(3.4)	(7.9)	(0.0)	(0.7)
Kentucky	-0.5%	**<u>-15.9%</u>**	**<u>-13.6%</u>**	<u>8.1%</u>	**<u>-15.4%</u>**	**<u>-11.7%</u>**	**<u>-17.8%</u>**
	(0.0)	(28.6)	(25.7)	(3.1)	(17.1)	(23.2)	(55.4)
Louisiana	-0.1%	**<u>-8.3%</u>**	2.4%	<u>-4.5%</u>	3.8%	3.2%	-1.6%
	(0.0)	(16.7)	(0.7)	(3.3)	(2.1)	(2.5)	(0.7)
South Carolina	<u>-4.2%</u>	**<u>4.8%</u>**	**<u>15.7%</u>**	<u>-2.8%</u>	**<u>5.8%</u>**	-0.8%	0.1%
	(3.4)	(8.3)	(71.8)	(2.9)	(7.3)	(0.3)	(0.0)
Number of observations	37080	49633	49651	62487	63463	65498	65490
R^2	0.81	0.76	0.914	0.807	0.847	0.81	0.802
Number significantly positive	5	4	5	7	7	5	4
Number insignificantly positive	4	4	5	3	3	4	5
Number significantly negative	7	8	9	12	8	8	5
Number insignificantly negative	7	7	4	1	5	6	9

Table 7: Poisson Estimates: All socioeconomic variables and the arrest rate of violent crime

	Murder	Rape	Robbery
Before (-10)	-0.6%	1.3%	**-3.5%**
	(1.3%)	(0.7%)	(0.3%)
Before (-9)	-2.3%	**1.8%**	**1.1%**
	(1.4%)	(0.7%)	(0.3%)
Before (-8)	**-3.6%**	0.8%	**2.2%**
	(1.4%)	(0.7%)	(0.3%)
Before (-7)	-2.7%	**1.8%**	**8.5%**
	(1.4%)	(0.7%)	(0.3%)
Before (-6)	**-3.6%**	-1.3%	**2.8%**
	(1.4%)	(0.7%)	(0.3%)
Before (-5)	-0.6%	1.0%	-0.1%
	(1.3%)	(0.6%)	(0.3%)
Before (-4)	2.0%	0.2%	**1.4%**
	(1.3%)	(0.6%)	(0.3%)
Before (-3)	0.7%	**-1.5%**	-0.2%
	(1.4%)	(0.6%)	(0.3%)
Before (-2)	0.0%	**6.7%**	**1.7%**
	(1.4%)	(0.7%)	(0.3%)
Before (-1)	-0.5%	**3.5%**	**5.1%**
	(1.4%)	(0.7%)	(0.3%)
Zero	1.9%	**1.6%**	**5.2%**
	(1.4%)	(0.7%)	(0.3%)
After (1)	-1.9%	**3.1%**	-0.1%
	(1.5%)	(0.7%)	(0.3%)
After (2)	-1.1%	-0.7%	**2.6%**
	(1.6%)	(0.7%)	(0.3%)
After (3)	1.2%	-.07%	**4.8%**
	(1.5%)	(0.7%)	(0.3%)
After (4)	**-5.4%**	**-6.2%**	**-1.8%**
	(1.5%)	(0.7%)	(0.3%)
After (5)	**-7.6%**	**-4.4%**	0.7%
	(1.6%)	(0.7%)	(0.4%)
After (6)	**-8.2%**	0.5%	**-2.0%**
	(1.9%)	(0.9%)	(0.4%)
After (7)	**-12.1%**	**-3.4%**	**-1.8%**
	(2.2%)	(1.0%)	(0.4%)
After (8)	**-9.3%**	-1.1%	**1.5%**
	(2.3%)	(1.0%)	(0.4%)
Number of observations	61,219	62,446	62,023
LogLikelihood	-93,064	-192,793	-441,104

Note: Non-robust 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: All socioeconomic variables and the arrest rate of violent crime

	Murder	Rape	Robbery
	Single dummy variable model		
Post-passage dummy	**-3.1%**	**-2.7%**	**1.0%**
	(0.8%)	(0.4%)	(0.2%)
LogLikelihood	-93,184	-193,183	-442,310
	Spline model		
Before trend	**0.5%**	**0.3%**	**0.8%**
	(0.1%)	(0.0%)	(0.0%)
After trend	**-0.6%**	**-0.4%**	**0.5%**
	(0.1%)	(0.1%)	(0.0%)
LogLikelihood	-93,169	-193,170.42	-441,531
Difference between trends	-0.0111	-0.0070	-0.0029
χ^2 test statistic	40.35	73.25	54.99
Prob > χ^2	0.0%	0.0%	0.0%
	Hybrid model		
Before trend	**0.6%**	**0.4%**	**0.9%**
	(0.1%)	(0.1%)	(0.0%)
Post-passage dummy	**-3.5%**	**-3.4%**	**-4.2%**
	(1.1%)	(0.5%)	(0.2%)
After trend	-0.2%	-0.0%	**-1.0%**
	(0.2%)	(0.1%)	(0.0%)
LogLikelihood	-93,164	-193,146	-441,364
Difference between trends	-0.0078	-0.0040	-0.0008
χ^2 test statistic	15.10	18.49	2.99
Prob > χ^2	0.01%	0.0%	8.35%
	Number of observations in all 3 models		
	61,247	62,474	62,023

Note: Non-robust 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 Appendix 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 and Nagin	Tables 1 & 2 (National Effects)	1	8	12
Duggan	Table 12	1	15*	14*
Ludwig	Tables 4 and 5	0	19	0
Ayres and 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 Column 2 in Table 12 (p. 1110) 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. (Mark Duggan, "More Guns, More Crime," Journal of Political Economy, October 2001, pp. 1086-1114.)
** 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 percent level. The values in parentheses assume that a t-statistic of 1.6 is not significant at the 10 percent level, while the first values assume that a t-statistic rounded off to 1.6 is significant at that level. (See Ian Ayres and John Donohue, "Nondiscretionary concealed weapons laws: a case study of statistics, standards of proof, and public policy," American Law Economics Review 1999 1: 436-470.)
*** Some of these negative significant coefficients are a result of the authors replicating my 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). (Dan Black and Dan Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" Journal of Legal Studies, January 1998, pp. 209-220 and Jens Ludwig, "Concealed-Gun-Carrying Laws and Violent Crime," International Review of Law and Economics, September 1998, pp. 239-254.)

Table Appendix 1 lists out 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 my results are confirmed, and in only 2 percent of 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 percent level, three coefficients imply a statistically significant increase, twenty-two no significant change, and fifteen a statistically significant decline. Of course as mentioned in the introduction to the second section of this book, examining only simple before-and-after averages can be quite misleading and all these critical estimates report only these estimates.