Right-to-Carry Laws and Violent Crime Revisited:
Clustering, Measurement Error, and State-by-State Break downs

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Abstract

This paper investigates several contested issues over how concealed handguns affect crime. Whether accounting for robust errors with clustering or reducing measurement error in the crime rates, the results consistently show large drops in violent crime rates after right-to-carry laws are adopted. This paper is also the first to test whether clustering is actually the appropriate adjustment to make the t-statistics. By six years after the law, murder rates have fallen by 9 percent, rape by 11 percent, and robbery by 7 percent.

* I would like to thank Gertrud Fremling, Carl Moody, and Florenz Plassmann for their helpful comments.

I. Introduction

Many 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 my original research with David Mustard.[1] While some have argued that other statistical methods or data sets are preferable to what we used, even the strongest critics provide no evidence that right-to-carry laws increase violent crime, accidental gun deaths, or suicides.[2]

City, state, and county level panel data have all been analyzed with fairly consistent results for right-to-carry laws. Each type of data has merits and problems. For example, city data have the fewest imputation problems when jurisdictions don't report their crime rates, as those observations are simply left blank. On the other hand, city level data miss out on large parts of the U.S.. State level data have imputation problems when a police department does not report its crime rates, but such problems are less than for county level data. On the other hand, state data ignore that there is more variation in crime rates and other state characteristics within states than across states.[3] What might impact crime in an urban area might not have much impact in a rural area.[4] And increasing the arrest rate in one particular county by a large amount might have very different impacts on crime rates than a smaller but equal increase across all counties in a state.

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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: Eric Helland and Alexander Tabarrok "Using Placebo Laws to Test "More Guns, Less Crime"", *Advances in Economic Analysis & Policy*: Vol. 4: No. 1, Article 1 (2004); 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). See also Florenz Plassmann and John Whitley, "Confirming 'More Guns, Less Crime,'" 55 Stanford Law Review 1313-1369 (May 2003).

[2] In fact, these critics provided a great deal of supportive evidence. See Appendix Table 1. For critical papers see: Ian Ayres and John Donohue, "Nondiscretionary concealed weapons laws: a case study of statistics, standards of proof, and public policy," American Law and Economics Review 1999 1: 436-470; Dan Black and Dan Nagin, "Do Right-to-Carry Laws Deter Violent Crime?" Journal of Legal Studies, January 1998, pp. 209-220; Jens Ludwig, "Concealed-Gun-Carrying Laws and Violent Crime," International Review of Law and Economics, September 1998, pp. 239-254; John Donohue, "The Impact of State Laws Permitting Citizens to Carry Concealed Handguns," in Jens Ludwig and Philip Cook, Evaluating Gun Policy: Effects on Crime and Violence, Brookings Institution: Washington, DC, 2002; Ian Ayres and John Donohue, "Shooting Down the 'More Guns, Less Crime' Hypothesis," 55 Stanford Law Review 1193-1312 (May 2003); Mark Duggan, "More Guns, More Crime," Journal of Political Economy, October 2001, pp. 1086-1114; and Hashem Dezhbakhsh & Paul Rubin., Lives Saved or Lives Lost?, Am. Econ. Rev. 468-474 (May 1998).

[3] Lott (2000, pp. 30-33).

[4] Lott (2000, chp. 4).

Elsewhere I have argued that the imputation problems with county data were small,[5] but new more complete information on this problem is now available for the FBI's county level Uniform Crime Report data from 1994 to 2000. This paper is the first to analyze this information. If the previous findings with the less precise estimates of measurement error for the pre-1993 data are confirmed, county level data would have to be considered superior to state level data because of aggregation concerns.

The city, county, and state data sets raise another question: how to correctly measure statistical significance. For state level data, there is no debate, but when multiple jurisdictions in a state are affected at the same time there are questions of how the error term is correlated with the law change across counties or cities. Positive spatial autocorrelation in the error terms will cause the standard error to be underestimated. The problem is even greater to the extent that an independent variable is serially correlated, as can occur with policy variables.

For several reasons, crime levels can be correlated across counties (and cities). For example, counties might have similar socio-demographic profiles or police departments and courts with similar levels of efficiency, or counties may be victims of the same gang of criminals. Ideally, regression analyses of crime would explicitly model all possible factors that influence crime levels. Omitting relevant factors from the analysis might lead to a correlation among error terms, which invalidates the ordinary method of estimating standard errors that are necessary to assess statistical significance.

State-level laws affect all counties that are under the state's jurisdiction. There is an infinite number of ways in which such laws can affect the counties and regression analyses necessarily ignore some of them. Such omission might again lead to correlation among the error terms.

The standard approach to accommodate such correlation is to identify the "cluster" of observations whose error terms are correlated, and to use this information to adjust the standard errors of the coefficient estimates (see Moulton, 1989). A simple application of this approach is to assume that the error terms of all counties within a state are correlated ("clustering by state"), and to estimate an adjustment factors for the standard errors of all variables. Because the correlations among the error terms can be either positive or negative, such adjustments may either raise or lower significance levels.

Several papers have already used this approach (Moody (2001), Duggan (2001), and Plassmann and Whitley (2003)). Moody's and Duggan's results rely on data from 1977 to 1992 to examine just the simple before-and-after average effect of right-to-carry laws and found significant declines in crime rates after the law when using clustering.[6] Plassmann

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5 . John R. Lott, Jr. and John Whitley, "Measurement Error in County-Level UCR Data," Journal of Quantitative Criminology, 2003.

6 Duggan's paper (p. 1110, column 2 in Table 12) accidentally only lists murder rates and overall violent crime rates as significantly declining after adoption of the right-to-carry law. In fact, his own results show that the t-statistics using clustering for rape is 2.24 (=.052/.0232) and for aggravated assault is 2.52 (=.0699/.0277). Moody's papers uses a dynamic" model with two lags of the dependent variable as well as other lagged variables. Ayres and Donohue (p. 1395) provide results using clustering, though they do not

and Whitley use more recent data that finds large drops in violent crime, but their results were questioned because of some coding errors in the data. Another promising approach by Helland and Tabarrok (2002) used an empirical error function randomly generated from "placebo" laws. Unlike the clustering approach, the placebo approach can also solve autocorrelation problems, and Helland and Tabarrok find that murder, rape and robbery rate trends fall consistently after right-to-carry laws are adopted. They also show that all violent crime rates decline relative to property crime rates after the right-to-carry laws are adopted.

Clustering prevents researchers from assuming that they have more data than they really do. When the law is changed at the state level, all the counties or cities within a state do not represent independent experiments. Yet, if clustering is done at the state level, the assumption is that the law's impact applies equally across the state. In the instance of right-to-carry laws, this criterion clearly does not hold. Almost all the states adopting right-to-carry laws previously started with so-called "May Issue" laws, where local officials decided who obtained permits. Often that discretion was the only factor that determined whether a permit would be issued. In urban areas, discretion meant that few, if any, permits were given out. In many relatively less urban parts of states, local law enforcement frequently let almost anyone who applied obtain a permit. When "Right-to-carry" or "Shall Issue" laws replaced this local discretion with objective rules, the permit-issuing rate was affected quite differently in these different parts of a state. Indeed, while the total number of permits would rise dramatically with right-to-carry laws, in rural counties the additional fees and training requirements that often accompanied right-to-carry laws actually made it more difficult to obtain a permit.

There exists abundant anecdotal evidence that the law does not apply equally across counties. [7] In addition, past statistical work consistently shows a larger drop in violent crime in the more urban areas where right-to-carry laws created the biggest liberalization in permit.[8]

In addition, the clustering adjustment assumes that the error term is correlated with the law change across counties or cities within a state equally across all states. Appendix 2 indicates that assumption also does not hold.

One important problem has already been addressed extensively elsewhere and will not be dealt with here. In several papers, Florenz Plassmann and his co-authors have argued that analyses of the county-level crime data with the method of least squares yield unreliable

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mention that they are doing this in this one table when they are analyzing one table from Plassmann and Whitley's results and Ayres and Donohue do not use clustering in any of their other estimates.

[7] The recent experience in Minnesota, which adopted its law in 2003, has proven similar to other states. Half of the permits issued in the six months after the law was adopted were issued in Ramsey and Hennepin Counties, the two most urban counties in the state, even though those counties make up only about 30 percent of the state's population. A disproportionate number of new permits were also issued in the suburban counties surrounding Ramsey and Hennepin. The most rural (or "outstate") parts of the state where issuing permits had previously been very liberal were described as "experiencing sticker shock at the new costs of getting armed." Associated Press, "Jump in concealed handgun permits is less than expected," 11-28-2003 01:01:13 PM.

[8] For example, see Lott (2000, pp. 64 and 182).

estimates of the impact of right-to-carry laws (Plassmann and Tideman, 2001 and Plassmann and Whitley, 2003). The number of crimes is an integer that cannot be negative, and many of the 3,141 US counties experience not a single crime in categories like murder, (reported) rape, or robbery during a given year. Econometricians have known for at least twenty years (see Hausman et al, 1984) that least-squares methods yield unreliable estimates because they ignore these characteristics of the data, and that count analyses, for example Poisson or negative binomial models, yield better estimates. Critics of right-to-carry laws have consistently ignored this point.

In addition to yielding unreliable estimates for all coefficients, least squares methods will also bias their estimates on the right-to-carry law coefficient towards zero if right-to-carry laws cause crime rates to fall, because no matter how beneficial a right-to-carry law may be, it cannot lower the crime rate below zero.

Despite the persuasive arguments put forward by Plassmann, many still insist on using least square estimates. Even though I agree with Plassmann that least squares bias the results toward not finding that the laws reduce crime, this paper will primarily follow the preferred approach of my critics and use least square estimates. This paper examines whether clustering using least squares still supports the claim that right-to-carry laws produce statistically significant drops in violent crime.

Another question will be examined here too. Claims have recently been made that measurement errors in the county level data makes this data unusable. As I will explain later, fortunately, more recent Uniform Crime Report data from 1994 and later years allow us to account for police departments that fail to report crime data.

II. County Level Data from 1977 to 2000

A) Advantages and Disadvantages of Different Types of Data

Up until the last few years, most crime analysis has relied on state level data. However, the disaggregated county level data have an important advantage in that both crime and arrest rates vary more widely across counties than within states. In fact, the variation in both crime and arrest rates across states is almost always smaller than the average variation across counties within a state.[9] Thus, it is even more inaccurate to view all counties in the typical state as one homogenous unit than it is to view all states in the United States as one homogenous unit. Many different problems can arise from using state level data. For example, when a state's arrest rate rises, it may make a big difference whether that increase occurs place in the most or least crime-prone counties, or whether it increases a lot in one jurisdiction rather than uniformly across the entire state.

As has been done in my previous work, I will examine the FBI Uniform Crime Rate county level data for the entire United States for the entire period for which it is available, 1977 to 2000. Twenty-three states adopted right-to-carry laws during that time period (in order of adoption: Maine, Florida, Virginia, Georgia, Pennsylvania, West Virginia, Idaho, Mississippi, Oregon, Montana, Alaska, Arizona, Tennessee, Wyoming,

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[9] Lott, *supra* note 8, Chp. 2.

Arkansas, Nevada, North Carolina, Oklahoma, Texas, Utah, Kentucky, Louisiana, and South Carolina).[10] The control variables will remain the same as I have used in my books.

Unfortunately, not every police department reports crime data or only intermittently reports it.[11] This problem primarily occurs in rural, sparsely populated where there are often no crimes to report.[12] Yet, this could theoretically cause a problem for using county and state level data as they both suffer from the missing information. The question is whether this measurement error creates a systematic bias that is correlated with right-to-carry laws.[13] Fortunately, the FBI UCR data from 1994 and onwards contains a measure of how large this reporting problem is in a county, their so-called "coverage index."[14] The measure equals the portion of a year that each police agency in a county reports its crime rate, and is weighted by the share of the county's population covered by each agency and by the portion of the year for which the crime rate data are reported. Given that we will be using the data through 2000, there are now seven years covered by this measure.

B) The Impact of Clustering by State

Table 1 uses a specification that breaks down the changes in crime rates by two-year intervals as previously done by Donohue as well as by Plassmann and Whitley. It attempts to quantify the impact of using clustering by state when estimating the impact of right-to-carry on murder. All the control variables have been used in a series of papers that I have done and are described in detail in my book (1998): regional year fixed effects; arrest rates; death penalty execution rate; 36 different demographic variables by age, sex, and race; per capita income; welfare payments per capita; retirement payments per capita for those over age 65; per capita unemployment insurance payments; state level unemployment rates; state level poverty rates.

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[10] For a discussion of the dates used here see Lott, supra note 8, pp. 43 and 169 as well as Plassmann and Whitley (2003). Unfortunately, Florida does not report its arrest rates after 1996, so the data available for that early adopter is only available for the first nine years of the law. The crime rate data indicates that Florida continued to have large drops in the different violent crime rates after that date.

[11] Maltz, M. D. and Targonski, J., "A Note on the Use of County-Level UCR Data," Journal of Quantitative Criminology Vol. 18 (September 2002): 297-318.

[12] Using data from 1977 to 1992, Lott and Whitley showed that in a sample that weights 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. John R. Lott, Jr. and John Whitley, "Measurement Error in County-Level UCR Data," Journal of Quantitative Criminology, 2003.

[13] Ibid, pp. 189-195. At least what data is available for the years from 1977 to 1992 does not imply a systematic bias.

[14] If a rural police department would not have had any crime to report, the failure to include this information does not affect the final crime numbers reported by the county. Alternatively, it is possible that this missing data could result in too high of a crime rate being reported for that county. The reason is that when enough months are reported for a department the crimes from the months that they are assumed to occur at the same rate as crimes in the months when they are not reported. If the rate during the reported months are higher than the rate in the unreported months, it is possible that the reported crime rates will be higher than the actual rate.

Following these previous papers, the two-year intervals are broken down in the following pattern. Years –7 and –8 examine the murder rates in states that will adopt right-to-carry laws seven to eight years later relative to the states that are not going to be adopting right-to-carry laws. And years –5 and –6 examine the murder rates five to six years prior to the adoption of the law. Year zero signifies the year that the law is adopted and year one indicates the first full year that the law is in effect. While not quite as general as the year-by-year breakdown in the years before and after a right-to-carry law is adopted, which I have frequently done previously, the two year at a time breakdown is the next most general way to examine the impact of the law. One reason for using the approach of these other papers is that there are not enough state level observations with the clustering by state to obtain statistical significance with the individual year-by-year breakdown.

The first regression reported in Table 1, specification A, uses the two-year intervals for all the variables, as done in these previous papers, measuring the impact of the right-to-carry law. The second regression, specification B, breaks down the two-year specification between years zero and one to see how the drops in crime rates in later years relate to the crime rate in year zero. Robust standard errors both with and without clustering by state are reported for comparison.

The estimated drop in murder rates is large, but the level of statistical significance varies on the basis of whether one uses robust standard errors with clustering by state or simple robust standard errors. The second regression implies that between the year when right-to-carry is adopted and the second and third years after the law, the murder rate falls by 5 percent. By four to five years after the law, it declines by 11 percent, and thereafter it remains about 9 percent lower than when the law was adopted. The F-statistics using robust standard errors imply that the drops are consistently statistically significant. In only one case in the second regression is a drop only statistically significant at the 12 percent level: the comparison between the year that the law is adopted and years 2 and 3. In all other cases the decline is statistically significant at least at the 5 percent level.

The F-statistics with robust standard errors that use clustering by state are only statistically significant for both specifications at least at the 10 percent level for years 4 and 5 after the law and at the 12 or 13 percent level for years 6 and 7 after the law. Even though the implied drop in murder rates for 8 or more years after the law is greater than for years 6 and 7, the additional impact on crime is not statistically significant because there are so many fewer states that have the law being in effect for that long. Twenty-three states adopted right-to-carry laws during the sample period examined here and all of them had the law in effect for at least four years. Yet, by six to seven years after the law that total had fallen to seventeen and by eight years the total had fallen to thirteen.

Table 2 reports the year-by-year breakdown for all the violent crime categories as well as for property crime overall. The first five columns report regressions that correspond to the first murder regression shown in Table 1, though they only report F-statistics based on robust standard errors with clustering by state.

In all but one case, the crime rates decline. The one exception is a temporary increase for aggravated assault rates during 6 to 7 years after the adoption of right-to-carry laws, and even in that case it still indicates that there is a very small decline, not an increase. With that one exception, robust standard errors without clustering by state (not reported) continue to imply that these results are consistently statistically significant at least at the 10 percent level. Rape rates fall by 7 percent by the second and third years of the law and stay down by around 10 to 15 percent thereafter. F-statistics using robust standard errors with clustering by state show that the drops are significant at the 8 percent level for the second and third years and at better than the one percent level for years 4 through 7. While the drop continues to remain large for years 8 and later, just as with the murder rate, the effect is no longer statistically significant. Robbery rates fall by 6 percent in the second and third years and by about 7 to 10 percent thereafter, but the initial drops for the first two sets of years are statistically significant at the 15 and 10 percent levels.

The final four columns test the cross-equation restrictions that right-to-carry laws should lower violent crime rates relative to property crime. A couple possibilities exist. The laws could simply cause violent crime rates to decline and have no impact on property crimes. This would be plausible as there is no direct contact between the victim and the criminal in property crimes and thus the presence of a gun has not impact. Alternatively, there is a possible substitution effect between crimes to obtain money: such as robbery and property crimes. In this last case, robbers would stop directly confronting victims who might be able to defend themselves and switch to property crimes to obtain money. Given that crime such as murder, aggravated assault, and even rape can occur in conjunction with robberies, it is possible that those could also decline relative to property crimes.[15] In either case, there would be statistically significant drops in violent crime rates relative to property crimes. Even if criminals substitute out of crime altogether as crime becomes more costly, violent crime rates are expected to fall faster than property crime.

The point estimates imply that some substitution is occurring. In contrast to the consistent drops in violent crime, the estimates show a continual increase in property crime. Four to five years after the law, property crime has risen by a total of 2.5 percent and by eight or more years, the accumulated increase is 11 percent. Using robust errors with clustering by state, the results for murder, rape, and robbery imply that these differences are consistently significant at least at the 15 percent level and usually at least at the 5 percent level. For years 2 though 5 after the law, aggravated assaults fall relative to property crime rates at least at the 5 percent level.

Overall, these results from Tables 1 and 2 indicate that even with the revised standard errors and all the caveats associated with them, murder, rape and robbery rates clearly and consistently decline after right-to-carry laws are adopted. The cross-equation restrictions also show even more strongly that murder, rape and robbery rates are dropping relative to property crime rates.

C) Measurement Errors in Crime Rates Due to Non-reporting by Police Departments

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[15] When multiple crimes are committed, the FBI Uniform Crime Report data reports only the most serious offenses. Thus if a murder or rape was committed in accordance with a robbery, those would be the only crimes shown the data and that is where we would be observing any drop in those crime if it were to occur.

As noted earlier, there are potential problems with county level data in that not all police agencies report their crime data every month. To test the impact that this error may have on the results, Table 3a reruns the regressions reported earlier over the 1994 to 2000 time period in three different ways: 1) using all the available county data, 2) eliminate counties where police agencies that represent at least 1 percent of the county population fail to report monthly crime rates, or 3) eliminate counties where police agencies that represent at least 5 percent of the county population fail to report monthly crime rates. Eliminating counties where at least one percent of a county's population is represented by a police agency that is not reporting crime rates reduces the sample size for murder from 9,660 to 7,648 (a 26 percent drop in sample size), whereas the five percent cut off reduces the sample size to 8,560 (a 13 percent reduction in sample size).

With these slightly reduced samples, the results still show consistent, large declines in all the violent crime rates and an increase in property crimes. However, with the exception of property crime, the changes are only statistically significant when robust standard errors are used without clustering. The results are also fairly consistent across the three specifications. Eliminating county observations where crime data are not sufficiently well reported reduces the implied declines in crime for murder and robbery, but increases the size of the declines for rape and aggravated assaults and increases the upswing in property crime. Even the most restrictive results imply that just two to three years after the adoption of the law murder and robbery rates have fallen by a total of 5.8 percent, rape by 6.8 percent, and aggravated assault by 5.2 percent.

Table 3b re-estimates the results previously reported in Table 3a, but now uses the entire sample period. As can be seen, eliminating counties with at least 1 or 5 percent underreporting tends to increase both the size and the statistical significance of the effects. For example, using the 1 percent cutoff, by eight or more years after the law, murder rates decline by over 13 percent instead of by 9.7 percent. The same example for robbery results in a 17 percent decline instead of the previously reported 10 percent decline. The drops also tend to be more statistically significant even though clustering is used. In all cases the drops in murder, rape, and robbery are statistically significant using robust errors and cluster for years 4, 5, 6, and 7. There are other cases where the drops are also statistically significant but in no case does the estimate imply even a temporary increase in crime rates.

Figures 1A through 1E show that eliminating this "noise" in the data actually strengthens the reduction in violent crime rates after and that the changes are particularly dramatic for murder and robbery. Not surprisingly, the noise in the data merely appears to have made it more difficult to identify the drop in crime produced by right-to-carry laws. Despite the (untested) concern raised by others, there is no evidence that the previously reported drops in violent crime rates were due to measurement error.

D) A State-by-State Breakdown of the Impacts

Numerous papers have examined how the impact of right-to-carry laws has varied across states (e.g., Black and Nagin, 1998; Lott, 1998a and 1998b; Plassmann and Tideman, 2001), but a more recent examination by Ayres and Donohue (2003) claims that most states have seen increases in violent crime rates after the adoption of right-to-carry laws (see also Plassmann and Whitley, 2003). Using data from 1977 to 1997, Ayres and Donohue examine whether the average crime rates during the first five years that a law is in effect are lower than the crime rate when the law is adopted at year zero. They make a few changes in the specification employed in the previous tables: with the inclusion of an individual time trend for each state, changing Maine's right-to-carry law as being enacted in 1981 instead of 1985, using what they call a "hybrid" model that examines before-and-after law time trends along with an intercept shift, and not using clustering.[16]

Several approaches are examined here. The simplest and most general is just to take dummies for each of the five years immediately before-and-after the right-to-carry law is adopted for each individual state. Table 4 presents these results and finds that 65 percent of the states experience declines in murder and rape and 61 percent a decline in robbery, though just over half the states experience drops in aggravated assault. There is a 10 percent probability that this drop in murder and rape rates could randomly occur and a 20 percent probability of it randomly occurring for robbery rates. Murder rates declined on average by 6 percent, rapes by 8 percent, robbery by 10 percent, and aggravated assault by 3 percent.

Table 5 is based on the same specifications used in Table 4 and shows that the different violent crime rates are much lower by the fifth year after the law than they were at year zero. By the fifth year after the law 65 percent of states had experienced lower murder and robbery rates and 61 percent a lower rape rate. Again the changes in aggravated assault rates were equally divided across states. The drops in violent crime by the fifth year were similar to the average drops over the entire first five-year period. Eliminating the individual state time trends but still including the regional year fixed effects and other variables used in earlier regressions slightly strengthens these results and raises the percentage of states that experience a drop in murder rates to 70 percent. It also shows that 65 percent of the states experienced drops in rape and robbery rates.

While the year-by-year dummies for each state produce a much more accurate picture of changing crime rates, I also used the before-and-after trends for five years on either side of the law with the intercept shifts and a dummy for the year the law was adopted. The results are slightly stronger than those already reported in Tables 5 and 6. All violent categories showed that between fourteen and sixteen of the twenty-three states experienced declines in violent crime during the first five years.

Finally, there is the question of the overall impact for just those states reporting statistically significant results. Redoing the results reported in Table 3b (with a UCR coverage indicator of at least .99) and using the set of states exhibiting statistically significant impacts from either Tables 5 or 6 produce even greater declines in murder and rape rates after right-to-carry laws are adopted. The results for robbery and aggravated assault are not greatly changed. The results that correspond to the states identified in Table 4 are reported in the Appendix.

[16] Plassmann and Whitley (2003, pp. 1326-9) provide an extensive discussion of the Ayres and Donohue results.

Overall, it is clear that roughly two-thirds of states experience drops in murder, rape and robbery as a result of passage of right-to-carry. But while on average states experience a decline in aggravated assault, there is no consistent pattern across states. Limiting the estimates to those states that have statistically significant changes in the different violent crime categories produce at least as large drops in violent crime than those indicated in Table 3b.

III) Conclusion

According to the regressions presented in this paper, even with the revised standard errors and all the caveats associated with them, murder, rape and robbery rates clearly and consistently decline after right-to-carry laws are adopted. The results also strongly show that murder, rape, robbery, and aggravated assault rates are dropping relative to property crime rates. The findings hold up even when the regressions break down the year-by-year impact of the law on a state-by-state basis. Finally, reducing the measurement error in how crime rate data were collected strengthens the implied crime reducing benefits of right-to-carry laws. These regressions continue to support previous research that "more guns mean less violent crime."

The results also provide some caution for those who make statistical adjustments without first testing to see whether the assumptions required for those assumptions are actually meet. Despite the assumption made by many, the residuals for county observations within the same state and year are not positively correlated.

Despite the large amount of work that has gone into analyzing right-to-carry laws, the results have shown a remarkable consistency. Whether one uses Poisson or weighted least squares estimates or whether robust standard errors with clustering is employed or whether the "placebo law" technique is used, or whether one accounts for measurement error in reported crime rates, there is still a clear and statistically significant drop in violent crime rates after these laws are adopted.

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Figure 1A: Comparing Murder Rates Before-and-After the Adoption of Right-to-Carry Laws: The Impact of Removing Counties Measured with Error

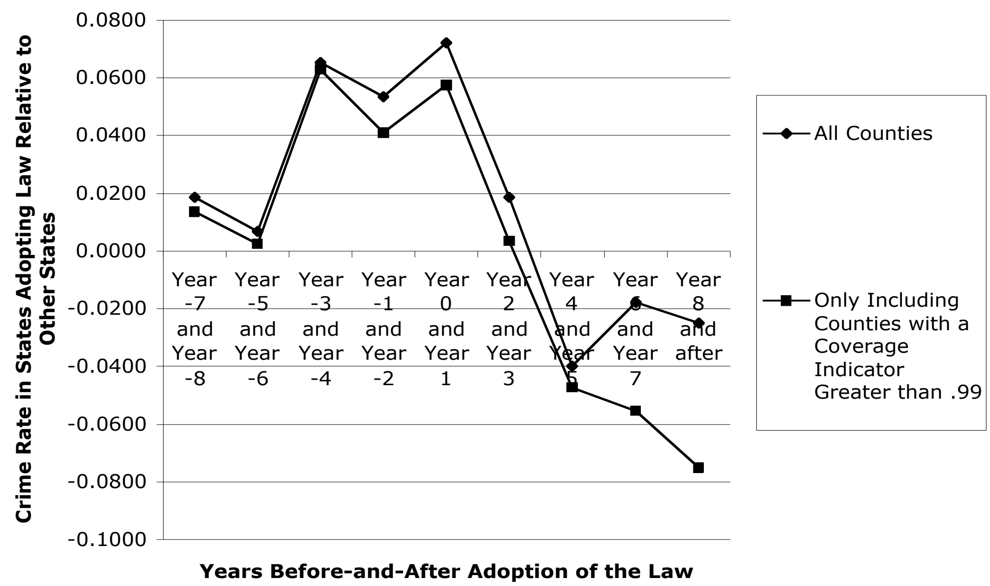


Figure 1B: Comparing Rape Rates Before-and-After the Adoption of Right-to-Carry Laws: The Impact of Removing Counties Measured with Error

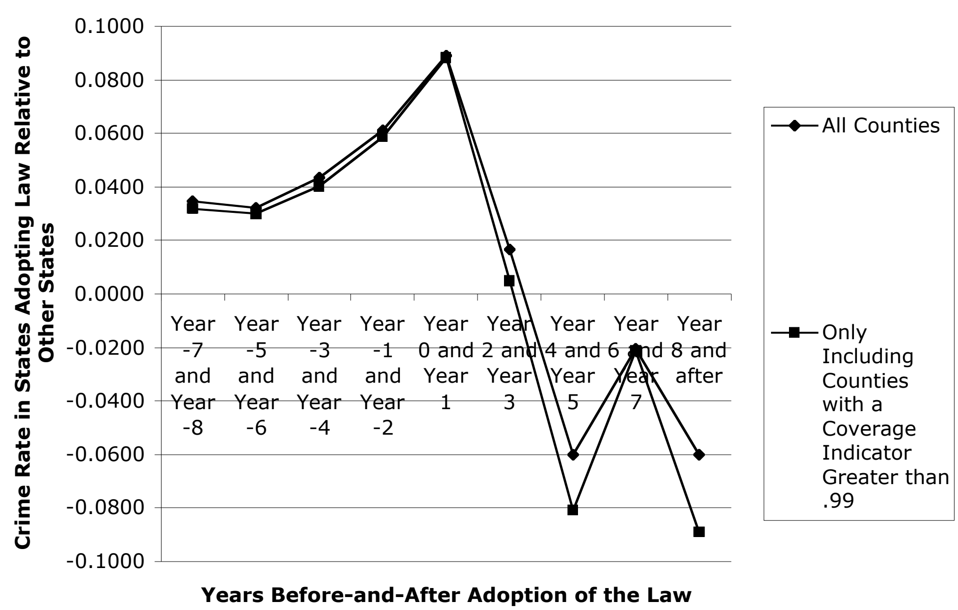


Figure 1C: Comparing Robbery Rates Before-and-After the Adoption of Right-to-Carry Laws: The Impact of Removing Counties Measured with Error

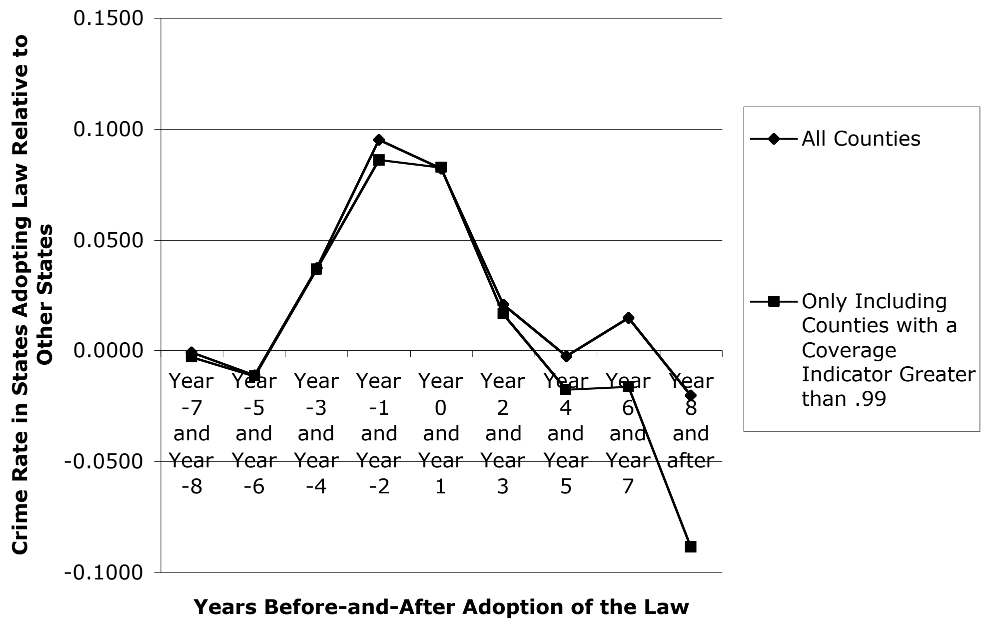
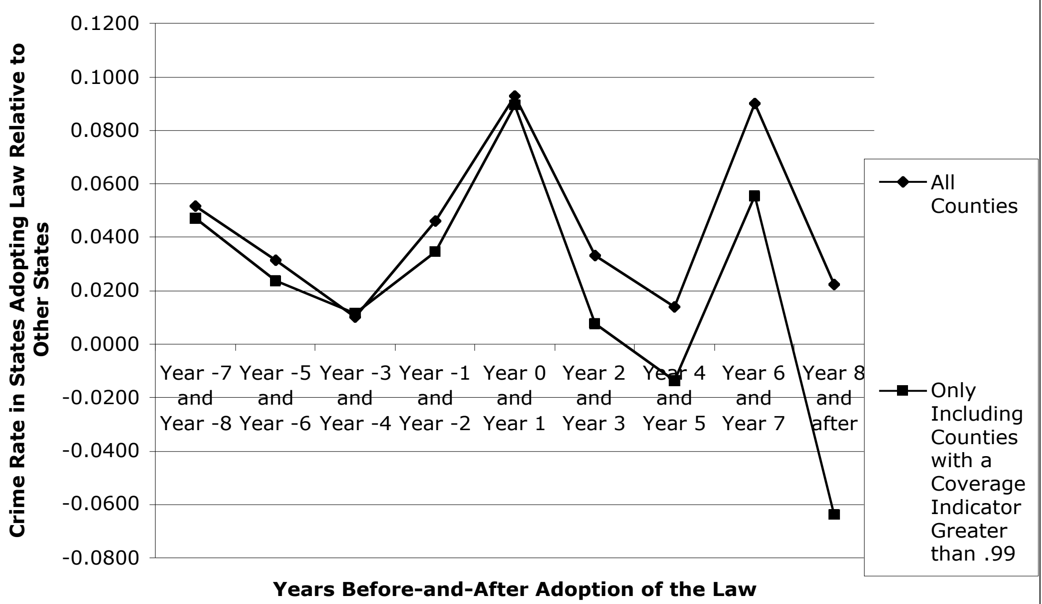


Figure 1D: Comparing Aggravated Assault Rates Before-and-After the Adoption of Right-to-Carry Laws: The Impact of Removing Counties Measured with Error



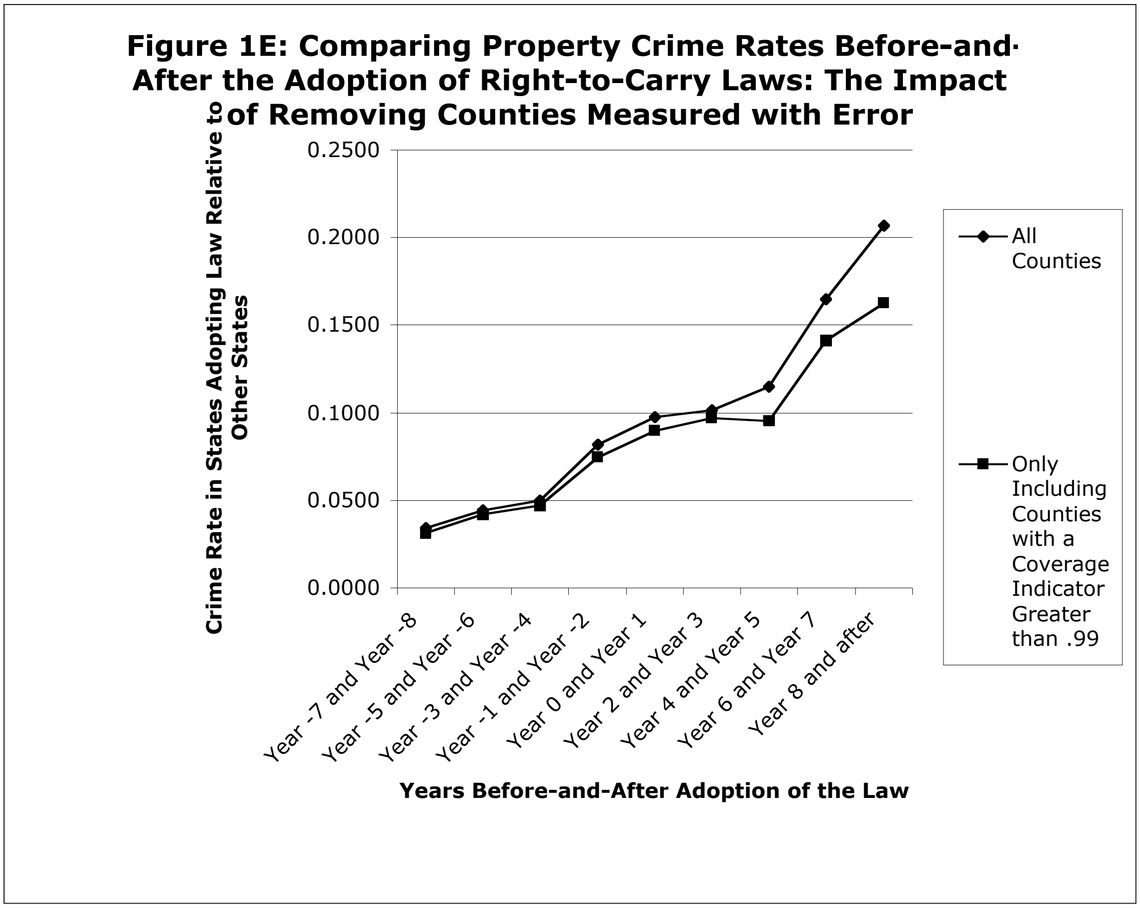


Figure 1E: Comparing Property Crime Rates Before-and-After the Adoption of Right-to-Carry Laws: The Impact of Removing Counties Measured with Error
Crime Rate in States Adopting Law Relative to Other States
0.2500
0.2000
0.1500
0.1000
0.0500
0.0000
Year -7 and Year -8
Year -5 and Year -6
Year -3 and Year -4
Year -1 and Year -2
Year 0 and Year 1
Year 2 and Year 3
Year 4 and Year 5
Year 6 and Year 7
Year 8 and after
Years Before-and-After Adoption of the Law
All Counties
Only Including Counties with a Coverage Indicator Greater than .99

Table 1: Impact of Right-to-carry Law on Murder Rates: Comparing clustering with no clustering using year-by-year data

	Donohue & Plassmann and Whitley Approach A	Probability	Separating out year 0 and year 1 B	Probability
Year -7 and Year -8 Coefficient	0.0187		0.019	
F-test comparing these years to base year(s)				
F-statistic Robust Standard Errors No Clustering	3.62	0.057	2.71	0.0999
F-statistic Robust Standard Errors Clustering	0.59	0.4441	0.56	0.4546
Year -5 and Year -6 Coefficient	0.0068		0.0077	
F-test comparing these years to base year(s)				
F-statistic Robust Standard Errors No Clustering	5.9	0.0151	3.95	0.0468
F-statistic Robust Standard Errors Clustering	1.95	0.1623	1.64	0.2006
Year -3 and Year -4 Coefficient	0.0653		0.0663	
F-test comparing these years to base year(s)				
F-statistic Robust Standard Errors No Clustering	0.06	0.8032	0.03	0.8681
F-statistic Robust Standard Errors Clustering	0.02	0.8927	0.01	0.9196
Year -1 and Year -2 Coefficient	0.0536		0.0544	
F-test comparing these years to base year(s)				
F-statistic Robust Standard Errors No Clustering	0.48	0.4863	0.33	0.5663
F-statistic Robust Standard Errors Clustering	0.22	0.6415	0.16	0.6863
Year Zero (Base Year) Coefficient			0.0716	
Robust t-statistics			2.17	0.03
Robust t-statistics with clustering			0.96	0.336
Year One Coefficient			0.0755	
F-test comparing year 1 to year 0				
F-statistic Robust Standard Errors No Clustering			0.01	0.9119
F-statistic Robust Standard Errors Clustering			0.01	0.9098
Year 0 and Year 1 Coefficient	0.0722			
Robust t-statistics	2.48	0.013		
Robust t-statistics with clustering	0.97	0.334		
Year 2 and Year 3 Coefficient	0.0187		0.0214	
F-test comparing these years to base year(s)				
F-statistic Robust Standard Errors No Clustering	4.05	0.0422	2.45	0.1177
F-statistic Robust Standard Errors Clustering	1.18	0.2782	0.96	0.3278
Year 4 and Year 5 Coefficient	-0.0399		-0.0379	
F-test comparing these years to base year(s)				
F-statistic Robust Standard Errors No Clustering	15.56	0.0001	11.26	0.0008
F-statistic Robust Standard Errors Clustering	3.43	0.064	3.18	0.0747
Year 6 and Year 7 Coefficient	-0.0177		-0.0155	
F-test comparing these years to base year(s)				
F-statistic Robust Standard Errors No Clustering	9.67	0.0019	6.46	0.0111
F-statistic Robust Standard Errors Clustering	2.39	0.1219	2.26	0.133
Year 8 Coefficient	-0.0248		-0.0181	
F-test comparing this year to base year(s)				
F-statistic Robust Standard Errors No Clustering	5.43	0.0198	4.33	0.0376
F-statistic Robust Standard Errors Clustering	0.061	0.4364	0.53	0.466

*The base years in the first set of numbers are Year 0 and Year 1, and for the second set the base year is Year 0 (the year the right to carry law was enacted).

TABLE 2: Impact of Right-to-carry laws on crime rates using clustering

{The relative changes in crime rate (murder, rape, robbery, and aggravated assault) and property crime rate between the passage of the law and the years associated with the row. (F-test in parantheses and probability below)

	Murder	**Rape**	**Robbery**	**Aggravated Assault**	**Property Crime**	**Change in Murder and Property Crime Rates**	**Change in Rape and Property Crime Rates**	**Change in Robbery and Property Crime Rates**	**Change in Aggra-vated Assault and Property Crime Rates**
Year -7 and Year -8	0.0187	0.0345	-0.0007	0.0517	0.0343				
t-statistic	0.90	1.06	-0.01	1.59	1.05				
F-statistic (-7 & -8 to 0 and 1)	0.59	1.21	0.80	0.36	1.32				
Probability	0.4441	0.2709	0.3711	0.5492	0.2513				
Year -5 and Year -6	0.0068	0.0322	-0.0111	0.0316	0.0444				
t-statistic	0.16	0.73	-0.19	0.93	1.86				
F-statistic (-5 & -6 to 0 and 1)	1.95	1.72	1.71	1.00	2.19				
Probability	0.1623	0.1897	0.1905	0.3183	0.1387				
Year -3 and Year -4	0.0653	0.0434	0.0371	0.0102	0.0500				
t-statistic	1.53	1.01	0.56	0.21	1.71				
F-statistic (-3 & -4 to 0 and 1)	0.02	1.67	0.53	3.34	3.38				
Probability	0.8927	0.1968	0.4656	0.0677	0.0659				
Year -1 and Year -2	0.0536	0.0612	0.0953	0.0460	0.0818				
t-statistic	1.05	1.69	1.23	0.88	2.38				
F-statistic (-1 & -2 to 0 and 1)	0.22	0.76	0.16	1.38	0.69				
Probability	0.6415	0.3828	0.6858	0.2400	0.4072				
Year 0 and Year 1	0.0722	0.0890	0.0821	0.0927	0.0978				
t-statistic	0.97	1.69	0.87	1.23	2.43				
Year 2 and Year 3	0.0187	0.0167	0.0210	0.0332	0.1016	-0.0573	-0.0761	-0.0649	-0.0633
t-statistic	0.33	0.37	0.27	0.41	2.43				
F-statistic (2 & 3 to 0 and 1)	1.18	3.08	2.03	2.42	0.04	{1.22}	{3.30}	{2.64}	{3.01}
Probability	0.2782	0.0793	0.154	0.1198	0.8333	0.2684	0.0690	0.1003	0.0803
Year 4 and Year 5	-0.0399	-0.0600	-0.0024	0.0141	0.1152	-0.1294	-0.1664	-0.1019	-0.0960
t-statistic	-0.59	-1.27	-0.03	0.16	3.00				
F-statistic (4 & 5 to 0 and 1)	3.43	7.39	2.63	2.19	0.59	{4.13}	{6.72}	{4.05}	{3.40}
Probability	0.064	0.0065	0.1045	0.1386	0.4437	0.0420	0.0095	0.0422	0.0600
Year 6 and Year 7	-0.0177	-0.0204	0.0150	0.0902	0.1649	-0.1570	-0.1765	-0.1343	-0.0696
t-statistic	-0.25	.-.39	0.20	0.92	3.53				
F-statistic (6 & 7 to 0 and 1)	2.39	7.43	1.70	0.00	2.95	{5.14}	{10.48}	{3.74}	{1.31}
Probability	0.1219	0.0064	0.1928	0.9617	0.0859	0.0233	0.0012	0.0531	0.2517
Year 8 and after	-0.0248	-0.0600	-0.0203	0.0223	0.2069	-0.2060	-0.2581	-0.2115	-0.1795
t-statistic	-0.27	-0.61	-0.26	0.16	3.38				
F-statistic (8 and after to 0 and 1)	0.61	1.66	0.62	0.30	3.03	{2.24}	{3.61}	{2.30}	{1.68}
Probability	0.4364	0.1977	0.4297	0.5838	0.0816	0.1343	0.0574	0.1240	0.1948

* t-statistics and F-statistics are robust with clustering

TABLE 3a: Limited 1994 to 2000 Sample: The Impact of Right-to-carry laws on crime rates using clustering and eliminating jurisdictions where UCR coverage indicator (available after 1993) equals at least .95 or .99

	Coverage Indicator>.99					Coverage Indicator>.95				
	Murder	**Rape**	**Robbery**	**Aggravated Assault**	**Property Crime**	**Murder**	**Rape**	**Robbery**	**Aggravated Assault**	**Property Crime**
Year -1 and Year -2	-0.1110	-0.0675	-0.1210	-0.2057	-0.1335	-0.0850	-0.0732	-0.1420	-0.1739	-0.1557
F-statistic (-1 & -2 to 0 and 1) no Clustering	1.45	6.63	2.25	10.26	1.14	1.63	8.03	3.14	7.76	0.09
F-statistic (-1 & -2 to 0 and 1) Clustering	0.32	0.82	0.27	1.25	0.40	0.33	0.92	0.35	0.93	0.04
Prob without Clustering/ Prob with Clustering	.57/.23	.37/.01	.60/.13	.26/.001	.53/.21	.56/.20	.34/.005	.56/.08	.34/.005	.85/.76
Year 0 and Year 1	-0.0393	0.0397	-0.0597	-0.0624	-0.1521	-0.0145	0.0355	-0.0750	-0.06	-0.1610
t-statistic - robust errors with clustering	0.58	0.60	0.90	0.65	2.45	0.20	0.58	1.14	0.67	2.76
Year 2 and Year 3	-0.0975	-0.0278	-0.1175	-0.1147	-0.1521	-0.0708	-0.0188	-0.1280	-0.0935	-0.1648
F-statistic (2 & 3 to 0 and 1) no Clustering	2.49	9.12	8.09	6.11	0.00	2.66	6.48	7.12	3.24	0.10
F-statistic (2 & 3 to 0 and 1) Clustering	0.82	2.21	1.85	1.47	0.00	0.84	1.54	1.65	0.75	0.05
Prob without Clustering/ Prob with Clustering	.37/.11	.14/.003	.17/.005	.23/.01	.99/.99	.36/.10	0.21/.01	.20/.008	.39/.07	.83/.76
Year 4 and Year 5	-0.1031	-0.0659	-0.0790	-0.1483	-0.0835	-0.0841	-0.0674	-0.0926	-0.1291	-0.0980
F-statistic (4 & 5 to 0 and 1) No Clustering	1.66	15.20	0.57	9.08	12.16	2.23	16.07	0.50	8.00	10.66
F-statistic (4 & 5 to 0 and 1) Clustering	0.64	3.77	0.10	1.20	3.97	0.75	3.56	0.08	0.86	3.41
Prob without Clustering/ Prob with Clustering	.43/.20	.14/.0001	.75/.45	.27/.002	0.05/.001	.38/.14	.06/.0001	0.77/.48	.35/.005	.07/.001
N =	7648	11409	10458	13619	14087	8560	12685	11671	14999	15476

	All Observations				
	Murder	**Rape**	**Robbery**	**Aggravated Assault**	**Property Crime**
Year -1 and Year -2	-0.0805	-0.0553	-0.1112	-0.2030	-0.1341
F-statistic (-1 & -2 to 0 and 1) no Clustering	2.27	7.77	1.46	5.56	0.15
F-statistic (-1 & -2 to 0 and 1) Clustering	0.45	0.88	0.15	0.65	0
Prob without Clustering/ Prob with Clustering	.50/.13	.35/.005	.70/.23	.42/.02	.82/.70
Year 0 and Year 1	-0.0035	0.0438	-0.0690	-0.1116	-0.14
t-statistic	0.04	1.79	1.12	1.25	2.2200
Year 2 and Year 3	-0.0707	-0.0041	-0.1127	-0.1266	-0.1565
F-statistic (2 & 3 to 0 and 1) no Clustering	4.12	5.60	5.40	0.52	1.40
F-statistic (2 & 3 to 0 and 1) Clustering	1.25	1.27	1.09	0.13	0.58
Prob without Clustering/ Prob with Clustering	.26/.04	.26/.02	0.30/.02	.72/.47	.45/.24
Year 4 and Year 5	-0.1061	-0.0508	-0.0880	-0.1493	-0.0910
F-statistic (4 & 5 to 0 and 1) No Clustering	5.43	15.14	0.66	1.67	6.84
F-statistic (4 & 5 to 0 and 1) Clustering	1.67	3.22	0.10	0.21	1.53
Prob without Clustering/ Prob with Clustering	.20/.02	.07/.0001	.75/.42	.65/.20	.22/.009
N =	9660	14527	13501	17346	17960

* t-statistics and F-statistics are robust with clustering

TABLE 3b: Full 1977 to 2000 Sample: The Impact of Right-to-carry laws on crime rates using clustering and eliminating jurisdictions where UCR coverage indicator (available after 1993) equals at least .95 or .99

	Coverage Indicator>.99					Coverage Indicator>.95				
	Murder	**Rape**	**Robbery**	**Aggravated Assault**	**Property Crime**	**Murder**	**Rape**	**Robbery**	**Aggravated Assault**	**Property Crime**
Year -7 and Year -8	0.0136	0.0320	-0.0027	0.0470	0.0313	0.0167	0.0331	-0.0010	0.0519	0.0333
F-statistic (-7 & -8 to 0 and 1)	0.91	1.33	0.91	0.40	1.17	0.45	1.30	0.91	0.44	1.26
Probability	0.34	0.2493	0.3400	0.5295	0.2797	0.5047	0.2540	0.3411	0.5083	0.2613
Year -5 and Year -6	0.0026	0.0300	-0.0114	0.0237	0.0419	0.0051	0.0308	-0.0106	0.0283	0.0440
F-statistic (-5 & -6 to 0 and 1)	1.90	1.86	1.90	1.20	1.92	1.60	1.88	1.90	1.29	2.10
Probability	0.1683	0.1727	0.1683	0.2738	0.1654	0.2065	0.1706	0.1684	0.2569	0.1470
Year -3 and Year -4	0.0629	0.0403	0.0367	0.0117	0.0471	0.0647	0.0410	0.0373	0.0063	0.0509
F-statistic (-3 & -4 to 0 and 1)	0.01	1.79	0.60	4.28	2.91	0.00	1.77	0.64	4.35	2.90
Probability	0.9084	0.1805	0.4388	0.0386	0.0880	0.9809	0.1831	0.4241	0.0370	0.0887
Year -1 and Year -2	0.0410	0.0588	0.0861	0.0346	0.0747	0.0470	0.0611	0.0915	0.0424	0.0806
F-statistic (-1 & -2 to 0 and 1)	0.17	0.91	0.01	2.22	0.60	0.16	0.75	0.02	2.03	0.44
Probability	0.6766	0.3392	0.9178	0.1363	0.4401	0.6888	0.3852	0.8842	0.1539	0.5057
Year 0 and Year 1	0.0574	0.0883	0.0829	0.0894	0.0898	0.0635	0.0897	0.0867	0.0969	0.0943
t-statistic	0.80	1.79	0.90	1.23	2.39	0.84	1.74	0.92	1.31	2.45
Year 2 and Year 3	0.0035	0.0048	0.0167	0.0076	0.0971	0.0119	0.0098	0.0202	0.0194	0.1041
F-statistic (2 & 3 to 0 and 1)	1.12	3.60	2.20	4.81	0.19	1.00	3.25	2.18	4.09	0.30
Probability	0.2895	0.0579	0.1383	0.0282	0.6648	0.3183	0.0715	0.1395	0.0432	0.5851
Year 4 and Year 5	-0.0473	-0.0809	-0.0175	-0.0136	0.0954	-0.0418	-0.0761	-0.0086	-0.0024	0.1113
F-statistic (4 & 5 to 0 and 1)	2.69	8.69	3.64	3.82	0.06	2.95	8.85	3.33	3.70	0.59
Probability	0.1004	0.0032	0.0565	0.0506	0.8031	0.0858	0.0029	0.0680	0.0546	0.4412
Year 6 and Year 7	-0.0554	-0.0211	-0.0162	0.0555	0.1410	-0.0375	-0.0200	0.0012	0.0770	0.1627
F-statistic (6 & 7 to 0 and 1)	4.13	5.58	3.61	0.29	1.40	3.36	6.85	2.69	0.11	2.57
Probability	0.0422	0.0182	0.0573	0.5928	0.2362	0.0670	0.0089	0.1009	0.7307	0.1090
Year 8 and after	-0.0746	-0.0891	-0.0886	-0.0638	0.1626	-0.0423	-0.0682	-0.0507	-0.0170	0.1877
F-statistic (8 and after to 0 anc	0.96	2.21	1.73	1.31	1.54	0.65	1.87	1.09	0.73	2.35
Probability	0.3264	0.137	0.1887	0.2529	0.2143	0.4207	0.1711	0.2965	0.3943	0.1253
N =	35411	46983	47058	59256	61860	35980	47791	47821	60140	63249

* t-statistics and F-statistics are robust with clustering

Table 4: Examining Whether the Average Crime Rate in the First Five Years After the Law is Greater or Less than Year Zero By State: Using the Entire Sample Period (1977 to 2000) and Eliminating Observations when the Coverage Indicator is less than .99 (Regional Year Fixed Effects and Individual State Time Trends)

	Change in Crime Rate Between Year Zero and the Average Over first 5 years of Law			
State	Murder	Rape	Robbery	Aggravated Assault
Maine	-0.2252544	0.01414372	0.00343804	-0.2618578
Florida	-0.23899123	-0.176969867	-0.214744833	-0.033943567
Virginia	-0.01386316	-0.0622133	-0.14776438	-0.03313966
Georgia	-0.12212968	-0.38740084	-0.39390982	-0.27277058
Pennsylvania	-0.0806563	-0.02475948	0.0491598	-0.07792464
West Virginia	-0.17197078	-0.07249504	-0.22159584	0.11080702
Idaho	-0.2159028	-0.03107074	0.1494223	0.10316914
Mississippi	0.04556516	-0.14628192	0.04504082	0.03501668
Oregon	0.03119602	-0.0085837	-0.09786562	0.0991009
Montana	-1.19216513	-0.49229445	-0.748668425	-0.0370885
Alaska	0.35117818	0.29685464	-0.2182447	0.29524754
Arizona	-0.2769903	-0.07004012	0.09530848	0.09805244
Tennessee	0.45408202	0.26042816	0.20420282	0.27438092
Wyoming	0.10497326	-0.04459892	0.11760492	0.21341884
Arkansas	-0.02594554	0.06244924	-0.03716538	0.2647085
Nevada	0.20629634	-0.25852228	-0.05582554	-0.33163572
North Carolina	0.2067976	0.0652809	0.03605814	0.13525524
Oklahoma	-0.16499914	0.22477172	-0.12665168	-0.05571932
Texas	-0.08267228	0.0450106	-0.0770675	-0.02341062
Utah	0.60798624	0.10054304	0.1271263	0.07219098
Kentucky	-0.247375	-0.407142325	-0.416079625	-0.786949125
Louisiana	-0.1447447	-0.280533325	-0.26060845	-0.3148867
South Carolina	-0.19115083	-0.397869475	-0.163102425	-0.2584079
Average Change	-0.06029289	-0.077882337	-0.102257939	-0.034190693
Number of Cases Implying Crime Dropped	15	15	14	12
Number of Cases statistically signficant	9	8	10	5
Number of Cases Implying Crime Rose	8	8	9	11
Number of Cases statistically signficant	5	3	1	7
Probability that the states showing a drop could have occurred	0.1	0.1	0.2	0.5

t-tests used robust standard errors with clustering

Table 5: Breaking Down the Change in Crime Rates Between Year Zero and Year Five By State Using the Entire Sample Period (1977 to 2000) and Eliminating Observations when the Coverage Indicator is less than .99 (Regional Year Fixed Effects and Individual State Time Trends)

	Change in Crime Rates Between Year Zero and Year Five			
State	Murder	Rape	Robbery	Aggravated Assault
Maine	-0.1156	-0.064	-0.0524	-0.4613
Florida	-0.2332189	-0.1312509	-0.3190278	0.0205661
Virginia	-0.1544388	-0.1432341	-0.293721	-0.2012215
Georgia	-0.2871245	-0.5914208	-0.5996459	-0.4072742
Pennsylvania	-0.1441408	-0.0763956	0.0699142	-0.0808034
West Virginia	-0.3914592	-0.2415964	-0.2616248	0.2242639
Idaho	-0.282502	-0.2723614	0.0735552	0.1639913
Mississippi	0.1116894	-0.0188978	0.1074413	0.3837249
Oregon	-0.06006	-0.1104008	-0.0162781	0.2754598
Montana	0.2140614	0.1620811	-0.3374699	0.1871518
Alaska	0.3922854	0.6039973	-0.1644156	0.4502257
Arizona	-0.3546207	0.0442542	0.2246911	-0.0827337
Tennessee	0.3222249	0.2704827	0.3409574	0.4020627
Wyoming	-0.0073804	0.0178966	0.2769925	0.2806475
Arkansas	-0.2325584	-0.0190525	-0.1521195	0.185621
Nevada	0.2410913	-0.2705908	-0.0262007	-0.5642194
North Carolina	0.2490273	0.1277405	0.1196497	-0.1534026
Oklahoma	-0.4101643	0.2170249	-0.28181	-0.253811
Texas	0.0524134	0.0129644	0.3185401	-0.1113504
Utah	0.6688315	0.2583022	0.2031454	-0.1747341
Kentucky	-0.5592842	-0.6153592	-0.6021461	-1.5155845
Louisiana	-0.1798027	-0.2519603	-0.3255682	-0.3924724
South Carolina	-0.1836553	-0.5215703	-0.1217877	-0.3461313
Average Change	-0.058451548	-0.0701455	-0.07910123	-0.094405383
Number of Cases Implying Crime Dropped	15	14	15	13
Number of Cases statistically signficant	11	8	9	7
Number of Cases Implying Crime Rose	8	9	8	10
Number of Cases statistically signficant	6	6	5	7
Probability that the states showing a drop could have occurred	0.1	0.2	0.1	0.34

t-tests used robust standard errors with clustering

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)
Note: None of these papers use robust errors with clustering.

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)**
Donohue – all non-Hybrid estimates	Tables 8.1 to 8.6	2	19	35
Hybrid estimates for **just** the first year of Law***	Tables 8.1 to 8.4 (Hybrid estimates no relevant to last two tables)	4	26	2
Ayres and Donohue – all non-Hybrid estimates	Tables 10 to 11 County estimates	2	8	14
Hybrid estimates for **just** the first year of Law***	Tables 10 to 11 County estimates	2	14	0
Totals		10	122 (125)	107 (104)

* 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 stud of statistics, standards of proof, and public policy," American Law and 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.)
****Ayres and Donohue do not provide any of their own estimates of whether the net effect of the dummy and trend variables that they use are statistically significant. Since most of the trend variables are negative and the intercept terms are often positive, the longer the law is in effect the greater the implied drop in violent crime. To bias the results against myself, I only report whether the net effect for year one is statistically significant at the 10 percent level.

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 239 coefficients reported by these critics, only 10 coefficients imply a statistically significant increase in crime after the passage of the law (and 6 of those are limited to so-called "hybrid" specifications that imply significant drops in violent crime after a few years), 122 imply no statistically significant change, and 107 a statistically significant decline in crime. In other words, half the time my results are confirmed, and in only 4 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. For a discussion of the problems with Ayres and Donohue's state-by-state estimates where they fit their hybrid models to nonlinear data and then limit their analysis to only the beginning of the time period see Plassmann and Whitley. The year-by-year estimates that we show here on a state-by-state basis clearly show that there is no initial increase in crime for the large majority of states.

Appendix 2: Does Clustering by State Meet the Conditions for Using It at the State Level with County Level Crime Data

As noted earlier, positive spatial autocorrelation in the error terms will cause the standard error to be underestimated. While this is a potential problem, researchers who have used this approach thus far in analyzing right-to-carry laws have simply assumed that the positive correlation exists. None of the research that has adjusted for clustering has actually tested to see if this adjustment is appropriate.

To test for the existence of this correlated error terms, the residuals for each of the regressions reported in Table 2 were broken down by state by year. The residuals for each of these state-year combinations were examined to see whether their means were within two standard deviation of zero. If the mean is significantly greater or less than zero, the implication is that the errors are positively correlated. If the mean is not statistically different from zero, it is possible that the error terms could be either uncorrelated or negatively correlated.[17]

For the murder regressions, in only 1.2 percent of the by state by year breakdowns were the average residual statistically significantly different from zero. For rapes, it was 15 percent; for robbery, 27 percent; and for aggravated assault, 13 percent. Table 3 looks at how the error terms were correlated by state for the years that the right-to-carry laws were in effect and again there is little evidence that these errors are positively correlated. (This breakdown is somewhat less desirable, but it is in a much more manageable form to report in a table.) For the twenty-three states that had these laws in effect, none of the states had residuals that were statistically different from zero for murder, rape or aggravated assault. In the case of murder, four states (Idaho, Maine, West Virginia, and Wyoming) exhibited residuals that were statistically significantly different from zero.[18]

There remains the question of whether the errors are correlated over some other geographic area. If this correlation in error terms exists, it could be among urban counties, neighboring counties, neighboring counties across state boarders in different states? There are essentially an almost infinite number of combinations. It is possible that one of these other combinations of geographic areas has correlated error terms, but what is clear is that this homogeneity assumption does not hold for this crime data.

Appendix Table 2 indicates that clustering by state increases the standard error of the crime coefficient. Because there is no statistically significant positive correlation among the error terms within each state without clustering, clustering by state is inappropriate

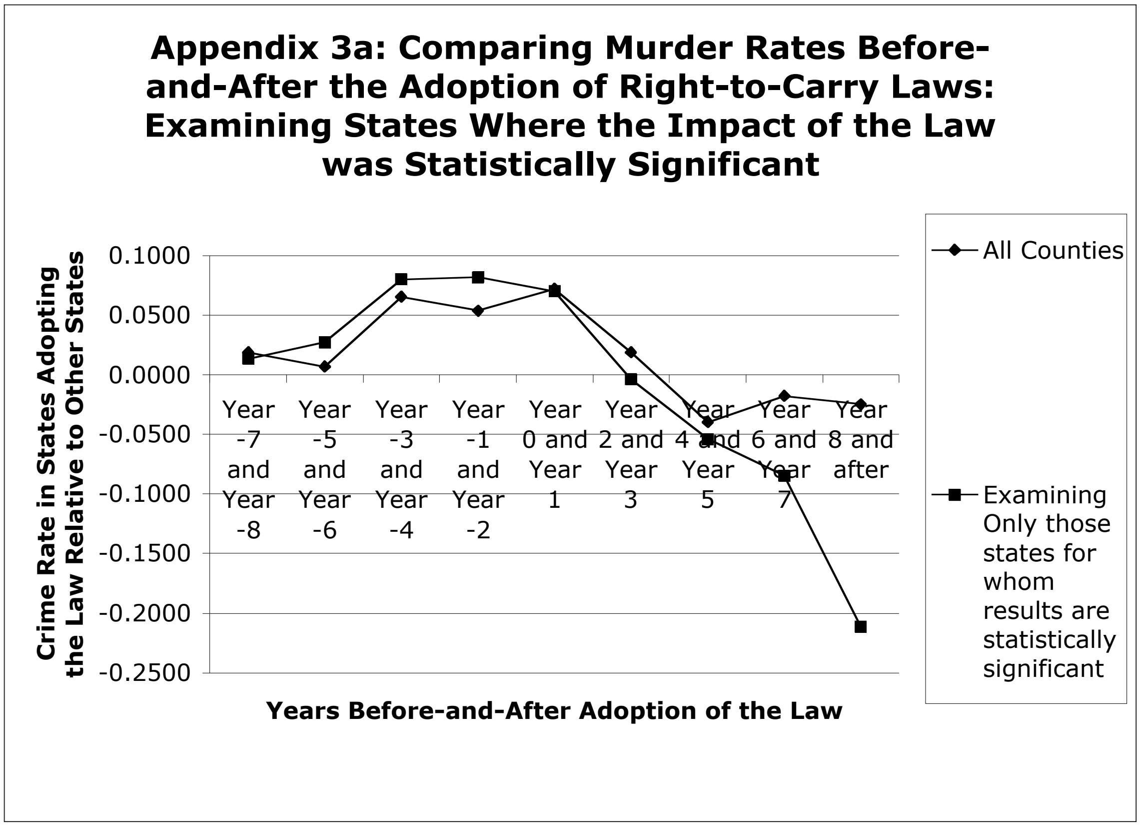
[17] Errors are positively correlated if a positive (negative) error implies that the error of another observation is also more likely to be positive (negative). Errors are negatively correlated if a positive (negative) error implies that the error of another observation is more likely to be negative (positive). Errors are uncorrelated if the observation that one error is either positive or negative does not provide any information about the sign of another error.

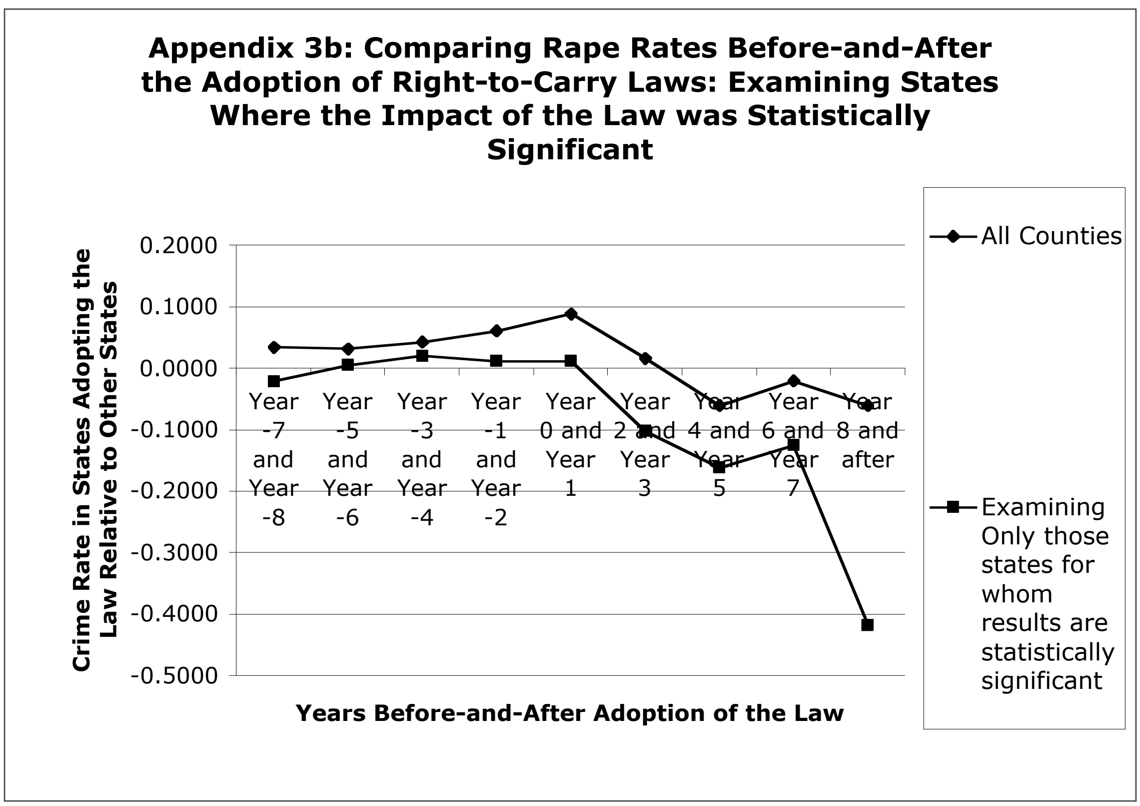
[18] There is one other caution that should be raised about using clustering. The method employed by canned statistical packages such as STATA assumes that the adjustment is the same across all states. Even in the case of robbery where about a quarter of the state year observations exhibit some problem, it is clear that the adjustment would be inappropriate for many state-year combinations.

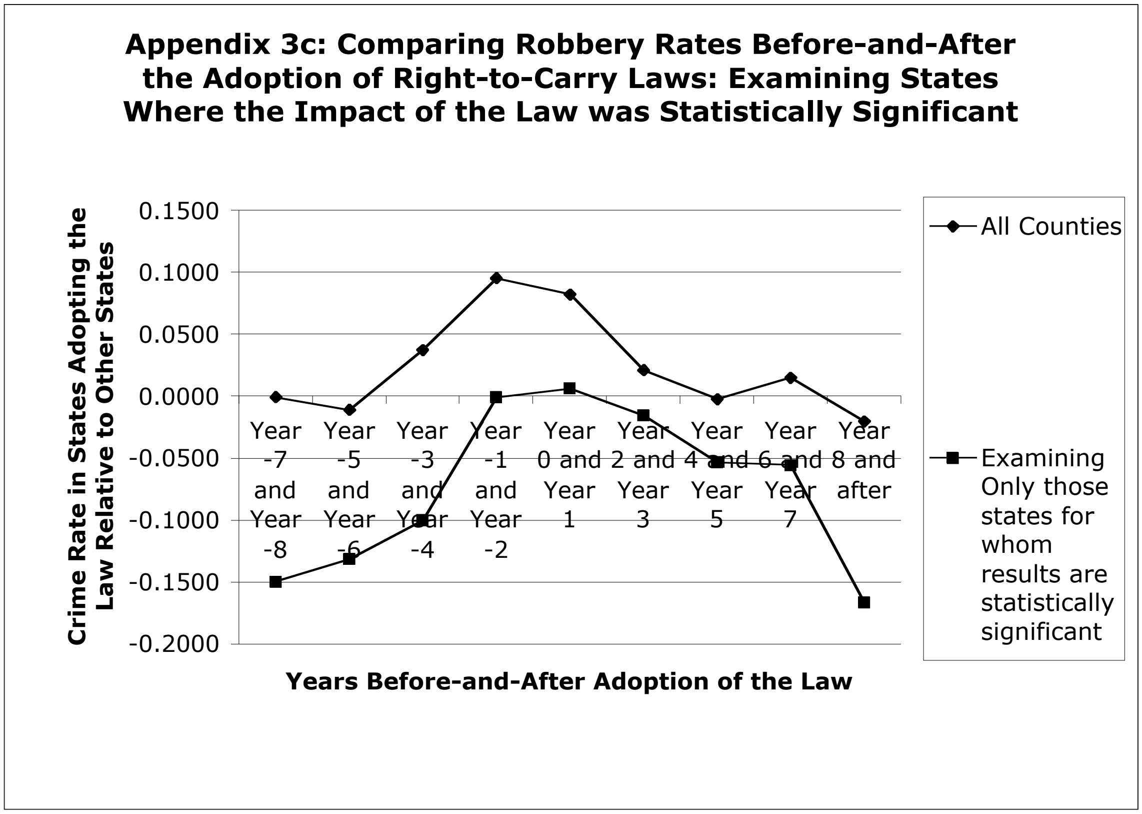
and biases the results against finding statistically significant changes in crime rates. For the sake of consistency, I continued to use clustering in the regression analyses whose results I report below.

Table Appendix 2: Does Clustering at the State Level Make Sense?: Plus or Minus 2 Standard Deviation intervals for the residuals by state when right-to-carry laws are in effect (Specifications for each crime rate correspond to those in Table 2)

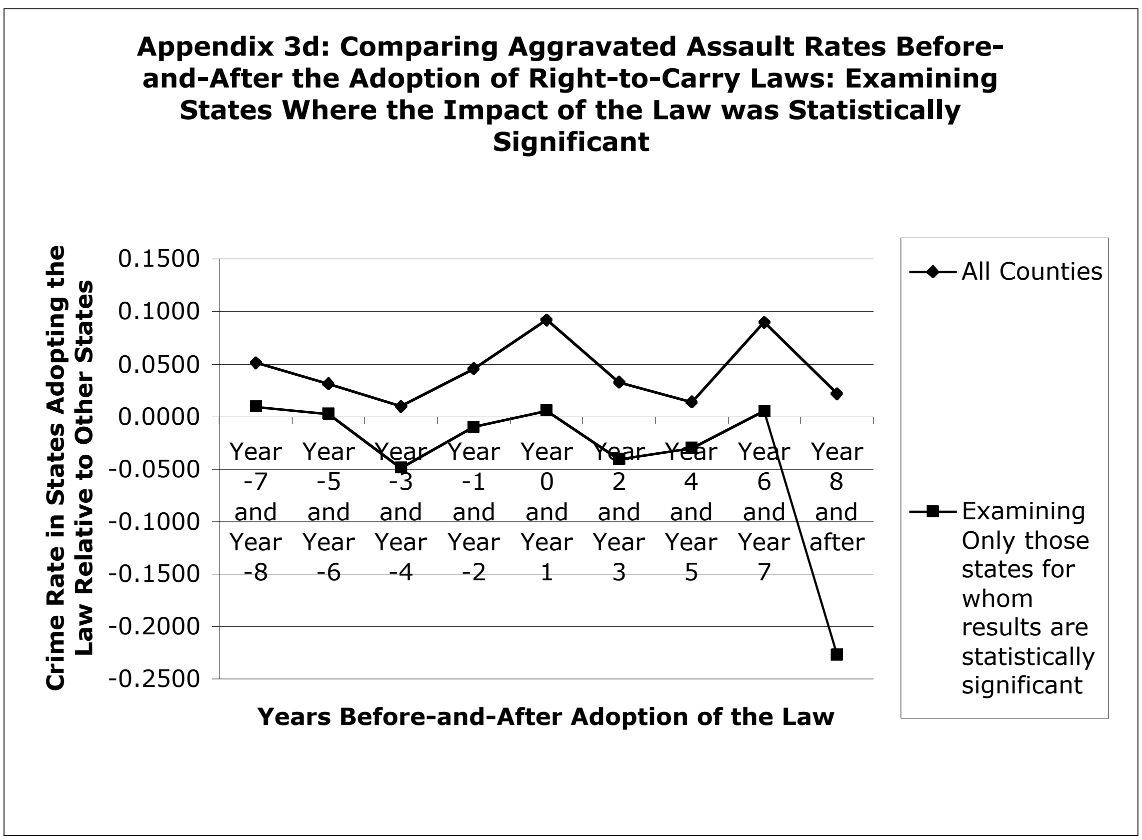
	Murder		Rape		Robbery		Aggravates Assault	
State	95% confidence Interval		95% confidence Interval		95% confidence Interval		95% confidence Interval	
Alaska	-1.3589673	2.6317	-2.986786	2.914286	-2.961784	1.581632	-1.9756001	2.1853079
Arizona	-1.2534893	1.61234	-3.335327	0.860569	-3.566518	1.847398	-0.9903463	1.9657357
Arkansas	-0.8577605	1.90284	-2.420182	1.363376	-3.522395	0.494737	-2.3114016	1.6153096
Florida	-1.1244347	1.56528	-1.670184	1.64846	-2.454288	1.68568	-1.1063469	2.0954899
Georgia	-1.2229965	1.44991	-2.254682	1.41538	-3.237045	0.673059	-2.1175718	1.650553
Idaho	-1.4599972	2.24669	-2.28397	0.258616	-3.236548	-0.33639	-1.6303133	1.2342475
Kentucky	-1.1742026	1.11924	-2.256888	0.437357	-2.701399	0.779998	-2.0872862	0.9678194
Louisiana	-0.9626848	1.39071	-1.897843	1.393102	-3.29525	0.699996	-0.806045	1.6672274
Maine	-1.9875578	0.56823	-2.779212	-0.325602	-3.873126	-0.210392	-2.4720516	-0.2952044
Mississippi	-1.4137531	1.27823	-2.158369	1.733284	-3.546549	0.460335	-2.8461156	1.1266364
Montana	-1.5849623	2.66732	-3.133133	0.253021	-3.389203	0.211021	-2.5248073	1.4799407
Nevada	-0.7191408	1.9296	-2.635018	1.15167	-3.050391	1.238585	-1.0147635	1.8359357
North Carolina	-0.9781789	1.41155	-1.880193	1.00623	-2.713784	0.899959	-1.3926233	1.1836155
Oklahoma	-0.5783352	2.15488	-1.811669	0.77849	-3.144692	0.117164	-1.35829	1.2966168
Oregon	-1.5966707	1.4117	-1.409974	0.818852	-2.069965	1.095536	-2.1631585	0.8872399
Pennsylvania	-2.0028234	0.83313	-2.390918	0.08767	-3.455001	1.132183	-2.0721308	0.4467808
South Carolina	-1.2589303	1.12663	-0.804057	1.545474	-1.590783	0.557478	0.0759353	1.8310133
Tennessee	-1.0307694	1.73981	-2.705844	0.978515	-3.577486	0.43397	-1.8784678	1.2886586
Texas	-0.914571	2.24106	-1.833542	1.286859	-3.074823	0.457559	-1.6793931	1.2457117
Utah	-1.8225216	1.87084	-2.320709	0.558686	-3.586872	0.0286	-1.6692003	1.2811209
Virginia	-1.1324347	1.36618	-2.367142	1.264606	-3.970771	0.445977	-2.5671561	0.8782671
West Virginia	-0.8923383	1.80646	-2.803301	0.372649	-3.922282	-0.220006	-3.0793148	0.5222888
Wyoming	-1.2492616	1.80814	-2.583784	0.201826	-3.378836	-0.504814	-0.8730112	1.000762
Cases where error terms are positively correlated with a state		0		0		4		0
Cases where error terms are either not correlated or negatively correlated		23		23		19		23


Appendix 3a: Comparing Murder Rates Before-and-After the Adoption of Right-to-Carry Laws: Examining States Where the Impact of the Law was Statistically Significant
Crime Rate in States Adopting the Law Relative to Other States
0.1000
0.0500
0.0000
-0.0500
-0.1000
-0.1500
-0.2000
-0.2500
Year -7 and Year -8
Year -5 and Year -6
Year -3 and Year -4
Year -1 and Year -2
Year 0 and Year 1
Year 2 and Year 3
Year 4 and Year 5
Year 6 and Year 7
Year 8 and after
Years Before-and-After Adoption of the Law
All Counties
Examining Only those states for whom results are statistically significant


Appendix 3b: Comparing Rape Rates Before-and-After the Adoption of Right-to-Carry Laws: Examining States Where the Impact of the Law was Statistically Significant
Crime Rate in States Adopting the Law Relative to Other States
0.2000
0.1000
0.0000
-0.1000
-0.2000
-0.3000
-0.4000
-0.5000
Year -7 and Year -8
Year -5 and Year -6
Year -3 and Year -4
Year -1 and Year -2
Year 0 and Year 1
Year 2 and Year 3
Year 4 and Year 5
Year 6 and Year 7
Year 8 and after
Years Before-and-After Adoption of the Law
All Counties
Examining Only those states for whom results are statistically significant



Appendix 3c: Comparing Robbery Rates Before-and-After the Adoption of Right-to-Carry Laws: Examining States Where the Impact of the Law was Statistically Significant
Crime Rate in States Adopting the Law Relative to Other States
0.1500
0.1000
0.0500
0.0000
-0.0500
-0.1000
-0.1500
-0.2000
Year -7 and Year -8
Year -5 and Year -6
Year -3 and Year -4
Year -1 and Year -2
Year 0 and Year 1
Year 2 and Year 3
Year 4 and Year 5
Year 6 and Year 7
Year 8 and after
Years Before-and-After Adoption of the Law
All Counties
Examining Only those states for whom results are statistically significant



Appendix 3d: Comparing Aggravated Assault Rates Before-and-After the Adoption of Right-to-Carry Laws: Examining States Where the Impact of the Law was Statistically Significant
Crime Rate in States Adopting the Law Relative to Other States
0.1500
0.1000
0.0500
0.0000
-0.0500
-0.1000
-0.1500
-0.2000
-0.2500
Year -7 and Year -8
Year -5 and Year -6
Year -3 and Year -4
Year -1 and Year -2
Year 0 and Year 1
Year 2 and Year 3
Year 4 and Year 5
Year 6 and Year 7
Year 8 and after
Years Before-and-After Adoption of the Law
All Counties
Examining Only those states for whom results are statistically significant