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Editor's Note: Professors Ian Ayres and John J. Donohue have been invited to reply to this article. Their analysis will appear in the January 2009 issue of the journal.

The Debate on Shall-Issue Laws

Carlisle E. Moody[1] and Thomas B. Marvell[2]

Abstract

"Shall issue" right-to-carry concealed weapons laws require authorities to issue concealed-weapons permits, allowing the permit holder to carry a concealed handgun, to anyone who applies, unless the applicant has a criminal record or a history of mental illness. The shall-issue laws are state laws, applicable to all counties within the state.[3] In contrast, states with "may issue" laws allow considerable discretion to the authorities. In may-issue states, authorities typically require that the applicant demonstrate a particular need for a concealed weapons permit, and self-defense usually is not deemed sufficient. Consequently, shall-issue states are much more permissive of individual freedom to carry concealed handguns.

In 1997 John Lott and David Mustard published, "Crime, Deterrence and Right-to-Carry Concealed Handguns" in the *Journal of Legal Studies*. They found that shall-issue states had lower violent crime rates, presumably because the laws result in more people carrying concealed weapons. Criminals might be deterred by the greater likelihood of others being armed, and of arms being concealed. Lott and Mustard's article created a furor and the debate continues. Much of this debate takes place in op-ed columns, letters to editors, internet chat rooms, and web logs. In this article we concentrate on the academic debate. We review the main threads of the discussion in the literature and extend the debate with our own statistical analyses. In particular, we extend the investigation of influential work in *Stanford Law Review* by Ian Ayres and John J. Donohue III (2003a, 2003b), who, contrary to Lott and Mustard, claim to find that shall-issue laws actually lead to an overall increase in crime. The new statistical analysis contained in the present article finds that shall issue laws

1 Professor of Economics, College of William and Mary, Williamsburg, Virginia 23187, cemood@wm.edu
2 Director of Justec Research, Williamsburg, Virginia 23185, marvell@cox.net
3 Except for Philadelphia, which was initially exempt from Pennsylvania's shall-issue law.

are generally beneficial. Purists in statistical analysis object with some cause to some of methods employed both by Ayres and Donohue, by us, and by the literature in general. But the new investigation presented here upgrades Ayres and Donohue in a few significant ways, so, at least until the next study comes along, our paper should neutralize Ayres and Donohue's "more guns, more crime" conclusion.

The State of the Debate

In this paper, when we use the term "significant" to describe results of statistical investigation, the term means statistically significant at the 0.10 level.

The original study by Lott and Mustard (1997) used pooled time-series and cross-section data across all the counties in the United States for the years 1977 to 1992. They used the fixed-effects panel data model, which corrects for possible unobserved heterogeneity across counties. They also included time dummies, arrest rates, several income variables and a host of detailed demographic control variables. The target variable was a dummy variable that took the unit value for those counties in shall-issue states during or after the first full year of implementation, zero otherwise.[4] The primary set of results was reported in Lott and Mustard's Table 3 (1997, 20-23). The estimated coefficient on the shall-issue dummy variable was negative and significant for all the violent crimes (murder, rape, robbery, and assault), positive and significant for larceny and auto theft, and not significant for burglary. The estimated coefficients were also large enough numerically to cause substantial reductions in the estimated costs of crime. Lott and Mustard also offered a corresponding state-level model (1997, 27). They found that all violent crime categories were significantly reduced by shall-issue laws, again with large implied reductions in the costs of crime. They then engaged in a series of robustness tests all of which confirmed the basic finding that right-to-carry laws reduced violent crime.

Contrary findings appeared very quickly. Black and Nagin (1998) noted that Lott and Mustard, by using a single dummy variable for the shall-issue law, assumed the same effect for all states and all years. They extended the model to allow for separate dummies for each state and found that the results differed across states with some states significantly positive, some significantly negative, and some showing no effect. They also estimated a first-differenced model using pre- and post-law dummy variables for the five years before and after the adoption of the shall-issue law. Finally, they estimated a model with individual state trends as additional controls (but with a single shall-issue dummy). They concluded that the Lott and Mustard results were fragile and that, overall, the shall-issue law had no significant effect on crime.

4 Lott and Mustard also tried a shall-issue variable that took a fractional value indicating the proportion of the year the law was in effect in its first year, the results were unchanged.

Lott (1998) responded in the same issue of the *Journal of Legal Studies*. He pointed out that Black and Nagin ignored the models in which Lott and Mustard (1997) multiplied the shall-issue law dummy by the state population. Thus their criticism that the Lott and Mustard model relied on a single dummy variable for all states was misplaced. Lott's most telling criticism, however, concerned Black and Nagin's use of pre- and post-law dummy variables. If crime rates were generally increasing prior to the passage of the law and falling after, the series describing an "inverted V," as Lott and Mustard reported (1997, 35), the coefficients on the dummy variables for the two or three years before the law could be expected to be approximately the same as the corresponding coefficients for the two or three years after, implying no effect of the law, when the law in fact had a very significant effect on the crime rate.[5]

Black and Nagin also criticized Lott and Mustard for not including individual state trends as controls for potentially omitted variables. However, as Lott pointed out, the original paper had reported first differenced models, one of which included state dummies. In such a model, the state dummies are equivalent to individual state trends. Lott also argued that the original paper had allowed differential impacts across states in the sense that individual analyses were done for Pennsylvania and Oregon where data on the number of permits were available. Although Lott responded to each of the points raised by Black and Nagin, the issue remained unresolved.

At this point, the broad outline of the subsequent debate was already in place. Future work would have to address the problem of differing before and after trends (including the inverted V), allow for individual state trends, and allow the law to have differing impacts across states.

Two years later, Lott (2000) extended the sample to 1994 and introduced spline models to address the inverted V problem. Lott examined many alternative versions of the model and determined that the results were very robust. Shall-issue laws were found to significantly reduce violent crime.

In 2001 the *Journal of Law and Economics* published the proceedings of a conference on shall-issue laws. In that volume, several studies confirmed the hypothesis that shall-issue laws reduce crime. One year later, 2002, in the second edition of *More Guns Less Crime*, Lott extended the sample to 1996 and re-estimated the spline models, along with a host of alternative specifications. Shall-issue laws were again found to reduce violent crime.

At this point in the debate, the weight of evidence was firmly on the side of those claiming that shall-issue laws reduce violent crime. However, Ayres and Donohue (2003a) significantly shifted the debate. They noted that the aggregate model, which uses a single dummy for all states, was possibly susceptible to selec-

5 Note that this criticism of the dummy variable method only applies to short periods after the passage of the law. If the law remains in force for many years and crime falls continuously, the average effect estimated by the dummies will eventually be negative.

tion bias in the sense that early-adopting states are in the data set for many years and late-adopting states are barely represented. Thus, the aggregate model with a single dummy or trend for all states, when extended over many years, is eventually reflecting only a few states, not the entire country. For example, the aggregate model of the 24 states that have passed shall-issue laws since 1977, when extended out 14 years to 1990, includes only two states, Maine and Florida, with shall-issue the full period, giving them a dominant role in the model. Those two states may not be representative of the country as a whole.

Ayres and Donohue also claimed that the original 1997 Lott and Mustard paper, which was based on 1977-1992 data, included only states that adopted shall-issue laws in the 1980s when crime peaked because of the emergence of crack cocaine. Thus, the post-crack-wave fall in crime was being reflected in the negative coefficients on the shall-issue dummy variables. Ayres and Donohue argue that by extending the county data set to 1997, they are allowing the states that passed the law after the crack wave was over, to help determine the effect of the law and mitigate the crack-wave effect. However, Lott and Mustard estimated, but did not report, a model including the price of cocaine. They found that the results were not affected. Also, the presence of time dummies should mitigate the crack-wave effects unless the shall-issue states are more affected than other states. Finally, in his book Lott extended the sample to 1996 and included states passing laws after the crack wave, with no change in the general conclusions. The single additional year added by Ayres and Donohue is unlikely to have a significant effect. Nevertheless, the Ayres and Donohue criticism points to the need to control for the effects of crack cocaine.

Ayres and Donohue (2003a) estimated a model with individual state trends, individual state post-law dummies, and individual state post-law trends. This model, dubbed the "hybrid" model, is a generalization of the Lott spline model. The spline model assumes that the before and after trends look like a V or inverted V, thereby disallowing an immediate impact of the law. The hybrid model introduces a dummy variable that can be interpreted to measure the immediate impact of the law and a post-law trend which captures the long run effects. Ayres and Donohue concluded, using the hybrid model that, "For every crime type, there are more states where shall-issue laws produce a positive and statistically significant coefficient than states that produce a negative and statistically significant coefficient" (1232). They also computed the net effect of the law across all states. They estimated, "an increased cost ranging between $3 and $524 million" (1284). Thus, Ayres and Donohue present evidence that shall-issue laws increase crime.

However, Ayres and Donohue limit their analysis to the first five years after the law's passage. This five-year span has the effect of emphasizing the impact of the dummy variable and downplaying the impact of the long-run post-law trend. Since they find that shall-issue generally increases crime in the short run but decreases crime in the long run, the five-year span directly affects the overall result. We can show this by calculating the short and long run benefits and costs

using Ayres and Donohue's estimated coefficients.[6] Their results imply a short run increase of $4.23 billion in crime costs from the dummies, with an accompanying decrease of $1.25 billion per year from the trends. Thus, as Table 1 shows, the costs (negative values) increase for the first three years then start to decline in year four. Beginning in year six, the long run benefits (positive values) exceed the short run costs and the benefits grow continuously from then on. Ayres and Donohue stop their calculations at five years, ignoring the $1.25 billion per year reduction in crime costs in all further years. Since no shall-issue laws have been repealed and some states have had these laws on the books for decades (e.g., New Hampshire has had a shall-issue law since 1923), extrapolating the results to ten years is reasonable. Also, as of 2000 the last year of our data set, 14 of the 25 states that passed shall-issue laws within the sample period have had them on the books for six years or longer while nine of those states have had a shall-issue law for ten years or more (see Table 9 below.). Finally, as of 2008, *all* of the states that have ever passed shall-issue laws have had the law on their books for more than ten years—it is not as though the laws tend to come and go. We grant that an analysis should not allow an "eternity" of the trend effect determine the results. Rather, the analysis should extend out for some appropriate finite span. Based on the data and historical pattern of retaining shall-issue laws, we think that a ten-year span is appropriate, and that five years is certainly too short.

Discounting with a social discount rate between zero and four percent does not change the results. Using a social discount rate between .041 and .17 delays the appearance of net positive benefits by one year.[7]

The Ayres and Donohue article was followed in the same issue by a response by Plassmann and Whitley (2003), as well as a rejoinder by Ayres and Donohue (2003b). Plassmann and Whitley said that counting positive versus negative coefficients (by state) is not enough. Using Ayres and Donohue's own estimates from the aggregate model, they show that crime declines after shall-issue laws are

6 Their coefficients are taken from Ayres and Donohue (2003a, 1310-1311, Appendix Table 7). They are also available on Ayres' website, http://islandia.law.yale.edu/ayers/indexempirical.htm.

7 Ayres and Donohue's implied results were calculated as follows. Their coefficients on the individual state dummy and trend for each crime in each state, which represent percent changes, were multiplied by the level of each crime in each state in the year of passage, to get the change in crime due to the passage of the shall-issue law. The changes in crime were multiplied by the cost of each crime from Miller, Cohen and Wiersema (1996) converted to real 2000 dollars to get the implied change in the costs of each crime. The implied costs were summed across crimes to get the implied change in the total cost of crime for each state and then summed across states to get the implied change for the US as a whole. The spreasheet showing these calculations is available at C.E. Moody's website (link).

Table 1: Ayres and Donohue Implied Benefits of the Shall-Issue Law

Discount Rate	0			.025			.05		
Year	Dummy	Trend	Cumulative Effect	Dummy	Trend	Cumulative Effect	Dummy	Trend	Cumulative Effect
1	-4.23	1.25	-2.98	-4.23	1.25	-2.98	-4.23	1.25	-2.98
2	-4.23	2.50	-4.71	-4.13	2.44	-4.67	-4.03	2.38	-4.63
3	-4.23	3.75	-5.19	-4.03	3.57	-5.12	-3.84	3.40	-5.06
4	-4.23	5.00	-4.42	-3.93	4.64	-4.41	-3.65	4.32	-4.40
5	-4.23	6.25	-2.40	-3.83	5.66	-2.58	-3.48	5.14	-2.74
6	**-4.23**	**7.50**	**0.87**	**-3.74**	**6.63**	**0.31**	-3.31	5.88	-0.17
7	-4.23	8.75	5.39	-3.65	7.55	4.21	**-3.16**	**6.53**	**3.20**
8	-4.23	10.00	11.16	-3.56	8.41	9.06	-3.01	7.11	7.30
9	-4.23	11.25	18.18	-3.47	9.23	14.82	-2.86	7.61	12.05
10	-4.23	12.5	26.45	-3.39	10.01	21.35	-2.73	8.06	17.38

Note: costs are negative, benefits are positive.

passed. In their rejoinder, Ayres and Donohue (2003b) reiterate that their F-tests rejected the null hypothesis that the effect of the laws was the same across states, rejecting the aggregate model. Thus, they contend, the results of the aggregate model, presented by Ayres and Donohue (2003a) and used by Plassmann and Whitley, were originally presented only to show how wrong one can be when combining effects across states.

In 2004, the National Research Council of the National Academies produced a meta-study on gun violence that concluded with respect to shall-issue laws that "with the current evidence it is not possible to determine that there is a causal link between the passage of right-to-carry laws and crime rates" (National Research Council 2004, 150). However, the Committee did some independent analyses that indicated that shall-issue laws reduce murder (269-70).

In Table 2 we list the key research items of the debate. In our judgment, the weight of evidence—particularly that of peer-review—indicates that shall-issue laws reduce crime. Although Ayres and Donohue (2003a, 1397) conclude that "the best evidence suggests overall small increases in crime associated with the adoption of shall-issue laws," that conclusion relies on ignoring their own implied long-run reductions in crime. In the next section we offer a fresh statistical analysis based on the method of Ayres and Donohue, but our investigation improves the method and extends the data through 2000 (Ayres and Donohue's data was through 1997).

Table 2: Academic Evidence on the Relationship between Shall-Issue Laws and Crime

Shall-Issue Reduces Crime
Refereed journal articles and books
J.R. Lott and D.B. Mustard. 1997. Crime, deterrence, and right-to-carry concealed handguns. *Journal of Legal Studies* 26: 1-68. _____1998. The concealed handgun debate. *Journal of Legal Studies* 27: 221-243. W.A. Bartley and M.A. Cohen. 1998. The effect of concealed weapons laws--an extreme bound analysis. *Economic Inquiry* 36: 258-265. S.G. Bronars and J.R. Lott 1998. Criminal deterrence, geographic spillovers, and the right to carry concealed handguns. *American Economic Review* 88: 475-479. B.L. Benson and B.D. Mast. 2001. Privately produced general deterrence. *Journal of Law and Economics* 44: 725-746. C.E. Moody. 2001. Testing for the effects of concealed weapons laws: Specification errors and robustness. *Journal of Law and Economics* 44:799-813. D.B. Mustard. 2001. The impact of gun laws on police deaths. *Journal of Law and Economics* 44:635-657. D.E. Olsen and M.D. Maltz. 2001. Right-to-carry concealed weapons laws and homicide in large U.S. counties: the effect on weapons types, victim characteristics, and victim-offender relationships. *Journal of Law and Economics* 44:747-770. F. Plassmann and T. N. Tideman. 2001. Does the right to carry concealed handguns deter countable crimes? only a count analysis can say. *Journal of Law and Economics*, 44, pp. 771-798. J.R. Lott. 1998, 2001. *More guns, less crime : understanding crime and gun-control laws*. Chicago, University of Chicago Press. E. Helland and A. Tabarrok. 2004. Using Placebo Laws to Test 'More Guns, Less Crime.' *Advances in Economic Analysis & Policy* 4 : Issue. 1, Article 1.
Non-Refereed
F. Plassmann and J. Whitley. 2003. Confirming 'more guns, less crime.' *Stanford Law Review* 54: 1313-1369. J. R. Lott and W.M. Landis. 1999, 2001, 2003. Multiple victim public shootings, bombings and right-to-carry concealed handgun laws: contrasting private and public law enforcement. Link. Published as Chapter 6 of J. R. Lott. *The bias against guns*. Washington, DC, Regnery.
Unpublished
J. R. Lott. 2004. Right-to-carry laws and violent crime revisited: clustering, measurement error and state-by-state breakdowns. Working paper, American Enterprise Institute.

Shall-Issue Increases Crime
Referred journal articles: none.
Non-Refereed
I. Ayres and J.J. Donohue. 2003. Shooting down the more guns, less crime hypothesis. *Stanford Law Review* 54: 1193-1312. _____2003. The latest misfires in support of the 'more guns, less crime' hypothesis. *Stanford Law Review* 54: 1371-1398. J.J. Donohue. 2003. The impact of concealed carry laws. In J. Ludwig and P.J. Cook (eds.). *Evaluating Gun Policy*, Washington, DC: The Brookings Institution, 287-325.
Unpublished: none.
Shall-Issue Has No Significant Effect on Crime
Refereed
D.A. Black and D.S. Nagin. 1998. Do right-to-carry laws deter violent crime? *Journal of Legal Studies* 27: 209-219. H. Dezhbakhsh and P.H. Rubin. 1998. Lives saved or lives lost--the effects of concealed-handgun laws on crime. *American Economic Review* 88: 468-474. J. Ludwig. 1998. Concealed-gun-carrying laws and violent crime: Evidence from state panel data. *International Review of Law and Economics* 18: 239-254. M.V. Hood and G.W. Neeley. 2000. Packin' in the hood?: examining assumptions of concealed-handgun research. *Social Science Quarterly* 81: 523-537. G. Duwe, T. Kovandzic, and C.E. Moody. 2002. The impact of right-to-carry concealed firearm laws on mass public shootings. *Homicide Studies* 6: 271-296. T. Kovandzic and T.B. Marvell. 2003. Right-to-carry concealed handguns and violent crime: crime control through gun decontrol? *Criminology and Public Policy* 2: 363-396. National Research Council. 2005. *Firearms and Violence: A Critical Review*. Committee to Improve Research Information and Data on Firearms. Charles F. Wellford, John V. Pepper, and Carol V. Petrie, editors, Washington, DC: The National Academies Press. Kovandzic, T. V., T.B. Marvell, and L.E, Vieraitis. 2005. The Impact of 'Shall-Issue' Concealed Handgun Laws on Violent Crime Rates. *Homicide Studies*, 10: 292-323.
Non-refereed: none. **Unpublished**: none.

SHALL-ISSUE LAWS REVISITED

We apply the Ayres and Donohue hybrid model to the county data set extended through 2000, encompassing three additional years of data and all addi-

tional law enactments.[8] Also, we modify their model by adding two new variables. Ayres and Donohue argue that the crack wave gave rise to a spurious correlation in Lott and Mustard's statistical analysis based on data from 1977 to 1992—an omitted variable problem. Fortunately, a measure of crack cocaine activity has been developed by Fryer et al (2005). The measure is derived from cocaine arrests, cocaine-related emergency room visits, cocaine-induced drug deaths, newspaper reports, and DEA drug busts. The inclusion of this variable should allay concerns of a spurious correlation with the crack wave.

Table 3: Variable Names, Definitions, and Means

Ratmur	murder rate per 100,000	5.307
rattrap	rape rate per 100,000	20.637
Ratrob	robbery rate per 100,000	45.925
Rataga	aggravated assault rate per 100,000	196.571
Ratbur	burglary rate per 100,000	758.450
Ratlar	larceny rate per 100,000	1777.471
Rataut	auto theft rate per 100,000	173.088
Shallf	shall-issue dummy	0.278
Crack	crack cocaine index	0.878
Prison	prison population per capita	0.003
Aovio	arrest rate for violent crime	74.247
Aopro	arrest rate for property crime	30.366
execrate	execution rate	0.002
unemprt	unemployment rate	6.097
Rpcpi	real per capita persional income ($1000)	11.408
Rpcui	real per capita unemployment insurance	61.923
Rpcim	real per capita income maintenance	182.912
Rpcrpo	real per capita retirement payments	1619.632
Povrate	poverty rate	14.025
Popc	county population	7.895
ppbm1019	percent population black males 10-19	0.008
ppbf1019	percent population black females 10-19	0.008
ppbm2029	percent population black males 20-29	0.007
ppbf2029	percent population black females 20-29	0.008
ppbm3039	percent population black males 30-39	0.007
ppbf3039	percent population black females 30-39	0.007
ppbm4049	percent population black males 40-49	0.005
ppbf4049	percent population black females 40-49	0.006
ppbm5064	percent population black males 50-64	0.006
ppbf5064	percent population black females 50-64	0.007

8 The dataset is available at http://www.johnlott.org. All the studies cited above make use of this dataset (though differing years of it).

ppbm65o	percent population black males 65 and over	0.006
ppbf65o	percent population black females 65 and over	0.007
ppwm1019	percent population white males 10-19	0.070
ppwf1019	percent population white females 10-19	0.071
ppwm2029	percent population white males 20-29	0.062
ppwf2029	percent population white females 20-29	0.063
ppwm3039	percent population white males 30-39	0.063
ppwf3039	percent population white females 30-39	0.064
ppwm4049	percent population white males 40-49	0.053
ppwf4049	percent population white females 40-49	0.054
ppwm5064	percent population white males 50-64	0.065
ppwf5064	percent population white females 50-64	0.067
ppwm65o	percent population white males 65 and over	0.063
ppwf65o	percent population white females 65 and over	0.067
ppnm1019	percent population neither males 10-19	0.003
ppnf1019	percent population neither females 10-19	0.003
ppnm2029	percent population neither males 20-29	0.003
ppnf2029	percent population neither females 20-29	0.002
ppnm3039	percent population neither males 30-39	0.003
ppnf3039	percent population neither females 30-39	0.003
ppnm4049	percent population neither males 40-49	0.002
ppnf4049	percent population neither females 40-49	0.002
ppnm5064	percent population neither males 50-64	0.002
ppnf5064	percent population neither females 50-64	0.002
ppnm65o	percent population neither males 65 and over	0.002
ppnf65o	percent population neither females 65 and over	0.002

Our second novel variable is a lagged dependent variable, which is included to capture effects through time. An equation with a lagged dependent variable is a first-order difference equation, which can display patterns of growth, decline, or oscillation. The Ayres and Donohue model is completely static. It suffers from potentially serious omitted variable bias if the lagged dependent variable is significant. In addition to these two variables, we include all the variables used by Ayres and Donohue, including individual state trends, county dummies, and year dummies. Like Ayres and Donohue we disaggregate the effect of the shall-issue law to the state level. The target variables are the individual state shall-issue dummy variables and corresponding post-law trends. The shall-issue dummies take the unit value in the first full year following the passage of a shall-issue law. The post-law trends are zero up to the year of passage with the trend starting in the first full year after passage. We use Lott's coding.[9] The sanction variables are the arrest rate for violent crime, the arrest rate for property crime, the per capita prison population, and, in the case

9 There is some disagreement as to the exact dates of the passage of the various shall-issue laws. In preliminary analyses we used both the Ayres and Donohue dates and the Lott dates. The results were the same. Here we use the Lott dates.

of murder, the execution rate.[10] The control variables are those used in previous analyses. The variable names, definitions, and means are presented in Table 3.

Again, shall-issue laws are state laws, applicable to all counties within the state.[11] Consequently, all counties within a state have the same values for the shall-issue dummy and post-law trend, implying that the errors are likely to be correlated across counties within states. This causes the usual standard errors to be underestimated and the t-ratios to be overestimated, potentially causing spurious correlation between the shall-issue laws and crime rates (Moulton 1990). To avoid that problem, we use heteroskedastic-consistent ("robust") standard errors corrected for clustering within states.[12] Because of the large number of zeroes in the murder and rape variables, 39 percent and 21 percent respectively, we add a small constant, .10, to these variables before taking logs. This changes the mean, but not the variance and therefore does not create measurement error. We recognize that there are good theoretical reasons for using methodologies specifically developed for count data, especially for relatively rare crimes such as murder and rape (see Plassmann and Tideman 2001). However, nearly all the articles in this literature, including Ayres and Donohue, use ordinary least squares and we continue the practice here. Also, the large number of observations (over 65,000) combined with the large number of variables (over 160) makes nonlinear procedures such as the negative binomial computationally difficult to carry out. Adding a small constant before taking logs is also standard practice. If we do not add this constant, all counties with zero crimes are dropped from the analysis. This has the effect of underestimating the effect of the shall-issue law because only positive crime rate counties are included, therefore the policy cannot reduce crime to zero. The coefficient on any crime policy variable is already biased toward zero in such cases because no policy can reduce the crime rate to a negative number. After inspecting Ayres and Donohue's paper, results and do files, and attempting to replicate their results, our best guess as to their treatment of zeros is that they used the variables as originally defined by Lott, who included a small constant. Therefore, it would seem that our treatment of the zeros should not be a significant source of discrepancy between our results and those of Ayres and Donohue.

The results with respect to the interesting control variables are presented in Table 4.[13]

10 The arrest rate is the clearance rate (arrests/crimes). The arrest rate might be endogenous in the crime equation. For that reason we dodge the simultaneity issue by using the arrest rate for all violent crimes in the murder, rape, robbery, and assault equations and the arrest rate for all property crime in the burglary, larceny, and auto theft equations. Doing so also alleviates the problem of zero arrest rates causing the county to be dropped from the data set.

11 Except for Philadelphia, which was initially exempt from Pennsylvania's shall-issue law.

12 Neither Ayres and Donohue nor Lott and Mustard corrected their standard errors for clustering.

13 To conserve space, we do not report the coefficients on the 36 demographic variables, the individual state trends, the year dummies, and the individual county intercepts. The coefficients on the shall-issue law shift dummy and post-law trend variables are presented in Tables 5a, 5b, 7a, and 7b below. Complete results, data, and Stata programs are available at C.E. Moody's website (link). We do not compute equa-

Table 4: Estimated Coefficients

	murder		rape		robbery		assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
Crack	0.0320	1.45	0.0447	1.57	**0.0709**	3.99	**0.0200**	2.60
Prison	**-91.7578**	-3.80	-50.0644	-0.43	**-101.1922**	-5.09	-14.6002	-1.13
Aovio	**-0.0004**	-3.06	**-0.0006**	-4.47	**-0.0009**	-5.94	**-0.0008**	-4.85
unemprt	-0.0143	-1.41	**-0.0140**	-1.29	-0.0008	-0.07	-0.0010	-0.21
Rpcpi	0.0016	0.29	**-0.0095**	-1.93	0.0025	0.34	-0.0044	-0.88
Rpcui	-0.0341	-0.16	-0.0614	-0.17	0.0002	0.68	**-0.2767**	-2.15
Rpcim	0.1289	0.44	0.5254	0.88	-0.0001	-0.30	**-0.2309**	-1.70
Rpcrpo	-0.0235	-0.21	0.0523	0.25	0.0000	0.48	**0.1293**	1.87
Povrate	-0.0005	-0.10	0.0085	0.90	0.0020	0.52	-0.0003	-0.14
Popc	**0.0006**	1.65	**-0.0040**	-4.77	**-0.0005**	-1.98	0.0001	0.25
Y(t-1)	0.0130	1.51	**0.1241**	4.24	**0.1104**	6.38	**0.3663**	13.81
R-square	0.65		0.66		0.85		0.83	
N	54169		54148		58844		58830	

	burglary		larceny		auto	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
Crack	**0.0284**	3.56	**0.0284**	3.56	**0.0634**	3.90
Prison	**-38.9346**	-2.61	**-38.9346**	-2.61	**-85.4660**	-5.72
Aovio	**-0.0005**	-5.95	**-0.0005**	-5.95	**-0.0006**	-5.10
unemprt	0.0077	1.46	0.0077	1.46	-0.0037	-0.41
Rpcpi	**-0.0078**	-2.91	**-0.0078**	-2.91	**0.0105**	1.87
Rpcui	0.0561	0.59	0.0561	0.59	0.3479	1.07
Rpcim	-0.0169	-0.15	-0.0169	-0.15	-0.1613	-0.39
Rpcrpo	0.0834	1.56	0.0834	1.56	0.0853	0.87
Povrate	0.0006	0.29	0.0006	0.29	0.0015	0.56
Popc	**-0.0006**	-3.29	**-0.0006**	-3.29	**-0.0011**	-1.80
Y(t-1)	**0.3656**	6.03	**0.3656**	6.03	**0.2788**	4.81
R-square	0.86		0.87		0.83	
N	61550		61550		61551	

Notes: The dependent variable is the crime rate logged. Because of the relatively large number of zeroes in the murder and rape counts we added .10 to the per capita rates before taking logs. Y(t-1) is the lagged dependent variable. Coefficients in bold are significantly different from zero at the .10 level, two-tailed. We suppress the estimated coefficients on the 36 demographic variables, the year dummies, the individual state trends, and the 24 shall-issue dummies and post-law trends. Complete results are available at C.E. Moody's website (link). The execution rate was not significant in the murder equation and was dropped. The overall results were unchanged.

tions for total crime, violent crime, or property crime because these aggregates merely count the various subcategories. Therefore, because there are so many more assaults than murders, rapes, or robberies, violent crime is virtually indistinguishable from assault. Similarly, property crime and total crime are dominated by larceny, the most common type of index crime.

The crack variable is significant and positive in all of the crime equations, except murder and rape, indicating that the crack wave had significant effects on most crime categories. Of the sanctioning variables, prison population has a significantly negative effect on murder, robbery, burglary, larceny, and auto theft. Arrest rates have negative and significant impacts for all crimes. Real per capita personal income (rpcpi) is negative and significant in the rape, burglary, and larceny equations and positive in the auto theft equation. Real per capita unemployment insurance payments (rpcui), real per capita welfare payments (rpcim), and real pension payments are significant only in the assault equation. The poverty rate is not significant in any of the crime equations. The population level (popc) is negatively related to rates of rape, robbery, burglary, and larceny and positively related to the murder rate. The lagged dependent variable is significant in all of the equations except murder, indicating the importance of dynamic effects in most crime categories. Although we suppress the thirty-six demographic variables for readability, they are significant as groups and are therefore retained in the regressions. The year dummies and individual state trends are also jointly significant.[14]

The results with respect to the state-specific dummy variables are presented in Table 5a and Table 5b.

14 In the rape, robbery, assault, and auto theft equations, we tested for and found significant negative autocorrelation. The effect of negative autocorrelation on the standard errors and t-ratios is unknown. Because we use heteroskedastic consistent standard errors corrected for clustering on states, we partially correct for autocorrelation. We believe that our hypothesis tests are valid.

Table 5a: Shall-issue Dummy Coefficients: Violent Crime

	Murder		Rape		Robbery		Assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	0.125	1.38	**-0.517**	-4.31	-0.024	-0.31	0.038	0.89
AZ	**0.264**	6.2	-0.064	-0.82	**0.171**	4.70	**0.053**	1.97
AR	0.048	1.04	0.031	0.31	**-0.073**	-2.19	**0.099**	4.56
FL	-0.089	-1.22	**-0.181**	-2.87	**0.141**	1.88	**0.073**	2.82
GA	**-0.200**	-4.28	-0.052	-0.89	**-0.151**	-3.62	**-0.052**	-2.67
ID	**0.978**	23.04	**0.302**	2.24	0.093	1.20	0.030	0.99
KY	0.046	0.90	**-0.301**	-3.98	**0.277**	5.29	**0.160**	6.36
LA	**0.381**	6.15	0.113	1.62	**0.287**	4.44	0.056	1.58
ME	**0.460**	9.70	0.121	1.64	**-0.144**	-3.08	**-0.151**	-5.15
MS	0.067	1.21	-0.034	-0.47	**0.143**	3.38	**0.115**	4.28
MT	0.008	0.22	0.233	1.56	**-0.430**	-6.90	**-0.210**	-7.92
NV	**0.551**	12.65	**0.151**	1.99	**0.107**	2.48	**0.174**	5.24
NC	0.009	0.20	0.053	0.41	**0.090**	2.11	**0.102**	3.61
OK	**0.090**	2.21	0.060	1.14	-0.062	-1.33	0.006	0.27
OR	**-0.213**	-5.18	0.025	0.30	**-0.240**	-4.14	**0.049**	1.91
PA	-0.022	-0.51	0.064	1.19	**-0.061**	-1.87	**-0.051**	-2.27
PH	-0.024	-0.63	**-0.344**	-5.41	**-0.060**	-1.77	**-0.213**	-7.70
SC	0.050	1.05	-0.126	-1.54	-0.052	-0.98	0.055	1.60
TN	-0.026	-0.69	**-0.154**	-2.51	**-0.091**	-2.85	0.046	1.48
TX	-0.055	-1.16	0.103	0.44	0.046	0.85	0.024	0.88
UT	**0.100**	1.66	-0.034	-0.38	**0.078**	1.74	**0.214**	6.38
VA	0.030	0.60	**0.107**	1.97	-0.054	-1.39	**-0.040**	-2.09
WV	**0.285**	6.44	0.100	1.47	-0.064	-1.51	**-0.075**	-2.70
WY	**-0.266**	-3.92	-0.003	-0.02	**0.512**	7.69	-0.042	-1.18
US	0.006	0.06	-0.007	0.01	0.008	0.10	**0.031**	6.36
negative	8		11		13		8	
significant	3		5		8		7	
positive	16		13		11		16	
significant	8		3		9		9	

Notes: Coefficients in bold are significant at the .10 level. The test statistics for the US weighted average are F-ratios corresponding to the null hypothesis that the weighted average is zero. PH is Philadelphia.

For all crimes except robbery and burglary, the number of states[15] with a positive effect as measured by the coefficient on the dummy variable is greater than those with a negative effect. Also, the population-weighted average across all states is positive for all crimes except rape and burglary and significantly positive for assault and auto theft. We computed the harm-weighted long run effect of these laws by multiplying the implied change in the number of crimes by the cost to the victims of each type of crime. The victim costs are taken from Miller, Cohen and Wiersema (1996, Table 2) and are adjusted to real 2000 dollars using the consumer price index (cpi-u-rs). The relevant per-victim costs are as follows:

15 Because Philadelphia was excluded from Pennsylvania's shall-issue law until 1995, we treat it as a separate jurisdiction. However, for convenience, we still refer to "states" when counting jurisdictions.

murder $3.44 million; rape, $101,790; robbery $9.360; assault $10,998; burglary $1,638; larceny $433; auto theft $4, 329. The short-run cost associated with the passage of the shall-issue law is shown in Table 6 where we use positive values to indicate the costs of crime and negative values to indicate that crime and its related costs have been reduced.

Table 5b: Shall-issue Dummy Coefficients: Property Crime

	Burglary		Larceny		Auto Theft	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	-0.021	-0.36	-0.044	-0.86	**-0.133**	-2.38
AZ	**0.073**	3.71	**0.058**	2.69	**0.175**	3.72
AR	**-0.075**	-4.27	0.004	0.24	-0.023	-0.72
FL	0.005	0.18	-0.014	-0.58	**0.154**	2.71
GA	**-0.124**	-5.11	**-0.081**	-4.65	**-0.167**	-4.74
ID	-0.015	-0.44	**0.070**	2.04	0.094	1.59
KY	-0.025	-1.03	**-0.079**	-4.06	**0.075**	1.70
LA	0.043	1.59	**0.052**	2.32	**0.235**	5.49
ME	0.007	0.32	**0.051**	3.01	0.077	1.61
MS	-0.031	-0.97	0.006	0.28	-0.044	-1.24
MT	**0.117**	3.79	0.029	1.01	0.023	0.43
NV	**0.159**	5.10	**0.107**	3.20	**0.142**	4.79
NC	-0.026	-0.83	**0.061**	2.29	**0.201**	5.66
OK	0.013	0.46	0.041	1.39	-0.029	-0.69
OR	**-0.084**	-2.73	0.016	0.64	-0.016	-0.31
PA	-0.021	-1.06	0.012	1.15	0.004	0.17
PH	**-0.107**	-2.76	**-0.235**	-8.04	-0.047	-1.18
SC	**-0.052**	-1.90	-0.025	-1.14	0.074	1.60
TN	**-0.036**	-1.68	**-0.037**	-1.71	-0.047	-1.41
TX	**0.073**	2.12	0.050	1.49	0.078	1.59
UT	**0.079**	1.96	-0.052	-1.58	**0.188**	4.55
VA	**-0.072**	-2.88	-0.012	-0.52	**-0.101**	-2.47
WV	**0.063**	2.06	**0.078**	4.26	**-0.093**	-2.12
WY	**0.145**	5.18	**0.071**	2.31	**0.165**	2.91
US	-0.010	0.30	0.009	0.39	**0.050**	6.47
negative	13		9		10	
significant	8		4		4	
positive	11		15		14	
significant	7		8		8	

Note: See notes to Table 5a.

The short run cost is $1.2 billion. All crime categories, except rape and burglary, show positive costs due to increases in crime.[16]

Table 6: Short Run Costs and Benefits, Millions of 2000 Dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	11.69	-13.68	-0.18	0.93	-0.15	-0.34	-1.59	-3.32
AZ	383.40	-9.29	10.40	11.02	5.73	3.67	29.71	434.65
AR	42.60	2.93	-2.12	9.84	-3.21	-0.17	-1.12	48.75
FL	-419.15	-111.36	56.84	55.88	9.05	0.69	59.40	-348.64
GA	-551.77	-16.46	-24.31	-14.03	-18.74	-6.43	-29.36	-661.10
ID	92.12	8.41	0.13	0.72	-0.41	0.66	0.50	102.12
KY	15.44	-13.95	8.10	10.09	-1.12	-1.60	1.28	18.25
LA	977.93	19.70	31.62	14.60	4.09	3.74	26.83	1078.52
ME	44.72	2.06	-0.38	-2.34	0.37	0.80	0.80	46.02
MS	43.52	-2.49	2.31	4.22	-2.48	-0.36	-0.92	43.79
MT	0.37	2.68	-0.40	-1.21	0.48	0.08	-0.21	1.79
NV	306.86	14.27	4.96	15.20	4.88	2.23	6.34	354.75
NC	21.77	12.42	10.72	32.54	-4.47	5.93	19.16	98.07
OK	122.97	8.92	-2.18	1.08	0.16	1.25	-2.88	129.30
OR	-79.23	3.37	-9.19	4.60	-5.31	0.10	-1.78	-87.44
PA	-21.16	14.02	-4.43	-9.42	-1.39	1.45	1.02	-19.92
PH	-35.87	-27.03	-7.65	-15.82	-2.37	-4.45	-2.34	-95.52
SC	57.22	-24.20	-3.16	16.57	-3.94	-1.44	5.12	46.16
TN	-39.44	-36.50	-8.86	10.85	-5.23	-2.25	-5.08	-86.50
TX	-321.52	89.52	14.46	19.90	18.25	8.78	29.22	-141.38
UT	26.31	-2.85	0.94	9.20	1.80	-2.43	5.62	38.59
VA	46.15	16.93	-3.22	-3.66	0.77	4.32	-2.08	59.20
WV	118.94	3.53	-0.47	-1.15	1.25	0.97	-1.24	121.83
WY	-15.71	-0.04	0.38	-0.46	0.64	0.35	0.48	-14.35
US	**828.17**	**-59.08**	**74.29**	**169.16**	-1.33	**15.53**	**136.88**	**1163.63**

Notes: Costs are positive values (crime has increased) and benefits are negative values (crime has decreased). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia.

However, the results with respect to the coefficients on the post-law trends, presented in Table 7a and Table 7b, tell a different story.

16 The results are similar if we use only coefficients that are significantly different from zero at the .10 level. In that case the overall net cost to the US is $1.5 billion.

Table 7a: Shall-Issue Post-Law Trend Coefficients: Violent Crime

	Murder		Rape		Robbery		Assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	**-0.104**	-4.49	-0.041	-0.42	**-0.093**	**-4.58**	-0.023	-1.49
AZ	**-0.055**	-2.69	0.026	0.91	-0.003	-0.15	-0.010	-1.02
AR	**-0.108**	-6.22	**-0.081**	-2.98	0.004	0.17	**0.048**	5.21
FL	**-0.054**	-4.24	**0.032**	1.76	**-0.085**	-6.59	0.003	0.42
GA	0.010	1.29	**-0.066**	-5.39	-0.016	-1.26	-0.005	-0.80
ID	**-0.057**	-4.55	-0.003	-0.15	**0.077**	3.77	**0.032**	4.06
KY	-0.025	-1.16	**-0.103**	-4.81	**-0.050**	-2.17	**-0.081**	-6.86
LA	0.002	0.09	0.038	1.02	0.039	1.58	0.008	0.47
ME	**0.025**	2.95	-0.016	-0.71	-0.013	-1.16	**0.014**	2.35
MS	**0.053**	4.75	**0.059**	2.97	**0.084**	5.50	**0.067**	6.93
MT	**-0.025**	-1.84	-0.030	-1.51	**0.131**	13.39	**0.202**	24.85
NV	**-0.131**	-7.66	**-0.077**	-1.84	-0.023	-1.22	**-0.062**	-4.89
NC	-0.010	-0.57	**-0.083**	-1.65	-0.003	-0.15	-0.015	-1.34
OK	-0.002	-0.13	**-0.041**	-1.75	0.003	0.15	-0.002	-0.26
OR	**-0.083**	-8.24	-0.038	-1.64	**-0.038**	-3.43	**0.046**	6.33
PA	0.008	1.21	**-0.026**	-2.25	**0.030**	4.16	**0.011**	2.55
PH	-0.003	-0.28	**0.062**	2.50	0.032	1.18	**0.050**	4.27
SC	0.004	0.15	-0.069	-1.48	0.018	0.61	-0.020	-1.01
TN	**0.113**	9.97	**0.086**	4.74	**0.116**	6.81	**0.072**	8.03
TX	0.000	-0.01	-0.043	-1.61	-0.007	-0.45	**-0.016**	-2.26
UT	-0.016	-0.78	0.004	0.10	0.038	0.79	0.012	0.88
VA	0.001	0.09	0.004	0.42	**0.056**	3.34	**0.027**	3.81
WV	**-0.098**	-9.21	**-0.046**	-2.46	0.001	0.03	**0.105**	11.34
WY	**0.167**	7.53	-0.020	-0.81	0.018	0.77	**0.059**	5.78
US	**-0.017**	4.74	-0.022	1.77	0.0003	0.00	**0.011**	3.19
Negative	15		16		10		9	
Significant	8		8		4		3	
Positive	9		8		14		15	
Significant	4		4		6		12	

Note: see notes to Table 4a.

Table 7b: Shall-Issue Post-Law Trend Coefficients: Property Crime

	Burglary		Larceny		Auto	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	**-0.030**	-1.95	**-0.030**	-2.06	**-0.046**	-2.34
AZ	-0.003	-0.34	**-0.024**	-2.87	**-0.060**	-4.38
AR	-0.013	-1.06	-0.005	-0.51	-0.004	-0.20
FL	**-0.031**	-4.43	-0.003	-0.45	-0.007	-0.49
GA	**-0.016**	-2.66	-0.007	-0.96	**0.027**	1.95
ID	0.004	0.50	**-0.021**	-2.62	0.016	0.95
KY	**-0.025**	-1.66	-0.007	-0.61	-0.016	-1.08
LA	**0.033**	2.32	**0.024**	2.21	**0.059**	2.07
ME	-0.004	-0.60	-0.001	-0.12	-0.003	-0.37
MS	**0.049**	7.05	**0.058**	8.50	**0.100**	6.46
MT	-0.002	-0.37	**0.017**	2.92	0.007	0.89
NV	**-0.024**	-2.52	**-0.048**	-4.73	-0.010	-0.57
NC	-0.015	-1.13	-0.012	-1.04	0.020	0.93
OK	-0.010	-0.97	-0.010	-0.88	0.002	0.11
OR	**-0.013**	-1.78	0.004	0.67	**-0.022**	-2.17
PA	0.000	0.03	**0.014**	3.54	-0.006	-1.08
PH	-0.012	-1.64	0.007	0.76	0.035	1.14
SC	-0.022	-1.19	-0.006	-0.34	0.042	1.33
TN	**0.054**	5.96	**0.061**	7.44	**0.077**	4.47
TX	0.005	0.52	-0.003	-0.28	-0.004	-0.25
UT	0.007	0.36	-0.009	-0.40	0.014	0.34
VA	0.002	0.36	0.004	0.60	0.025	1.52
WV	0.003	0.34	0.000	-0.05	**0.018**	1.71
WY	**-0.021**	-2.12	**-0.018**	-1.75	0.008	0.47
US	-0.004	0.02	0.003	0.02	0.008	0.35
Negative	15		16		10	
Significant	6		5		3	
Positive	9		8		14	
Significant	3		5		5	

Note: see notes to Table 5a.

The number of states with negative post-law trends is greater than the number with positive trends for murder, rape, burglary, and larceny. The US weighted average trend is significantly negative for murder, the most costly crime, and significantly positive only for assault. Because, as time passes, the trend will eventually dominate the shift, the trend is the only coefficient that matters in the long run. The implied costs and benefits are presented in Table 8.

Table 8: Long Run Costs and Benefits (Post-Law Trends), Millions of 2000 Dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	-9.70	-1.08	-0.72	-0.56	-0.16	-0.23	-0.55	-13.00
AZ	-80.37	3.81	-0.19	-2.03	-0.29	-1.97	-11.18	-92.22
AR	-96.03	-7.66	0.12	4.79	-0.52	-0.16	-0.14	-99.60
FL	-254.54	19.39	-34.08	2.11	-13.80	-0.50	-2.54	-283.96
GA	27.76	-20.62	-2.52	-1.34	-2.92	-0.72	4.81	4.47
ID	-5.32	-0.09	0.11	0.77	0.05	-0.26	0.12	-4.63
KY	-8.43	-4.76	-1.47	-5.10	-0.54	-0.11	-0.43	-20.83
LA	6.29	6.72	4.28	2.16	2.92	1.70	6.84	30.91
ME	2.47	-0.27	-0.03	0.21	-0.06	-0.01	-0.03	2.28
MS	34.48	4.28	1.37	2.47	1.81	1.02	1.79	47.21
MT	-1.14	-0.34	0.12	1.17	-0.01	0.11	0.04	-0.05
NV	-72.85	-7.27	-1.06	-5.46	-0.80	-1.11	-0.49	-89.03
NC	-22.43	-19.46	-0.37	-4.73	-2.42	-1.16	1.92	-48.66
OK	-2.14	-6.11	0.12	-0.40	-0.71	-0.43	0.13	-9.54
OR	-30.93	-5.17	-1.45	4.27	-0.67	0.16	-1.23	-35.03
PA	7.24	-5.67	2.20	2.10	0.01	0.97	-0.87	5.99
PH	-4.91	4.84	4.13	3.68	-0.32	0.13	3.65	11.20
SC	4.58	-13.20	1.10	-5.93	-1.75	-0.33	2.88	-12.64
TN	170.38	20.46	11.32	17.02	4.61	3.27	9.04	236.10
TX	-0.92	-37.90	-2.12	-13.74	1.64	-0.71	-1.78	-55.53
UT	-4.32	0.35	0.46	0.52	0.18	-0.33	0.45	-2.68
VA	1.06	0.69	3.35	2.48	0.13	0.25	1.90	9.86
WV	-41.03	-1.63	0.00	1.62	0.05	0.00	0.22	-40.76
WY	9.86	-0.33	0.01	0.64	-0.11	-0.12	0.03	9.98
US	**-370.94**	**-71.03**	**-15.33**	6.74	**-13.67**	-0.53	**14.61**	**-450.15**

Notes: Costs are positive values (crime has increased) and benefits are negative values (crime has decreased). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia

All crime categories except assault and auto theft show post-law benefits from the shall-issue laws. Murder, rape, robbery, and burglary show significant benefits across all states. The overall net benefit to the US is $450 million per year.[17] At this rate, it will take approximately six years for the initial costs to be offset by the eventual long-run benefits. After that, the net benefits increase continuously. The breakeven point is the same as that implied by the Ayres and Donohue analysis.

Another way to evaluate the effect of shall-issue laws is to estimate the cumulative effect through 2000 on the states implementing them. We

17 The numbers are very similar using only significant coefficients. In that case the annual net benefit from crime reduction is $398 million per year.

estimate the cumulative effect of the law by combining the estimated coefficient on the dummy variable with the corresponding coefficient on the trend variable using the formula,

$$E_i = (N + (N-1)\hat{g} + (N-2)\hat{g}^2 + \cdots + (N-(N-1))\hat{g}^{N-1})\hat{b}_{1i} + \left(\sum_{t=1}^{N} t + \hat{g}\left(\sum_{t=1}^{N-1} t\right) + \hat{g}^2\left(\sum_{t=1}^{N-2} t\right) + \cdots + \hat{g}^{N-1}\right)\hat{b}_{2i}$$

where E_i is the effect for state i, N is the number of years the law has been in effect, $\hat{g}$ is the coefficient on the lagged dependent variable, $\hat{b}_{1i}$ is the coefficient on the shall-issue dummy for state i, and $\hat{b}_{2i}$ is the coefficient on the post-law trend for the same state. This is the cumulative effect over all the years the law has been in existence in each state, up to the year 2000. The net effect for the U.S. as a whole is computed as the population-weighted average. The results are presented in Table 9.

The number of states experiencing increases in crime is larger than the number with reductions in murder, robbery, assault, and auto theft, confirming the Ayres and Donohue finding for those crimes. On the other hand, there are more reductions for rape, burglary, and larceny. The results are similar if we only count significant coefficients. Despite the fact that the number of states with increases in crime is larger than the number experiencing declines, the overall population-weighted effect for the US is significantly negative for murder and burglary. The only crime for which the net effect of these laws across the US is significantly positive is assault. The other crimes have cumulative effects that are not significantly different from zero.

Table 9: Cumulative-2000 Effect of the Shall-Issue Laws on Crime

	Year Passed	N	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto
AK	1994	6	**-1.430**	**-3.957**	**-2.087**	-0.252	**-0.808**	**-0.894**	**-1.771**
AZ	1994	6	0.422	0.169	**0.960**	0.112	0.293	-0.241	-0.304
AR	1995	5	**-1.376**	-1.067	-0.302	**1.221**	**-0.590**	-0.107	-0.220
FL	1987	13	**-6.064**	0.516	**-5.877**	**1.197**	**-2.563**	-0.190	1.538
GA	1989	11	**-1.532**	**-4.910**	**-2.685**	**-0.905**	**-2.244**	**-1.088**	-0.030
ID	1990	10	**6.257**	**2.570**	**4.314**	**1.696**	-0.105	**-0.462**	**1.367**
KY	1996	4	-0.068	**-2.232**	**0.606**	-0.169	**-0.460**	**-0.496**	0.032
LA	1996	4	**1.550**	**0.834**	**1.539**	**0.306**	**0.515**	**0.444**	**1.518**
ME	1985	15	**9.961**	-0.110	**-3.736**	-0.635	-0.104	0.904	1.054
MS	1990	10	**3.616**	**2.897**	**6.063**	**4.863**	**2.026**	**2.996**	**5.006**
MT	1991	9	**-1.034**	0.761	**2.018**	**7.201**	**0.774**	**0.869**	-0.041
NV	1995	5	**0.792**	-0.399	0.192	-0.066	**0.378**	**-0.237**	**0.495**
NC	1995	5	-0.099	**-0.981**	0.402	**0.288**	**-0.355**	0.121	**1.299**
OK	1995	5	**0.425**	-0.316	-0.259	-0.003	-0.145	-0.004	-0.176
OR	1990	10	**-6.700**	**-1.852**	**-4.482**	**3.022**	**-1.704**	0.225	**-1.518**
PA	1989	11	0.257	-1.006	**1.336**	0.189	-0.139	**1.127**	-0.340
PH	1995	5	-0.170	**-0.795**	0.186	**-0.322**	**-0.632**	**-1.009**	0.418
SC	1996	4	0.238	**-1.196**	-0.028	0.023	**-0.426**	-0.161	**0.715**
TN	1994	6	**2.219**	**0.889**	**1.899**	**1.776**	**0.767**	**1.025**	**1.358**
TX	1995	5	-0.278	-0.139	0.129	-0.126	**0.349**	0.122	0.263
UT	1995	5	0.255	-0.108	0.957	**1.253**	**0.460**	**-0.453**	**1.080**
VA	1988	12	0.407	**1.630**	**3.737**	**1.634**	0.255	**1.028**	1.629
WV	1989	11	**-3.352**	-1.936	-0.667	**6.119**	0.913	0.897	0.090
WY	1994	6	**1.912**	-0.440	**3.449**	**0.979**	0.326	-0.054	**1.032**
US			**-1.169**	-0.589	-0.571	**0.971**	**-0.522**	0.262	0.549
Negative			11	16	9	8	13	13	8
Significant			7	7	5	2	9	7	2
Positive			13	8	15	16	11	11	16
Significant			8	5	10	13	7	6	9

Notes: coefficients in bold are significant at the .10 level using standard F-tests. Coefficients are the estimated percentage change in crime over the N years the law has been in effect. PH is Philadelphia.

We can estimate the cumulative benefits of the law using the costs of each crime and the cumulative effects from Table 9. The results are presented in Table 10.

Table 10: Cumulative-2000 Benefits of the Shall-Issue Law, Millions of 2000 Dollars

	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	-134	-116	-17	-7	-6	-9	-26	-315
AZ	626	24	64	40	42	-21	-48	727
AR	-1238	-109	-10	163	-33	-4	-10	-1241
FL	-28938	339	-2602	1375	-1691	-53	736	-30834
GA	-4289	-1722	-480	-357	-601	-174	-19	-7641
ID	597	80	7	58	-2	-7	13	745
KY	-23	-113	20	-10	-13	-10	1	-147
LA	4013	159	184	106	60	42	216	4781
ME	979	-2	-11	-16	-2	17	12	977
MS	2357	234	108	256	107	74	115	3251
MT	-48	10	2	58	6	8	0	35
NV	448	-40	10	-1	19	-6	32	462
NC	-229	-249	53	134	-80	19	158	-195
OK	589	-50	-10	0	-13	0	-16	499
OR	-2521	-278	-190	402	-135	14	-111	-2819
PA	247	-243	106	44	-22	116	-59	188
PH	-255	-71	25	-38	-23	-28	52	-340
SC	277	-249	-2	15	-44	-12	60	45
TN	3363	217	194	548	79	68	186	4656
TX	-1638	-123	45	-131	163	47	154	-1482
UT	68	-10	13	74	16	-23	44	182
VA	644	289	245	214	30	106	160	1688
WV	-1414	-76	-5	134	27	15	1	-1318
WY	113	-8	3	13	3	0	4	128
US	**-26406**	**-2105**	**-2250**	**3075**	**-2114**	**177**	**1655**	**-27969**

Notes: Costs are positive values (crime has increased) and benefits are negative values (crime has decreased). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia.

Fourteen states experienced cumulative benefits while ten states experienced cumulative costs. However, the consequences are very different across states. Louisiana and Tennessee have suffered combined increases in crime costs of approximately $10 billion, while Florida and Georgia have enjoyed benefits of crime reduction of $38 billion. The estimated population-weighted net effect across all states is a reduction in crime costs of $28 billion. The results are similar using only significant coefficients, with an estimated net benefit of $28.4 billion in reduced crime.

The cumulative results through 2000 are dominated by Florida, which benefited to the tune of $30.8 billion from passing the shall-issue law in 1987. Since

the net effect across all states is $28 billion, the other states have experienced a net increase in crime amounting to a cost of $2.8 billion. However, this sum is not significantly different from zero. Also, even without Florida, there is a long run net benefit of $183 million per year, which is significantly different from zero. If the ethically proper social discount rate is reasonably low, then the only relevant result is the ongoing long-run effect, which is less crime. Therefore, even excluding Florida, the state which has apparently benefited most from a right-to-carry law, the overall long run impact of these laws is lower crime.

Summary and Conclusion

Many articles have been published finding that shall-issue laws reduce crime. Only one article, by Ayres and Donohue who employ a model that combines a dummy variable with a post-law trend, claims to find that shall-issue laws increase crime. However, the only way that they can produce the result that shall-issue laws increase crime is to confine the span of analysis to five years. We show, using their own estimates, that if they had extended their analysis by one more year, they would have concluded that these laws reduce crime. Since most states with shall-issue laws have had these laws on the books for more than five years, and the law will presumably remain on the books for some time, the only relevant analysis extends beyond five years. We extend their analysis by adding three more years of data, control for the effects of crack cocaine, control for dynamic effects, and correct the standard errors for clustering. We find that there is an initial increase in crime due to passage of the shall-issue law that is dwarfed over time by the decrease in crime associated with the post-law trend. These results are very similar to those of Ayres and Donohue, properly interpreted.

The modified Ayres and Donohue model finds that shall-issue laws significantly reduce murder and burglary across all the adopting states. These laws appear to significantly increase assault, and have no net effect on rape, robbery, larceny, or auto theft. However, in the long run only the trend coefficients matter. We estimate a net benefit of $450 million per year as a result of the passage of these laws. We also estimate that, up through 2000, there was a cumulative overall net benefit of these laws of $28 billion since their passage. We think that there is credible statistical evidence that these laws lower the costs of crime. But at the very least, the present study should neutralize any "more guns, more crime" thinking based on Ayres and Donohue's work in the *Stanford Law Review*.

We acknowledge that, especially in light of the methodological issues of the literature in general, the magnitudes derived from our analysis of crime statistics and the supposed costs of crime might be dwarfed by other considerations in judging the policy issue. Some might contend that allowing individuals to carry a concealed weapon is a moral or cultural bad. Others might contend that greater

liberty is a moral or cultural good. All we are confident in saying is that the evidence, such as it is, seems to support the hypothesis that the shall-issue law is generally beneficial with respect to its overall long run effect on crime.

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ABOUT THE AUTHOR

Carlisle E. Moody is Professor of Economics at the College of William and Mary.

Thomas B. Marvell is an attorney-sociologist and is director of Justec Research in Williamsburg, VA.

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