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The Debate on Shall Issue Laws, Continued

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ABSTRACT

Introduction

We want to be clear on one point. We are not advocates of the shall-issue law. We have both published papers that have found that shall-issue laws reduce crime (Moody 2001; Marvell 2001, fn 29) and do not reduce crime (Duwe, Kovandzic and Moody 2002; Kovandzic and Marvell 2003; Kovandzic, et al. 2005).

Our paper (Moody and Marvell 2008) criticizes Ayres and Donohue on only one point. Their response article (Ayres and Donohue 2009) discusses that point briefly and then criticizes our paper with a large number of arguments. Our single criticism of Ayres and Donohue concerned their article in the *Stanford Law Review* claiming that shall-issue laws increase crime (2003a). This claim is based on their regression model, the "hybrid model," that uses both a dummy variable (zeroes before and ones after the shall-issue law) and a post-law time trend (zeroes before and a trend beginning with the implementation of the law) to capture the effect of the law on crime. They use separate shall-issue law variables for each state. This procedure, they stress, is the best way to analyze this issue, so we limited ourselves to that.[3] Our objection is that, when calculating the impact of the shall-issue laws, they used only the first five years of the trend variables and ignored the rest. Using this technique, they concluded that shall-issue laws increase crime. When we added a sixth year, the results were reversed. Their model showed that shall-issue laws reduced crime for all years after the fifth. This implied beneficial effect continually increased with each additional year.

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3 "We take these disaggregated (state-specific) hybrid regressions to be our most definitive … The disaggregated hybrid model that we have finally settled on allows the data to reveal a variety of different impacts of the law—allowing separate intercept and trend effects for each of the 24 passing jurisdictions" Ayres and Donohue (2003b: 143).

Ayres and Donohue (2009, 36) argue that it is risky to carry trends out too many years. This indicates that perhaps they did not believe the implications of their model, since their model contained trend variables that continue indefinitely. Also, their data contained trends that extended to the end of the data set, often extending well past five years. The estimated trend coefficients are therefore based on all the years. They also say that some states did not have more than five years of after-law data, but ten of the twenty-four states have data out to at least six years (Moody and Marvell 2008, 289).

The rest of this paper discusses Ayres and Donohue's other points. They list four topics: (1) our literature review, (2) our attempt to improve on their model, (3) the influence of Florida on the overall results, and (4) their own new estimates. To help the reader we follow the arguments in order, although some issues cross the four topics.

Our Literature Review

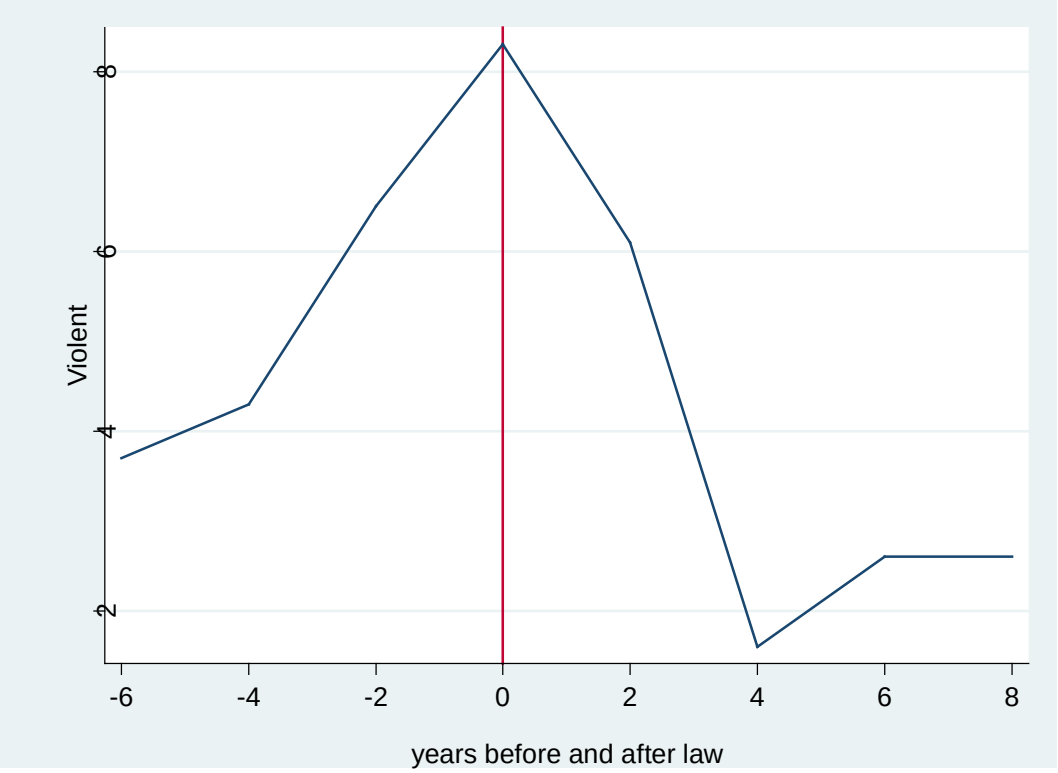
Ayres and Donohue (2009, 35-39) argue that, by not taking quality into account, we overemphasized studies finding that shall-issue laws reduced crime and underemphasized studies finding the opposite. The latter group consists entirely of work by themselves, except for a study that came out last November (Grambsch 2008).[4] We apologize for missing Donohue (2004).

However, we note that Donohue (2003) reports a model that again does not prove what its author claims it proves. Donohue estimated a number of regressions, including several that, instead of estimating a dummy and trend, estimated groups of dummy variables for years before and years after the shall-issue law. He concludes, "…the effect for the 'two or three years after' dummy is seen to be highly positive and statistically significant …Certainly, there is no evidence of any statistically significant decline in the value of the estimated effect across these two periods, which is what one would expect if shall-issue laws reduced crime" (Donohue 2003, 312). In a response article Mustard (2003, 329) noted that, "…although it is important to know whether the coefficient estimates in the postlaw years are positive or negative, it is also important to understand how they compare to the prelaw estimates. For example, if the prelaw estimate is 8.5, and the postlaw estimate 5.5, the law may have lowered the crime rate in shall-issue states relative to the other states." We illustrate this point by graphing Donohue's coefficient estimates of the violent crime equation.[5]

4 Grambsch tries to control for a possible tendency for states with shall-issue laws to have more crime growth before the laws, meaning that there is a possible regression-to-the-mean effect. The years when the laws were passed are wrong for 6 of the 25 shall-issue laws she studied, and this analysis (unlike, say, using a step dummy) requires precisely correct dates. She also fails to include many routine and important control variables, such as year effects, state trends, lagged dependent variables, prison population, and police levels.

5 Graphs of the individual violent crimes are presented in Mustard (2003, 330). They all show large declines after the passage of a shall-issue law.

Figure 1: Donohue's (2003) Estimates of the Effect of Shall-Issue Laws on Violent Crime[6]



Note that Donohue's own coefficients indicate a considerable benefit of the shall-issue law. Donohue might choose to interpret this graph as a regression to the mean, but that requires that the turning point just happens to occur at the time when the shall-issue law goes into effect (in different years in different states). We also note that the post-law mean is well below the pre-law mean.

Our Estimates of Their Model

In Moody and Marvell (2008), we took the Ayres and Donohue hybrid model, added three more years of data, addressed two shortcomings (adding a lagged dependent variable and correcting standard errors for clustering of counties within states), and used a new control variable for crack. The results turned out to be very similar to theirs.[7]

6 This is a graph of Donohue's coefficients estimating the percent change in violent crime from his Table 8-5, "Entire Period (1977-97)" (Donohue 2003, 309).
7 Complete results, data, and Stata .do-files are available at cemood.people.wm.edu/ADreply.zip.

Table 1: Variable Names, Definitions, and Means

Variable	Definition	Mean
ratmur	Murder rate per 100,000	5.253
ratrap	Rape rate per 100,000	20.789
ratrob	Robbery rate per 100,000	45.925
rataga	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 violent crime	74.247
aopro	Arrest rate property crime	30.366
unemprt	Unemployment rate	6.097
rpcpi	Real per capita personal income	11.408
rpcui	Real per capita unemployment insurance	0.062
rpcim	Real per capita income maintenance	0.183
rpcrpo	Real per capita retirement payments	1.620
povrate	Poverty rate	14.025
popc	County population	7.895
ppb	Percent population black	0.081
pp1019	Percent population 10-19	0.163
pp2029	Percent population 20-29	0.145
pp3039	Percent population 30-39	0.146
pp4049	Percent population 40-49	0.122
pp5064	Percent population 50-64	0.150
pp65o	Percent population 65 and over	0.147

Source: See note 7.

However, as Ayres and Donohue point out (2009, 51), we erroneously dropped 1998 data from our murder and rape regressions and we had the wrong date for the Philadelphia law. We made these corrections and also took their advice (2009, 52 fn 14) and collapsed the 36 demographic variables into the percent of the population that is black and the percent of the population in the six age groups in order to reduce multicollinearity and mitigate data problems. The net effect of these changes is that the estimated costs implied by the generally positive short-run dummy coefficients are increased but so are the estimated benefits implied by the long run trends. The long

run benefits now exceed the short run costs in less than three years.[8] See Tables 1-6.

Table 2: Estimated Coefficients

Variable	Murder		Rape		Robbery		Assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
Y(t-1)	**0.018**	2.03	**0.142**	4.91	**0.115**	6.94	**0.368**	13.55
crack	**0.055**	2.20	**0.055**	1.95	**0.082**	4.08	**0.024**	3.13
prison_1	**-63.975**	-2.34	-72.242	-0.70	**-88.985**	-4.01	-14.946	-1.07
aovio	**-0.001**	-3.02	**-0.001**	-4.51	**-0.001**	-5.78	**-0.001**	-4.76
unemprt	-0.016	-1.63	**-0.018**	-1.70	-0.001	-0.10	-0.002	-0.39
rpcpi	-0.003	-0.54	**-0.026**	-5.33	-0.005	-0.98	-0.006	-1.44
rpcui	-0.061	-0.24	0.019	0.06	0.176	0.47	**-0.289**	-2.02
rpcim	0.411	1.62	0.000	0.00	-0.233	-0.55	**-0.243**	-1.76
rpcrpo	-0.083	-0.66	0.049	0.23	0.070	0.90	**0.151**	2.02
povrate	0.001	0.12	0.005	0.50	0.002	0.58	0.000	-0.03
popc	**0.001**	1.80	**-0.005**	-4.73	**-0.001**	-1.98	0.000	0.16
ppb	**4.720**	4.59	**-5.641**	-4.44	**3.072**	2.35	-0.295	-0.53
pp1019	**-4.533**	-2.38	**6.510**	2.69	2.724	0.64	**2.537**	1.73
pp2029	-3.057	-1.42	**6.462**	2.54	1.888	0.76	0.799	0.55
pp3039	**-6.247**	-2.11	0.732	0.26	-2.490	-0.78	1.530	0.88
pp4049	-3.980	-1.27	-0.306	-0.13	-0.438	-0.15	0.176	0.12
pp5064	**-6.219**	-3.36	4.055	1.60	-3.490	-1.47	-0.499	-0.39
pp65o	-3.795	-1.17	3.164	1.04	-2.780	-1.28	-0.551	-0.37
R-square	0.64		0.66		0.85		0.83	
N	58845		58818		58844		58830	
	Burglary		**Larceny**		**Auto**			
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio		
Y(t-1)	**0.402**	9.07	**0.386**	7.24	**0.293**	5.52		
crack	**0.041**	3.30	**0.034**	4.01	**0.070**	3.97		
prison_1	**-58.306**	-3.94	**-41.321**	-3.14	**-74.679**	-3.89		
aopro	**-0.001**	-5.15	**-0.001**	-3.59	**-0.001**	-3.41		
unemprt	**0.015**	2.64	0.006	1.31	-0.003	-0.34		
rpcpi	**-0.018**	-4.90	**-0.012**	-3.99	0.005	0.92		
rpcui	0.170	1.32	-0.015	-0.13	0.191	0.57		
rpcim	0.103	0.59	-0.119	-1.49	-0.201	-0.45		

8 If we restrict the cost-benefit analysis to significant coefficients only, the long run benefits exceed the short run costs in less than four years.

rpcrpo	0.107	1.24	0.104	1.65	0.140	1.54		
povrate	0.001	0.25	0.001	0.50	0.002	0.85		
popc	**-0.002**	-5.03	**-0.001**	-2.33	**-0.001**	-2.29		
ppb	-0.569	-1.59	-0.176	-0.69	**4.222**	2.78		
pp1019	**2.961**	2.40	1.583	1.18	2.992	0.88		
pp2029	**1.688**	1.98	1.556	1.89	3.009	1.40		
pp3039	0.926	0.78	1.183	1.14	-0.563	-0.19		
pp4049	-0.328	-0.26	0.799	0.87	0.029	0.01		
pp5064	0.709	0.76	0.812	1.05	-2.175	-1.24		
pp65o	-0.555	-0.38	-0.702	-0.83	**-4.720**	-2.89		
R-square	0.86		0.87		0.84			
N	61550		61550		61551			

Note: The dependent variable is the crime rate logged. Because of the 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 have suppressed the estimated coefficients on the year dummies and the individual state trends. County fixed-effects are absorbed.
Source: See note 7.

Table 3a: Shall-Issue Dummy Coefficients: Violent Crime

State	Murder		Rape		Robbery		Assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	-0.010	-0.11	**-0.586**	-6.41	0.039	0.51	**0.074**	1.97
AZ	**0.263**	5.80	-0.026	-0.34	**0.202**	5.34	**0.071**	2.76
AR	0.089	1.66	0.049	0.50	-0.062	-1.52	**0.102**	4.86
FL	-0.042	-0.57	**-0.197**	-3.04	**0.160**	1.89	**0.081**	3.14
GA	**-0.174**	-3.67	**-0.142**	-2.63	**-0.130**	-2.77	-0.036	-1.66
ID	**0.940**	20.02	**0.283**	2.30	**0.122**	1.71	0.040	1.48
KY	0.032	0.78	**-0.359**	-5.57	**0.280**	5.34	**0.152**	6.39
LA	**0.300**	5.68	0.068	1.28	**0.225**	4.43	**0.073**	2.65
ME	**0.380**	7.77	**0.168**	2.10	**-0.202**	-4.19	**-0.170**	-5.40
MS	**0.166**	3.41	-0.083	-1.34	**0.141**	3.39	**0.121**	4.43
MT	**-0.096**	-2.49	0.133	0.89	**-0.436**	-7.14	**-0.219**	-8.68
NV	**0.486**	13.66	**0.184**	2.46	**0.122**	3.80	**0.197**	6.86
NC	0.003	0.07	0.116	1.17	**0.082**	1.80	**0.107**	4.38
OK	**0.083**	2.03	0.068	1.34	-0.019	-0.48	0.019	0.82
OR	**-0.297**	-5.71	0.000	0.00	**-0.252**	-4.33	0.032	1.42
PA	**-0.104**	-2.32	0.042	0.70	**-0.101**	-2.48	**-0.072**	-3.09
PH	**0.210**	4.07	**-0.246**	-3.59	**0.176**	2.73	**-0.104**	-3.98
SC	0.033	0.78	-0.112	-1.44	-0.066	-1.39	**0.063**	2.65

TN	-0.046	-1.34	**-0.148**	-2.17	**-0.102**	-3.04	0.034	1.13
TX	-0.069	-1.16	0.113	0.52	0.035	0.58	0.015	0.49
UT	**0.141**	2.60	-0.099	-1.09	**0.136**	3.38	**0.228**	7.57
VA	0.032	0.60	0.058	1.19	-0.020	-0.43	-0.029	-1.41
WV	**0.242**	5.19	0.109	1.65	**-0.116**	-2.41	**-0.107**	-4.34
WY	**-0.249**	-3.61	-0.108	-1.07	**0.515**	7.18	**-0.070**	-2.08
US	-0.009	0.10	-0.020	0.14	0.008	0.07	**0.031**	4.92
Negative	9		12		11		8	
Significant	5		6		7		6	
Positive	15		12		13		16	
Significant	10		3		11		11	

Note: Coefficients in bold are significant at the .10 level, two-tailed. 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.
Source: See note 7.

Table 3b: Shall-Issue Dummy Coefficients: Property Crime

State	Burglary		Larceny		Auto	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	0.001	0.02	-0.031	-0.76	-0.044	-1.04
AZ	**0.090**	5.22	**0.082**	4.36	**0.200**	4.55
AR	**-0.049**	-3.06	0.022	1.39	-0.008	-0.25
FL	-0.004	-0.14	-0.022	-0.82	**0.162**	2.73
GA	**-0.111**	-4.75	**-0.068**	-4.52	**-0.135**	-4.03
ID	0.015	0.53	**0.095**	3.30	**0.136**	2.57
KY	-0.028	-1.27	**-0.086**	-4.25	**0.090**	1.86
LA	**0.045**	1.70	**0.056**	2.51	**0.196**	5.48
ME	-0.005	-0.22	**0.042**	2.49	0.019	0.39
MS	-0.036	-1.19	0.013	0.56	-0.046	-1.36
MT	**0.113**	3.63	0.025	0.84	0.027	0.50
NV	**0.172**	6.58	**0.106**	4.71	**0.147**	5.32
NC	-0.006	-0.32	**0.073**	4.11	**0.183**	5.61
OK	**0.043**	2.39	**0.060**	3.40	0.011	0.36
OR	**-0.100**	-3.04	0.006	0.23	-0.021	-0.41
PA	**-0.043**	-1.89	-0.001	-0.08	-0.035	-1.24
PH	-0.007	-0.34	-0.016	-0.97	0.082	1.25
SC	**-0.043**	-1.85	-0.010	-0.59	0.070	1.53
TN	-0.026	-1.43	-0.031	-1.47	-0.052	-1.58
TX	0.052	1.57	0.041	1.24	0.055	1.06

UT	**0.095**	2.98	-0.029	-1.30	**0.248**	6.67
VA	**-0.070**	-2.52	-0.004	-0.16	-0.056	-1.28
WV	0.026	0.93	**0.053**	2.78	**-0.134**	-2.80
WY	**0.115**	5.02	**0.050**	2.03	**0.189**	3.65
US	-0.012	0.64	0.011	0.87	**0.049**	5.18
Negative	13		10		9	
Significant	6		2		2	
Positive	11		14		15	
Significant	7		9		9	

Note: See notes to Table 3a.
Source: See note 7.

The fact remains that the hybrid model estimates trends and eventually the trends will dominate, so if the coefficients on the trends tend to be negative and the coefficients on the dummy positive, the laws will eventually be beneficial. Since Ayres and Donohue suggested this model and derived policy implications from it, they are presumably obligated to accept its implications.

Earlier, Ayres and Donohue (2003a, 114, 147-148, 153) complained at length that Lott and Mustard did not control for the crack epidemic.[9] That is a weak criticism, because one can usually come up with at least one potentially important missing variable in any study, and they themselves never tried to control for crack. We did try but they did not appreciate the effort. We used Fryer et al's (2005) measure of crack. Ayres and Donohue (2009, 55) object because it is a state-wide measure and crack does not affect counties evenly. That is true, but they offer no better measure; the perfect should not be the enemy of the good. Also, crack is concentrated in cities, as is crime, and it is positive and highly significant (after correcting for clustering) in all seven regressions. Finally, Ayres and Donohue use state-wide variables – notably the prison population and the shall-issue laws - in their analyses.

9 Even though Lott and Mustard (1997) did control for crack by including the price of cocaine in some of their regressions, the results did not change.

Table 4: Short Run Costs and Benefits, Millions of 2000 Dollars[10]

State	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	-0.89	-15.52	0.30	1.78	0.01	-0.24	-0.52	-15.09
AZ	381.36	-3.68	12.27	14.90	8.66	6.62	37.23	457.35
AR	79.43	4.61	-1.82	10.12	-2.00	0.65	-0.30	90.70
FL	-199.91	-121.23	64.38	62.10	-1.97	-4.17	57.06	-143.74
GA	-482.36	-44.54	-20.95	-9.76	-19.74	-7.43	-24.16	-608.95
ID	88.59	7.87	0.17	0.98	0.21	1.15	0.97	99.94
KY	10.61	-16.64	8.19	9.60	-0.60	-1.30	2.34	12.20
LA	768.30	11.97	24.69	19.08	4.00	4.06	22.75	854.85
ME	36.91	2.86	-0.54	-2.64	-0.09	0.51	0.16	37.17
MS	107.34	-6.04	2.29	4.43	-1.35	0.23	-0.81	106.09
MT	-4.43	1.53	-0.41	-1.27	0.56	0.17	0.14	-3.71
NV	270.95	17.34	5.65	17.20	5.65	2.47	7.23	326.50
NC	6.58	27.17	9.87	34.07	-0.91	7.31	17.57	101.65
OK	113.93	10.04	-0.66	3.25	2.92	2.70	0.74	132.93
OR	-110.41	-0.05	-9.67	2.96	-5.25	0.24	-1.21	-123.39
PA	-99.36	9.22	-7.33	-13.31	-4.47	-0.08	-4.73	-120.05
PH	303.71	-16.13	22.41	-7.69	-0.18	-0.32	8.45	310.24
SC	38.36	-21.43	-3.98	18.92	-3.37	-0.56	4.82	32.76
TN	-69.72	-35.08	-9.96	8.05	-2.20	-1.69	-6.07	-116.67
TX	-403.76	98.34	10.95	12.87	17.33	11.35	24.91	-228.00
UT	37.06	-8.27	1.64	9.79	2.42	-1.10	8.03	49.58
VA	49.51	9.14	-1.19	-2.68	-5.35	-0.26	-4.23	44.94
WV	101.20	3.85	-0.86	-1.65	0.50	0.61	-1.69	101.96
WY	-14.67	-1.76	0.38	-0.77	0.58	0.33	0.63	-15.28
US	**1008.34**	**-86.42**	**105.82**	**190.34**	-4.65	**21.27**	**149.30**	**1384.00**

Source: See note 7.

The Influence of Florida

Ayres and Donohue's main criticism, at least in terms of number of pages (42-47), is that without Florida there is little evidence that shall-issue laws have an effect. However, lots of people live in Florida (16 million in 2000), so the state has extra weight in the cost-benefit analysis. Also, it is not legitimate to simply drop an inconvenient outlier. The researcher should at least also drop the equivalent outlier

10 Costs are positive (crime rates increase) and benefits are negative (crime rates decrease). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia.

on the other side, which is Tennessee. For crime rates, the (positive) coefficients for Tennessee are larger than the (mostly negative) coefficients for Florida (Moody and

Table 5a: Shall-Issue Post-Law Trend Coefficients: Violent Crime

State	Murder		Rape		Robbery		Assault	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	0.032	1.58	-0.027	-0.46	**-0.064**	-3.97	**-0.028**	-2.50
AZ	**-0.047**	-2.76	0.007	0.25	-0.002	-0.13	**-0.015**	-1.80
AR	**-0.147**	-10.57	**-0.078**	-3.78	0.006	0.23	**0.052**	6.29
FL	**-0.064**	-3.66	**0.052**	3.11	**-0.089**	-6.51	0.001	0.20
GA	0.011	1.17	**-0.040**	-3.72	**-0.034**	-3.45	-0.008	-1.60
ID	**-0.026**	-2.61	0.000	0.00	**0.089**	4.23	**0.035**	4.90
KY	-0.007	-0.40	**-0.088**	-4.27	**-0.039**	-1.83	**-0.071**	-6.02
LA	-0.006	-0.32	0.054	1.41	0.033	1.61	0.007	0.65
ME	**0.043**	4.22	-0.014	-0.68	0.005	0.34	**0.016**	2.96
MS	**0.033**	2.82	**0.065**	2.91	**0.074**	4.20	**0.070**	10.86
MT	**0.056**	5.99	**0.049**	2.11	**0.157**	15.33	**0.210**	29.10
NV	**-0.077**	-6.55	**-0.067**	-2.17	-0.022	-1.38	**-0.072**	-6.73
NC	0.004	0.30	**-0.068**	-1.94	-0.026	-1.33	**-0.021**	-2.26
OK	0.010	0.80	-0.029	-1.56	-0.001	-0.05	-0.006	-0.60
OR	**-0.045**	-2.86	-0.038	-1.27	-0.018	-1.20	**0.053**	7.00
PA	**0.024**	3.40	-0.010	-0.87	**0.046**	4.88	**0.017**	3.06
PH	**-0.057**	-3.99	**0.143**	6.91	**-0.031**	-2.05	**0.130**	13.07
SC	-0.003	-0.16	**-0.069**	-2.28	-0.019	-0.87	**-0.033**	-3.01
TN	**0.114**	9.09	**0.107**	6.25	**0.115**	6.31	**0.078**	9.29
TX	-0.011	-0.86	-0.031	-1.44	-0.015	-0.82	**-0.016**	-1.71
UT	**-0.054**	-3.61	-0.014	-0.44	0.013	0.33	-0.002	-0.18
VA	0.000	0.01	0.009	0.85	**0.034**	3.06	**0.025**	4.72
WV	**-0.091**	-9.21	**-0.058**	-3.49	0.017	0.93	**0.110**	12.60
WY	**0.153**	13.16	-0.014	-0.63	**0.044**	1.87	**0.069**	9.16
US	-0.013	1.98	-0.009	0.34	-0.004	0.06	**0.013**	3.89
Negative	13		15		12		10	
Significant	9		7		5		7	
Positive	11		9		12		14	
Significant	6		5		7		12	

Note: See notes to Table 3a.
Source: See note 7.

Table 5b: Shall-Issue Post-Law Trend Coefficients: Property Crime

State	Burglary		Larceny		Auto	
	Coeff	T-ratio	Coeff	T-ratio	Coeff	T-ratio
AK	**-0.032**	-2.14	**-0.027**	-2.35	**-0.030**	-2.20
AZ	-0.007	-0.76	**-0.027**	-3.93	**-0.057**	-3.61
AR	-0.005	-0.64	-0.001	-0.14	-0.006	-0.33
FL	**-0.021**	-2.56	0.003	0.43	-0.013	-0.89
GA	**-0.013**	-1.94	-0.004	-0.62	0.007	0.80
ID	0.012	1.56	**-0.012**	-1.69	0.030	1.61
KY	-0.009	-0.82	0.005	0.57	0.000	0.02
LA	**0.047**	4.73	**0.031**	3.84	**0.055**	2.41
ME	0.003	0.66	0.005	0.92	0.017	1.25
MS	**0.057**	7.22	**0.066**	8.82	**0.089**	6.25
MT	0.009	1.58	**0.027**	6.33	**0.038**	4.35
NV	**-0.026**	-3.15	**-0.045**	-6.56	-0.006	-0.44
NC	-0.013	-1.35	**-0.013**	-1.75	0.001	0.08
OK	**-0.016**	-1.76	-0.013	-1.64	-0.001	-0.08
OR	-0.002	-0.24	**0.012**	1.89	-0.003	-0.20
PA	**0.012**	2.89	**0.022**	5.79	0.011	1.37
PH	-0.002	-0.33	**0.039**	7.57	**-0.059**	-5.33
SC	**-0.023**	-1.82	-0.011	-1.09	0.007	0.38
TN	**0.060**	7.10	**0.067**	8.26	**0.083**	5.12
TX	**0.016**	1.78	0.002	0.31	-0.005	-0.42
UT	-0.005	-0.37	-0.015	-1.16	-0.012	-0.36
VA	0.000	0.07	0.002	0.36	0.001	0.09
WV	0.014	1.55	0.009	1.01	**0.038**	2.75
WY	-0.013	-1.45	-0.009	-1.19	0.027	1.47
US	0.003	0.24	0.008	1.87	0.006	0.24
Negative	14		11		10	
Significant	6		5		3	
Positive	10		13		14	
Significant	5		7		5	

Note: See notes to Table 3a.
Source: See note 7.

Marvell 2008, 282-286). In the cost-benefit analysis of the long term impact, the savings for all 24 shall-issue states ($450 million per year) is not much more than the $402 million without Florida and Tennessee (287). These results are similar to the updated results in Table 6 below ($466 million per year without Florida and Tennessee). Ayres

and Donohue refer to the cumulative effect (Moody and Marvell 2008, 289 Table 9), where Florida dominates due to the large number of post-law years. When comparing states, the average yearly effect is the only meaningful measure. The only purpose of our previous Table 9 is to estimate overall USA cost and benefits during the study period.

As Ayres and Donohue note, Florida has data problems, but their presentation of the problems is misleading. In their Figure 1 they compare aggregated county level homicide data to the FBI estimate of total state data, showing that the latter drops less after 1975. These curves are not comparable. The reason for the difference is that Dade County data are missing during that period, and the FBI data include an estimate of homicides for Dade. In the county data set, Dade homicide is simply scored as missing. Ayres and Donohue's diatribe on page 46 and 47 is based on the assumption that Florida should be dropped; again, it is not correct to drop inconvenient observations in the data set. It is not enough to argue that Florida's data has problems; they must show that the problems are worse than in other states, which they fail to do.

Ayres and Donohue (2009, 45) quote a passage from a draft version of Kovandzic and Marvell (2003) that differs from the final version. These authors obviously considered the draft language wrong; consequently the material in Ayres and Donohue's quote is wrong. Kovandzic and Marvell found no evidence of a relationship between crime rates and the rate of carry permits. But the results of the two studies are not directly comparable. The Ayres and Donohue hybrid model compares crime across states with and without the shall-issue law. It is quite possible that Florida's law reduced crime, compared to states that did not pass a shall-issue law, even if the marginal impact of a new concealed carry permit did not have a significant effect within Florida.

Table 6: Long Run Costs and Benefits (Post-Law Trends), Millions of 2000 Dollars

State	Murder	Rape	Robbery	Assault	Burglary	Larceny	Auto	Total
AK	2.96	-0.72	-0.50	-0.69	-0.17	-0.21	-0.36	0.32
AZ	-67.85	0.98	-0.14	-3.05	-0.63	-2.18	-10.52	-83.39
AR	-130.79	-7.36	0.16	5.13	-0.21	-0.03	-0.21	-133.31
FL	-302.35	32.10	-35.69	0.92	-9.31	0.54	-4.69	-318.47
GA	31.55	-12.41	-5.47	-2.14	-2.31	-0.40	1.26	10.09
ID	-2.45	0.00	0.12	0.86	0.16	-0.15	0.21	-1.24
KY	-2.24	-4.09	-1.14	-4.48	-0.20	0.08	0.01	-12.06
LA	-15.25	9.49	3.66	1.83	4.15	2.20	6.42	12.50
ME	4.18	-0.25	0.01	0.25	0.06	0.07	0.14	4.47
MS	21.11	4.69	1.19	2.57	2.10	1.15	1.59	34.39
MT	2.57	0.57	0.15	1.21	0.04	0.18	0.19	4.92
NV	-43.13	-6.37	-1.03	-6.34	-0.85	-1.04	-0.29	-59.05
NC	9.78	-16.02	-3.09	-6.69	-2.13	-1.32	0.12	-19.34
OK	14.23	-4.28	-0.04	-0.96	-1.09	-0.57	-0.09	7.21
OR	-16.78	-5.19	-0.69	4.93	-0.11	0.52	-0.16	-17.47

PA	22.94	-2.24	3.34	3.06	1.23	1.58	1.55	31.45
PH	-81.63	9.35	-3.98	9.64	-0.06	0.79	-6.13	-72.03
SC	-3.51	-13.17	-1.15	-9.82	-1.80	-0.65	0.48	-29.63
TN	171.76	25.30	11.14	18.52	5.15	3.62	9.73	245.22
TX	-64.52	-26.72	-4.67	-13.26	5.17	0.58	-2.44	-105.87
UT	-14.25	-1.14	0.16	-0.07	-0.13	-0.58	-0.38	-16.39
VA	0.12	1.43	2.05	2.28	0.03	0.12	0.07	6.10
WV	-38.12	-2.07	0.12	1.69	0.27	0.10	0.47	-37.53
WY	9.00	-0.24	0.03	0.75	-0.07	-0.06	0.09	9.52
US	**-492.66**	**-18.34**	**-35.45**	6.13	**-0.70**	4.35	**-2.94**	**-539.60**

Note: Costs are positive (crime rates increase) and benefits are negative (crime rates decrease). Bold indicates that the sum across states is significantly different from zero at the .10 level. PH is Philadelphia.

Source: See note 7.

Ayres and Donohue's New Aggregate Analyses

Ayres and Donohue (2009, 48-50) present charts showing trends for states without shall-issue laws, states with early laws and states with later laws. This methodology is obviously weak, since it makes no attempt to control for the effects of relevant control variables.

Ayres and Donohue (2009, 50-55 Table 1) present a regression using aggregated shall-issue law variables, even though in their previous articles they persistently recommended separate shall-issue law variables for each state. For example,

> "It [aggregate analysis] simply overlooks …the entire thrust of our paper: that aggregated specifications of the effects of these laws are badly marred by jurisdiction selection effects … *[C]laims based on these aggregated estimates are inaccurate and misleading*…. The data at every turn reject the idea that concealed-carry laws passed in different jurisdictions have a uniform impact on crime. Therefore the results of disaggregated regressions must … be taken as a more authoritative assessment of the overall impact of concealed-carry laws" (Ayres and Donohue 2003b, 1372-3, italics supplied).

Nevertheless, in their latest analysis they attempt to measure the effect of the shall-issue laws across all adopting states with a single dummy and a single trend and claim to find increases in crime (Ayres and Donohue 2009, 56). These claims rest on the finding of no significant effect of the law, except for a small positive effect on assault. (Ayres and Donohue 2009, 51 Table 1). They also find some positive effects on property crime when they drop the state trends (53 Table 2), but this is not legitimate since the state trends are highly significant.[11] Finally, the data thoroughly

11 Ayres and Donohue repeatedly argue that county level data has quality problems. Nevertheless, Ayres

reject their aggregate specification. Simple F-tests of the equality of the coefficients on the dummy variables as well as the same tests on the post-law trends across states are rejected for all seven crimes. The smallest F-ratio was over 300 and the probability-values were all less than 0.0000.

Conclusion

Ayres and Donohue (2003a) estimated a model using a dummy variable and a post-law trend on county data and claimed to find that shall-issue laws increased crime. However, a close reading of their article revealed that this claim depended on their truncating the trends at five years, one year before their estimates would have shown a decrease in crime. If they had merely included in their paper something like the following statement, we would not be having this debate:

> In calculating the costs and benefits of the shall-issue laws we extrapolate the post-law trends for five years. However, when we extrapolate the trends for six or more years, we find that the conclusions are reversed.

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and Donohue (2003a) considered their county-level analysis to be their key analysis. Ayres and Donohue (2003b) did use the 1977-2000 dataset but not with shall issue laws disaggregated by state, which they consider the best procedure and which we used.

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