Lott and Mustard Revisited

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1 Introduction

The question of concealed carry permits and their effect on crimes has long been an extremely controversial topic. Lott and Mustard (1997) is an extremely important analysis of these permits but, since it's original publishing in 1997 research design has become far more adept at determining causal factors.

By re-analyzing this data with newer, more thorough methods, we can investigate more closely the findings of the original paper and determine what, if any, effect these laws have on crime.

2 Background and Theory

Crime deterrence as a whole is a more-than controversial subject in the modern day; the concept of being "tough on crime" is commonplace in modern media and politics and comes in many forms, from increasing police presence and prison sentences to denying civil liberties and services in perpetuity. The goal is to drive the cost of crime so high that it becomes rare that a rational person would *ever* choose to engage in illicit activity. "Shall-issue" laws are another step in this direction; these are state-level laws that ensure any person who is legal and meets some basic eligibility requirements be issued a permit for carrying a concealed weapon.

The theory here is quite simple: more armed victims *should* mean a higher expected cost of committing a crime and, subsequently, a lower number of crimes committed. This carries the obvious restrictions that we would only expect this change in crimes where the victim is aware and present at the time the crime is committed, such as assault and robbery, and smaller or no changes when the victim is not present or aware, while the crime is being committed, such as auto theft.

Table 1: Shall-Issue Law Rollouts by State

State	Rollout
Alabama	Pre-1977
Connecticut	Pre-1977
Indiana	Pre-1977
New Hampshire	Pre-1977
North Dakota	Pre-1977
South Dakota	Pre-1977
Vermont	Pre-1977
Washington	Pre-1977
Florida	1987
Virginia	1988
Georgia	1989
Maine	1989
Oregon	1989
Pennsylvania	1989
West Virginia	1989
Idaho	1990
Mississipi	1990
Montana	1991

Shall-issue laws were a hot-bed political topic starting in 1976 and, from 1987 to 1991 [1] there were several of these laws passed. Prior to this wave of laws (and the start of our data) there were eight states that already had these laws in effect. These laws were very similar to one another in that they funcitonally removed the ability for any lower-than-state level official to restrict the issuance of a gun permit. There is some heterogeneity among the effect of these laws within each state, since some officials were more restrictive than others, but across each state this resulted in greater access to concealed carry permits issuance.

We will use several models to determine the effect of these laws on crimes; our goal here is to determine the average treatment effect on the treated groups. The model most similar to the approach of Lott and Mustard (1997) [5] is the two-way fixed effect model but this estimator is known to have some unfavorable properties. To handle this, we will also investigate the Bacon decomposition [4] for the two-way fixed effect model. Implement the Callaway and Sant'anna estimator [1] and the Sun and Abraham event study to get a more contemporary idea of the effects.

3 Data

Here we are working with the state level data from the National Research Council's review of firearms and gun violence, provided by Peter Donohue [3]. This data covers the yearly crimes for violent crimes

[1] The years for each law come from Lott and Mustard (1997) [5] and Cramer and Kopel (1995) [2]. The only discrepancy between the two sources is that of Oregon, which Lott and Mustard list as 1990 whereas Cramer and Kopel cite the law as 1989. This may be due to the timing of the law, later analysis does not seem consequentially sensitive to this shift in any case.

and property crimes in all fifty states from 1977 to 2006. The violent crimes subdivided into murder, rape, assault and the property crimes are subdivided into robbery, auto theft, burglary, and larceny.

Table 2: Summary Statistics for Statewide Yearly Crimes and Crime Rates

Variable	N. Obs	Mean	Std. Dev
Crime Counts			
Violent Crimes	1941	27066.81	41920.33
Property Crimes	1941	211212.22	262156.25
Murder	1941	382.63	529.34
Rape	1941	1631.25	2054.90
Assault	1941	15421.66	23517.30
Robbery	1941	9631.27	17207.33
Auto Theft	1941	23714.54	37503.03
Burglary	1941	54414.24	72421.71
Larceny	1941	133083.69	157018.36
Crime Rates			
Violent Crime Rate	1941	458.85	309.28
Property Crime Rate	1941	4168.46	1256.21
Murder Rate	1941	7.25	6.80
Rape Rate	1941	32.33	14.52
Assault Rate	1941	270.80	167.23
Robbery Rate	1941	148.48	160.60
Auto Theft Rate	1941	404.60	234.87
Burglary Rate	1941	1046.53	429.72
Larceny Rate	1941	2717.34	788.57

4 Empirical Models

We need to visit two separate models, the first is a standard two-way fixed effect model as was carried out in Lott and Mustard (2017). For our purposes, we will still be using the state level data, and we will specifically look at the Bacon decomposition to consider the potential innaccuracies of this model.

Following that, we will look at the Callaway Sant'anna estimation, which will avoid the unfavorable properties of the two-way fixed effect model, and look at the effects in the form of an event study.

4.1 Two-Way Fixed Effects

This model is estimated by regressing the natural log of crime rates on the treatment, as well as the arrest rate for that specific crime. This provides an estimation of change in crime in terms of percentages, controlling for the current level of enforcement. We also include a wide variety of controls for demographic shifts over time.

Dependent Variables:	Violent	Murder	Rape	Assault
Model:	(1)	(2)	(3)	(4)
Variables				
Treatment	-0.0505	-0.0280	-0.0878***	-0.0403
	(0.0367)	(0.0341)	(0.0308)	(0.0508)
Fixed-effects				
year	Yes	Yes	Yes	Yes
fipsstat	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,481	1,436	1,429	1,439
R^2	0.95783	0.93192	0.88324	0.92972
Within R^2	0.34949	0.23081	0.50501	0.26237

Clustered (year & fipsstat) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

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Dependent Variables:	Property	Robbery	Auto	Burglary	Larceny
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Treatment	0.0205	-0.0490*	0.0464	-0.0277	0.0281
	(0.0159)	(0.0274)	(0.0435)	(0.0205)	(0.0169)
Fixed-effects					
year	Yes	Yes	Yes	Yes	Yes
fipsstat	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	1,489	1,436	1,438	1,439	1,439
R^2	0.93649	0.97382	0.91642	0.94993	0.92891
Within R^2	0.59052	0.42838	0.52291	0.56501	0.57558

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In this estimation, we find that there is a small but statistically insignificant reduction in violent crime. The only specific crime that has a significant reduction is rape, which drops by a surprising 8.8%.

On the flip side, we see a slight but insignificant increase in property crimes, with a somewhat-significant increase in robbery specifically. This is not inconsistent with, but is far less decisive than, the results found by Lott and Mustard, where there is a reduction in violent crime and a smaller increase in property crime.

4.2 Bacon Decomposition

When estimating the weights and average estimates of the treatment variables, there are some obvious issues that arise. The Earlier vs. Later treated group has an outsized weight compared to the other

groups, while the Later vs. Earlier treated group is much smaller.

Table 3: Bacon Decomposition

Type	Weight	Average Est
Earlier vs Later Treated	0.2742302	0.0021544
Later vs Always Treated	0.2351916	0.0574595
Later vs Earlier Treated	0.0873926	0.0446130
Treated vs Untreated	0.4031856	0.1965715

Principally, these group weightings can cause our estimates to be inconsistent to the point of uselessness, especially in situations where there are already-strong trends year over year, and comparing a treated group to a treated group in this case can cause wildly inconsistent estimations.

4.3 Callaway and Sant'Anna

For a more accurate estimation in which we never compare a treated group to another treated group, we can turn to Callaway and Sant'Anna [1]. For this estimation, we will look at the treatment for all states in a single year as a collective and look at the treatment effect for those states. We will narrow our focus to violent crimes for now, since these are the crimes where we will find conclusive results, if there are any.

Table 4: Aggregate Group ATTs

Group	Aggregate ATT	SE
1987	-0.13	0.08
1988	0.01	0.08
1989	-0.03	0.31
1990	0.03	0.09
1991	0.48	0.09

Here, the estimation is *far* different; the average treatment effect on the treated group is barely negative, and only significantly so for the very first group of treated states. In no case do we have conclusive evidence that these crimes are being influenced much at all.

4.4 Event Study

Considering this data in the form of an event study, where we aggregate the effects by the years since the treatment for each group; negative values are the pre-treatment years, in which we can see both the trends and potentially anticipation. Our pre-treatment 'trend difference' provides some intuition of the potential accuracy we could attain from our data set. The post-event estimations fail to show any conclusive evidence that there are effects on the violent crime rate from these laws. We also see extremely wide standard errors and a noisy estimation in general, both of which indicate that there is

Table 5: Event Study

			95% Confidence Interval	
Event Time	Coefficient	SE	Lower	Upper
-5	-0.0481866	0.1873409	-0.4153679	0.3189948
-4	-0.0075730	0.1867580	-0.3736120	0.3584661
-3	-0.0525203	0.2112834	-0.4666282	0.3615875
-2	-0.0012044	0.2371528	-0.4660153	0.4636066
-1	0.0250265	0.2434649	-0.4521559	0.5022090
0	-0.0380029	0.2470897	-0.5222897	0.4462840
1	-0.0388197	0.2408278	-0.5108336	0.4331942
2	-0.0634309	0.2342575	-0.5225671	0.3957053
3	-0.0639702	0.2248330	-0.5046348	0.3766944
4	-0.0963113	0.2402981	-0.5672869	0.3746644
5	-0.0619916	0.2230570	-0.4991752	0.3751921
6	-0.0900063	0.2353931	-0.5513684	0.3713557
7	-0.0069768	0.2452190	-0.4875971	0.4736436
8	0.0214361	0.2370008	-0.4430769	0.4859491
9	0.0922565	0.2267394	-0.3521445	0.5366575
10	0.1634692	0.2114618	-0.2509882	0.5779267

very little to be pulled from this data set beyond simply failing to reject the null hypothesis that more guns means nothing for crime.

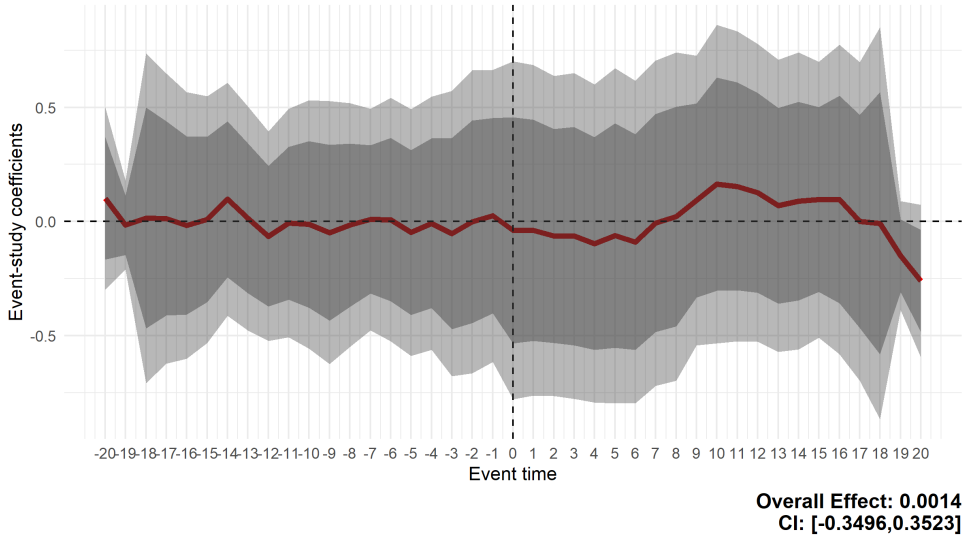

Figure 1: Event Study of Shall Issue law passage

Full Callaway Sant'Anna Results

The analysis of the violent crime trends continues to hold when we observe crimes for which we have data.

Table 6: Aggregate Group ATTs, Violent Crimes

	Violent		Murder		Rape		Assault	
Group	ATT	SE	ATT	SE	ATT	SE	ATT	SE
1987	-0.13	0.08	-0.33	0.04	-0.17	0.04	-0.05	0.03
1988	0.01	0.08	0.01	0.04	-0.02	0.04	0.02	0.03
1989	-0.03	0.31	-0.21	0.11	-0.06	0.10	0.02	0.19
1990	0.03	0.09	0.15	0.03	0.32	0.04	-0.04	0.03
1991	0.48	0.09	-0.36	0.04	0.20	0.03	0.55	0.03

You can still see very clearly that there are no substantial effects beyond the very slightest effect in the first year estimated. Even those effects are only minimally significant.

5 Conclusion

From this we can handily conclude that the effect of shall issue laws on crime are not statistically significant or substantial. This is a partial refutation of the conclusions in Lott and Mustard (2017), though Lott and Mustard use the slightly-different county level data.

Table 7: Aggregate Group ATTs, Property Crimes

	Property		Robery		Auto		Burglary		Larceny	
Group	ATT	SE	ATT	SE	ATT	SE	ATT	SE	ATT	SE
1987	-0.10	0.03	-0.28	0.04	0.00	0.05	-0.12	0.03	-0.09	0.02
1988	0.02	0.02	0.02	0.04	-0.02	0.05	-0.07	0.03	0.04	0.02
1989	0.02	0.06	-0.14	0.14	-0.17	0.10	-0.03	0.09	0.06	0.06
1990	0.00	0.02	0.25	0.04	0.19	0.06	-0.03	0.02	-0.01	0.02
1991	0.01	0.02	0.28	0.04	0.03	0.06	-0.11	0.03	0.01	0.02

There is one factor in this analysis that is a complete mismatch with the original data: the within state heterogeneity in the impact of these laws is unaccounted for in this state level data. This may be non-trivial but, since these laws are passed at the state level, the aggregate effect of these laws should be evaluated at the state level.

Regardless, given the slight differences between the state-level and county-level analysis and the similarity between the state- and county-level Bacon Decomposition, it is likely that the bias implicit in a two way fixed effect model is also similiar.

References

[1] Brantly Callaway and Pedro H.C. Sant'Anna. Difference-in-differences with multiple time periods. *Journal of Econometrics*, 225(2):200–230, 2021. Themed Issue: Treatment Effect 1.

[2] David B. Cramer, Clayton E. Kopel. Shall issue: The new wave of concealed handgun permit laws. *Tennessee Law Review*, 62:679, 1994-1995.

[3] John Donohue. The impact of right-to-carry laws and the nrc report: Lessons for the empirical evaluation of law and policy.

[4] Andrew Goodman-Bacon. Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, 225(2):254–277, 2021. Themed Issue: Treatment Effect 1.

[5] John R. Lott Jr. and David B. Mustard. Crime, deterrence, and right-to-carry concealed handguns. *The Journal of Legal Studies*, 26(1):1–68, 1997.