INDISSOLUBLE BONDS BETWEEN CONCEALED WEAPON LAWS AND VIOLENT CRIME

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ABSTRACT

The issue of whether allowing qualified citizens to carry concealed handguns reduced crime has been first brought to academic debate by John Lott and David Mustard in 1997. The so-called shall-issue laws permit bringing concealed weapons to adults without documented transcripts of criminality or psychogenic disorders. This paper examines the relationship between the adoption of shall-issue laws and violent crime. Using panel data for U.S. states from 1977 to 1999, I construct a dummy variable regression with fixed effects and demonstrate that the adoption of the laws is not significantly associated with the violent crime rate. There is suggestive evidence that the result reveals issues of omitted variable bias and model misspecification, which are probable sources of estimation inconsistency.

Introduction

Guns have always been a sign of criminal violence and a major tool of homicide. The U.S. has the highest rate of per capita gun-related deaths (Cook& Ludwig, 2000) and the gun homicide rate is 25 times higher than other developed countries (Grinshteyn& Hemenway, 2016). Common sense leads us to have the 'more guns, more crime' association, but what if we ask this question reversely: will the relaxation of permitting concealed weapons reduce the incidents of crime instead? Getting a sensible grasp of this effect can help make wiser policy decisions and possibly reduce the mortality rate. In this paper, we narrow down the scope of interest and mainly focus on the effect of concealed-carry laws on violent crime.

To do so, we use a state-wise panel dataset spanning 1977 to 1999 and implement a dummy variable linear fixed effects regression. However, we find that there is no statistically significant evidence to induce notable associations between them. Based on this interesting result, we further assess the underlying problems of endogeneity and misspecification as critiques of our analysis.

Literature Review

Lott and Mustard made a counter-intuitive argument that state laws enabling citizens to carry concealed handguns had reduced crime. Hidden weapons supply a sense of protection and courage and provide adequate defense for potential victims, thereby hindering criminals from attacking (Lott& Mustard 1997). Supportive statements reason that, by the nature of criminality, lawbreakers will be armed to teeth regardless of regulations of laws, so the legalization of carrying concealed weapons for law-abiding citizens leads offenders to take higher risks on committing crimes, thereby deterring such misdoing behavior (Kleck& Gary 1991).

There is also evidence against the 'more guns, less crime' argument. As Donohue mentioned, if more weapons are circulating in the society, no matter in the hand of consumers (buyers with validated permits) or suppliers, burglars and thieves can readily take weapons away and then quickly change hands to potential criminals (Donohue 2003). Besides, Duggan

estimated that in the U.S., around 500,000 guns are stolen every year (Duggan 2001). Legal guns likely help expand the arsenal of criminals instead.

In line with the FBI crime reports, crime can be classified into two summary categories, violent and property crimes. This broad division brings a hurdle to the determination of which specific types of crime can be dampened by the larger population with concealed weapon ownership. In general, cases involving direct face-to-face contact between victims and offenders are possibly discouraged since victims may lose the chance to bring out the concealed weapons as deterrence (Lott& Mustard 1996).

Data Description

The data consists of 3 types of crime rates (violence, robbery and murders), demographic characteristics that possibly determine the crime rate and incarceration rates in all states of all years between 1977 and 1999 as a proxy for deterrence effects. A dummy variable is used to indicate whether the shall-issue laws were enacted. Note that murder and robbery are both included in the category of violent crime, according to the Bureau of Justice Statistics.

Table 1 gives a summary of descriptive statistics. There are no missing values in all variables, so the data is well balanced. By having a glimpse at the second row, the violent crime rate has a wide range across its minimum and maximum and its spread is higher than any other type of crime. Moreover, the distribution of it is highly skewed. The reported skewness is two times higher than the common rule of thumb cutoff ±1.

Table 1.b shows the summary of all states in all years where the shall-issue laws were enacted, whereas Table 1.c outlines key descriptive statistics of states in all years failing to establish the laws. One direct observation is that states with established shall-issue laws have lower average violent crime rates than those without passage of the laws, which corresponds to what Lott and Mustard suggested in their initial paper (Lott& Mustard 1997).

However, these two overall summary tables ignore the panel structure of the data. Not all states are consistent in terms of shall-issue law enactment during the entire 22 years. 17 states did not adopt the laws until 1989 and 8 states only enacted the laws between 1977 and 1990(Ian& Donohue 2002). It is less likely to distinguish the partial effect of the shall-issue

laws when the legitimacy process does not occur indiscriminately across states and years like a randomized experiment (Donohue 2003).

Econometric Model and Testing Hypothesis Specification

Motivated by the previous discussion of literature, the passage of shall-issue laws has an impact on the crime rate. More specifically, if we consider the 'violence – property' subdivision of crime types mentioned previously, we are more interested in testing whether the average violent-specific crime rate is decreased due to the enactment of the laws in a statistically and economically significant way.

Thus, we employ a dummy variable regression model for our panel data set with both state and year fixed effects. Our base model specification in equation form is

$$\ln(vio)_{it} = \beta_0 + \beta_1 shall_{it} + \beta_2 incare\_rate_{it} + \beta_3 density_{it} + \beta_4 avginc_{it} +$$
$$\beta_5 pb1064_{it} + \beta_6 pw1064_{it} + \beta_7 pm1029_{it} + a_i + \theta_t + u_{it}$$

where i indexes the ID of states, t indexes years from 1977 to 1999, vio is violent crime rate (incidents per 100,000), $shall$ is a dummy variable indicating the enactment of shall-issue laws (our variable of interest), a_i is the unobserved state fixed effect, θ_t is the unobserved time fixed effect, u_{it} is the idiosyncratic error and all other predictors are control variables (population, age, race, incarceration rates, etc.).

We apply log transformation on the response variable vio to reduce the skewness of the data. The natural log of the violent crime rate can be interpreted as the percentage change in violent crime that is correlated with the passage of shall-issue laws.

Our goal is to test the ceteris paribus effect of the laws on violent crime, so our null hypothesis is $H_0: \beta_1 = 0$ against $H1: \beta_1 \neq 0$. Since there is only one coefficient we need to test, an individual t-test is sufficient to meet the needs. Since we have both time and state fixed effects, one can get an unbiased and consistent estimation of β_1 by the following mechanics: state demeaning and using T-1 time binary indicators altogether (T=22 in our case), then estimate β_1 by pooled OLS. After that, calculate the t-statistics as well as the corresponding p-value, compare the p-value with the preset significance level and make the final rejection decision.

Model Estimation and Discussion

The results of the regression analysis for the base specification are captured in Table 2. All coefficients on the dummy variable 'shall' are negative but only statistically significant when the fixed effects are not induced. We fail to reject the null hypothesis in the specification (3) and (4) at any significance level, so the passage of shall-issue laws does not lead to varying violent crime rates.

Practically speaking, however, the magnitudes of two significant pooled OLS estimates are simply too large to believe. It is unrealistic to claim the passage of shall-issue laws drops in violent crime of nearly 44 or 37 percent on average. Thus, both pooled OLS estimates are not economically significant due to the counterfactual large size regardless of their high statistical significance at the 0.01 level. Even though adding a set of control variables notably explains more variability of violent crime rates from the regression model and decompose the influence on violent crime that is not purely due to the law's adoption, we still cannot draw any credible conclusion without describing unobserved effects of time.

As we introduce the state effects and consider specification (3), the size of the estimated coefficient of shall shrinks to 5 percent, which falls in a practically believable range, notwithstanding its statistical insignificance. In terms of time-invariant omitted variables, I suggest that metropolitan population rate, law enforcement employment ratio and average per capita alcohol consumption can all be deemed as examples that vary across states and are associated with shall-issue laws, practically speaking. The estimation is questioned to be consistent due to the omitted variable bias when we further assume that all such bias results from state effects.

After adding time effects, one can surprisingly notice that all individual t-tests are not significant at any level, whereas the global F-test is highly significant at the 0.01 level. This is an indication of multicollinearity where we have highly correlated regressors.

We can visualize the time and state effects by the scatterplot and mean bar graph of the natural log of violent crime rates given in the appendix. From the scatterplot, more states started to enact shall-issue laws after year 1990, but that is exactly when the violent crime rate rocketed up according to the bar graph. Nevertheless, one can also observe a graduate drop in mean

crime rates between 1995 and 1999. This up-and-down current may be attributed to nation-wise time effects like the Lyndon B. Johnson presidency and fast economic growth with the development of high technology. To be state-specific, the bottom trend showed a clear upward tendency after shall-issue laws were adopted in 1986, whereas the top trend indicated a state never passing the laws with fluctuating rates along the time.

Interpretation of Results

Other than the omitted variable bias, another possible problem is the selection bias. It is undeniable that a great number of offences are carried out underground, so they fail to be accounted into the dataset. Bureaucracy likely tends to underreport the incidents of crime for political reasons as well. As a result, one cannot safely make a causality argument that the passage of shall-issue laws causes a decline in the violent crime rate.

As for the remaining threat to the validity, there are 22 states never passed shall-issue laws during 1977-1992(Ian& Donohue, 2002), so it is fair to deduce that there are states never passing the laws during 1977-1999. Accordingly, this is a violation against using the fixed effect model since no time-invariant regressor should be included.

Conclusion

The analysis in this paper shows that the adoption of shall-issue laws hurts the violent crime rate. The estimates are statistically significant but not economically evident when we apply pooled OLS, but the other way around after inducing the unobserved fixed effects. The result is inconsistent with that of Ian and Donohue who suggested the 'more guns, more crime' association (Ian& Donohue, 2003).

The conclusions above are subject to several limitations that lead to model misspecification. First, the omitted variable bias and selection bias result in biased and inconsistent estimated coefficients on the primary regressor. Second, the choice of the fixed effect model is invalid if the passage of shall-issue laws is time-invariant. The robustness of the findings is left for future research using data for more years.

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Appendix: Tables and Figures

Table1: Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
year	1173	88	6.64	77	99
violent	1173	503.07	334.28	47	2921.8
murder	1173	7.67	7.52	.2	80.6
robbery	1173	161.82	170.51	6.4	1635.1
incarceration rate	1173	226.58	178.89	19	1913
pb1064	1173	5.34	4.89	.25	26.98
pw1064	1173	62.95	9.76	21.78	76.53
pm1029	1173	16.08	1.73	12.21	22.35
population	1173	4.82	5.25	.4	33.15
average income	1173	13.72	2.55	8.55	23.65
density	1173	.35	1.36	0	11.1
state id	1173	28.96	15.68	1	56
shall	1173	.24	.43	0	1
ln(violent)	1173	6.03	.65	3.85	7.98

Table 1.b [Law Enacted]

Variable	Obs	Mean	Std. Dev.	Min	Max
year	285	92.44	5.77	77	99
violent	285	381.05	266.55	51.3	1244.3
murder	285	5.28	3.62	.2	20.3
robbery	285	97.9	87.77	6.4	416.8
incarceration rate	285	239.95	155.61	26	736
pb1064	285	3.75	3.35	.25	12.89
pw1064	285	66.6	7.19	47.02	76.53
pm1029	285	15.23	1.47	12.51	18.69
pop	285	3.66	3.82	.48	20.04
average income	285	13.72	1.82	10.09	18.73
density	285	.08	.07	0	.28
state id	285	34.14	15.28	2	56
shall	285	1	0	1	1
ln(violent)	285	5.69	.73	3.94	7.13

Table 1.c [Law Not Enacted]

Variable	Obs	Mean	Std. Dev.	Min	Max
year	888	86.57	6.26	77	99
violent	888	542.24	344.35	47	2921.8

	888	8.43	8.26	.7	80.6
murder	888	8.43	8.26	.7	80.6
robbery	888	182.34	184.97	6.4	1635.1
incarceration rate	888	222.29	185.62	19	1913
pb1064	888	5.85	5.18	.36	26.98
pw1064	888	61.77	10.18	21.78	73.39
pm1029	888	16.35	1.72	12.21	22.35
pop	888	5.19	5.59	.4	33.15
average income	888	13.72	2.75	8.55	23.65
density	888	.44	1.55	0	11.1
state id	888	27.3	15.45	1	56
shall	888	0	0	0	0
ln(violent)	888	6.13	.58	3.85	7.98

Table 2: Regression Analysis of the log of the violent crime rate and shall-issue laws

|	(1) SLR	(2) MLR	(3) SE	(4) SE+FE
shall	-.44***	-.37***	-.05	-.03
	(.05)	(.03)	(.04)	(.04)
incarceration rate		0***	0	0
		(0)	(0)	(0)
density		.03*	-.17	-.09
		(.01)	(.14)	(.12)
average income		0	-.01	0
		(.01)	(.01)	(.02)
population		.04***	.01	0
		(0)	(.01)	(.02)
pb1064		.08***	.1***	.03
		(.02)	(.03)	(.05)
pw1064		.03***	.04***	.01
		(.01)	(.01)	(.02)
pm1029		.01	-.05**	.07
		(.01)	(.02)	(.05)
State Effect	No	No	Yes	Yes
Time Effects	No	No	No	Yes
Clustered s.e.'s	No	No	Yes	Yes
F-Statistics	86.86	95.67	34.10	56.86
p-value of shall	0	0	0.28	0.50
R-squared	0.09	0.56	0.21	0.42

Standard errors are in parentheses

*** $p<.01$, ** $p<.05$, * $p<.1$

Descriptive Statistics before/after Log Transformation

Variables	Mean	Std. Dev.	Min	Max	p1	p99	Skew.
violent	503.07	334.28	47	2921.8	66.9	2010.6	2.54
ln(violent)	6.03	.65	3.85	7.98	4.2	7.61	-.43

Graph1: Scatter plot of ln (violent crime rate) versus year
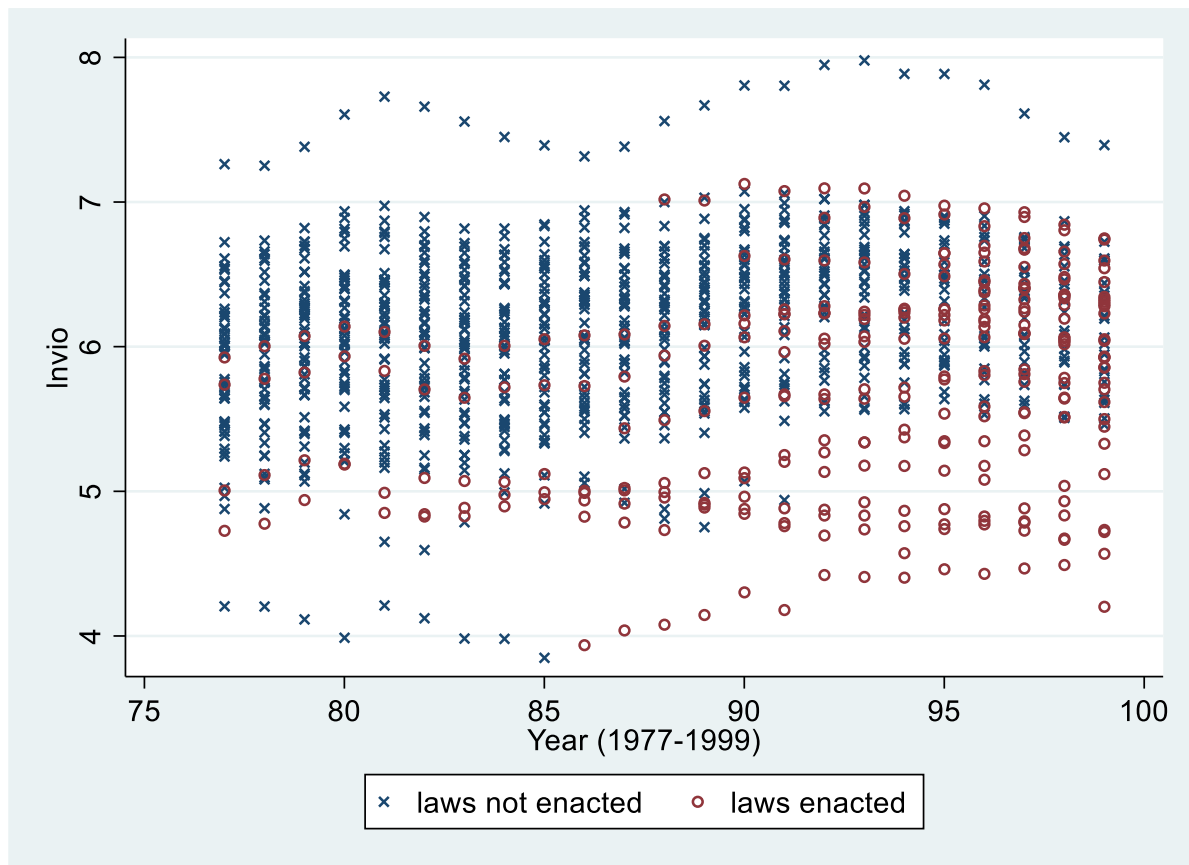
Graph2: Bar graph of the mean of ln (violent crime rate) of all states over year