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New Evidence on the Impact of Concealed Carry Weapon Laws on Crime

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Highlights:

- Consideration the type of laws that states had prior to passing concealed carry weapon ("shall issue") laws is proposed.

- Adopting "shall issue" laws only reduces the crime rate in states with "no issue" laws in place.

- "Shall issue" laws are redundant to "may issue" (restricted concealed carry) laws in terms of crime reduction.

Abstract

For more than a decade, there has been an academic debate over the deterrence effect of concealed carry weapon (shall issue) laws. However, all previous studies do not consider the types of gun-carry laws in place prior to the adoption of the "shall issue" laws. Using difference-in-difference methodology, findings of this study imply that considering the type of regulations that states had prior to passing "shall issue" laws matters and that "shall issue" laws do have a deterrence effect under certain circumstances. Adopting "shall issue" laws only reduces the crime rate in states with "no issue" laws in place, and "shall issue" laws are redundant to "may issue" (restricted concealed carry) laws in terms of crime reduction.

1. Introduction

The United States has more gun-related deaths than any other developed country in the world.[1] The estimated rate of private gun ownership (both licit and illicit) in the United States is 101.05 firearms per 100 people and the rate of all gun deaths per 100,000 people is 10.54.[2] Although crime rates have gone down significantly since 1980, there were still 8,124 firearm-related murders in 2014.[3]

Concealed carry weapon (shall issue) laws were introduced ostensibly to allow people to defend themselves, yet many decried that simply adding firearms to a society with a high rate of gun deaths is counterproductive. The two completely different beliefs about the effectiveness of "shall issue" laws have shown up in estimations of their effects as well. Some researchers (Lott and Mustard (1997), Barons and Lott (1998), Moody (2001), Plassmann and Whitley (2003), Gius (2013)), have shown that "shall issue" laws reduce the overall crime rate, but others (Rubin and Dezhbakhsh (1998), Ludwig (1998), Ayres and Donohue (2003a, 2003b)), have shown the crime rate has gone up since these laws were introduced.

What previous researchers have overlooked is that gun-carry regulations are heterogeneous and might have differing effects. When adopting "shall issue" laws, some states are transitioning from a "may issue" process while others are moving from a "no issue" process. The "shall issue" and "may issue" laws both allow private citizens to carry concealed weapons, but they require citizens to obtain a license in advance. While ''shall issue'' laws require the authorities to issue permits to qualified applicants, ''may issue'' laws give the authorities more latitude to reject applications. Therefore, unlike "shall issue" states, granting permits to carry is not the citizen's right in "may issue" states. This is why "may issue" laws are often called restricted concealed carry or limited issue laws by some (ex., National Rifle Association).[4] "No issue" laws, on the other hand, do not allow private citizens to carry concealed weapons in public at all. The hypothesis of this paper is that the effect of "shall issue" laws are likely dependent on

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[1] http://abcnews.go.com/blogs/health/2013/09/19/u-s-has-more-guns-and-gun-deaths-than-any-other-country-study-finds (Retrieve 2/24/2016)
[2] http://www.gunpolicy.org/firearms/region/united-states (Retrieve 2/24/2016)
[3] https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/expanded-homicide-data/expanded_homicide_data_table_8_murder_victims_by_weapon_2010-2014.xls (Retrieve 2/24/2016)
[4] http://web.archive.org/web/20081218111804/http://www.nraila.org/Issues/FactSheets/Read.aspx?ID=18(Retrieve 2/24/2016)

the types of gun carry regulations states had prior to the law change. Unlike "no issue" states, there is still a probability that citizens of "may issue" states could obtain the concealed carry license, which could result in criminal deterrence. Thereby, introducing "shall issue" laws would deter criminals in such a case only if "no issue" laws were in place.

The findings of this paper indicate that considering the type of regulations that states had prior to passing "shall issue" laws matters. While I find no deterrence effect for those states that switch to "shall issue" law from "may issue" laws, there exist a significantly positive effect (crime reduction) for those states that switched from "no issue" laws.

2. Background on "shall issue" Laws and Prior Research

During the 1920s and 1930s, many states passed laws that prohibited concealed carrying (Cramer and Kopel 1994). Based on these laws, some states did not allow their private citizens to carry concealed weapons at all (no issue laws) and some other states empowered local authorities to decide about issuing concealed carry permits (may issue laws). Thus, before 1960, there were only three types of gun carry regulations ("unrestricted", "may issue", and "no issue" laws) in the United States.[5] States then began to adopt the concealed carry weapon laws in different time spans, but this process was slow, and by 1988 only nine states had adopted "shall issue" laws (Grossman and Lee 2008).[6] However, in the 1990s legislative activity accelerated, with 37 states enacting "shall issue" laws as of 2014.

Criminal motives and deterrence research has long been the purview of criminologists, psychologists, and sociologists. Gary Becker (1968) was the first economist who extended this literature by introducing criminals' income as a part of expected utility. In his paper, Becker derived the supply of crime, which was negatively related to the punishment severity and the probability of conviction.[7] McDonald (1999) expanded Becker's theory by adding more

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[5] An *Unrestricted* gun-carry Laws are those that allow any private citizen to purchase, sell, and carry weapons (concealed or unconcealed) without any restrictions. Before 2003 Vermont was the only state with No-Control law. Alaska (2003), Arizona (2011), and Wyoming (2013) switched back to unrestricted laws as well.

[6] Alabama, Connecticut, Florida, Indiana, Maine, New Hampshire, North Dakota, South Dakota, Washington

[7] In Becker's (1968) paper the expected utility from committing an offense is defined as:

$E(U) = P_j U_j (Y_j - f_j) + (i - P_j) U_j (Y_j)$, where Y_j is an offender's income from committing an illegal activity; U_j is his utility function; P_j is his probability of conviction; and f_j is to be interpreted as the monetary equivalent of the punishment

determinant factors to the supply of crime function. He specifically showed that the less restrictive gun possession laws had a negative impact on the supply of crime. McDonald's (1999) findings are based on deterrence theory that implies criminals commit fewer crimes once they perceive the cost of committing a crime to be too high. Criminals have to be more cautious because their potential victims might be armed and more capable of protecting themselves. On the other hand, according to Duggan's (2001) findings, the mere presence of additional firearms in a community following the passage of less restrictive gun carrying legislation might increase the crime rate due to guns landing into the wrong hands. This is the so-called "more guns, more crime" effect.

Existence of the concealed carry weapon laws provides researchers with a good source to test the net effect of less restrictive gun laws. According to McDonald's (1999) findings, moving toward less restrictive gun carry (Ex. "shall issue") laws, positive deterrence effect dominates the negative "more guns, more crime" effect. This indicates that there should be lower crime observed in states that adopt "shall issue" laws. By using monthly homicide data from 1973-1992 for five counties, McDowall et al. (1995) was one of the first applied studies that assessed the effect of the "shall issue" laws.[8] Using the Autoregressive Integrated Moving Average model, the authors concluded that there is not enough evidence that "shall issue" laws could decrease the crime rate.

Lott and Mustard (1997) invigorated the literature and gun lobbyists by applying difference-in-difference (DD) methodology to estimate the effect of the "shall issue" laws on the crime rate for the period of 1977-1992. Based on their findings, Lott and Mustard concluded that states with the "shall issue" laws have lower crime rates than states with more restrictive gun carry regulations. Since then, this study has been endlessly cited by the National Rifle Association (NRA) and other gun advocates in support of their votes on behalf of concealed carry weapon laws.[9]

Lott and Mustard's findings were striking and prompted a large number of academic responses. By changing the econometric methodology and/or the model specification, other

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[8] Hinds county in Mississippi (Jackson), Multnomah and Clackamas (both counties were combined), Portland counties in Oregon, and Dade (Miami), Duval (Jacksonville), and Hillsborough (Tampa) counties in Florida)
[9] http://mediamatters.org/blog/2014/08/04/national-rifle-association-offers-weak-defense/200314(Retrieved 2/24/2016)

researches reanalyzed the Lott and Mustard dataset. Among these papers, Barons and Lott (1998), Bartley and Cohen (1998), Moody (2001), and Plassmann and Tideman (2001) corroborated the findings of Lott and Mustard. On the other hand, Rubin and Dezhbakhsh (1998), Ludwig (1998), and Ayres and Donohue (2003) concluded that "shall issue" laws increase the crime rate. Black and Nagin (1998) claimed that Lott and Mustard's findings are highly sensitive to minor changes in the sample. Based on their findings, Black and Nagin believed that there is not enough evidence to show a significant impact of the "shall issue" laws on the crime rate.

Due to many different and conflicting ideas about the effect of "shall issue" laws, the National Research Council (NRC) set aside one chapter of its book (Firearms and Violence: A Critical Review (2005)) to explore the causal effects of concealed carry weapon laws on crime rates. After reviewing the existing (and conflicting) literature and undertaking their own evaluation by using county-level data for the period of 1977-2000, a majority of the panel members came to the conclusion that the existing research failed to determine the true impact of "shall issue" laws. They also concluded that their own empirical results were imprecise and highly sensitive to changes in model specification and data period.

Donohue et al. (2010) raises the point that there may be serial correlation in panel data studies. This can lead to the underestimation of standard-errors (Wooldridge (2003, 2006), and Angrist and Pischke (2009)) posit that clustering standard-errors is a necessary correction in order to address this problem (Arellano (1987)). By using both county level and state level dataset for the period of 1977-2006 and after clustering standard-errors, Donohue et al. (2010), which is arguably the most reliable analysis to date, also found no statistical support for the deterrent effect of "shall issue" laws and brought all previous researches' findings under question.

Although Donohue et al. (2010) contradicts findings of McDonald (1999) concerning the deterrence effect of the less restrictive gun carry laws, they do not consider the types of gun-carry regulations in place prior to the adoption of the "shall issue" laws. This is perhaps a reason that they failed to find statistical support for an effect of "shall issue" laws. In this paper, I also find no statistical support for the impact of "shall issue" laws on the general crime rate. However, once I introduce separate treatment groups—those that switch from "may issue"

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process and those that switch from "no issue" process—I conclude that "shall issue" laws decrease the crime rate if states adopt the "shall issue" laws from "no issue" laws.

3. Conceptual Framework and Central Hypothesis

As mentioned, the contribution of this paper is based on this hypothesis that the deterrent effect is stronger when the changes in gun carry laws occur from "no issue", rather than "may issue". When law change occurs from "no issue", potential criminals are more deterred because their potential victims (private citizens) who were not allowed to carry guns at all, now have the right to carry guns concealed and are able to defend themselves. This is not necessarily the case when states change their laws from "may issue". Under "may issue" laws, there is still the probability that private citizens carry guns to defend themselves.

It also should be taken into consideration that adopting less restrictive gun laws like "shall issue" persuade people to buy more guns. Thus, it is reasonable to assume that adopting "shall issue" laws will increase the number of guns sold. The effect of "shall issue" laws on gun sales is important because many researchers (Ex. Cummings and Koepsell (1997), Mark Duggan (2001), Miller et al. (2002), Grassel and Wintemute (2003)) believe that the overall rate of death and suicide is usually higher in states with a high percentage of gun ownership than other states. Branas and Richmond (2009) also showed that those who possess handguns are more likely to die from violence than those without handguns. Thus, according to the hypothesis of this paper, "shall issue" is redundant to "may issue" and adopting the "shall issue" laws from "may issue" is an unnecessary change which might only serve to stimulate gun sales, without any benefit.

4. Data

In order to further understand the effect of the "shall issue" laws, I identify a set of states that enacted the concealed carry weapon laws from 1991-2008. I restricted the period to 1991-2008 because this is a period in which most of the states passed their "shall issue" laws.[10] Moreover, in their paper, Ayres and Donohue (2003a) pointed out that crime rose (especially in "non-shall issue" states) dramatically during the period from 1985-1992 and including this period may confound the estimation of the effect of "shall issue" laws. Ayres and Donohue's

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[10] From 16 states in 1993 to 37 states in 2008

(2003a) findings showed when they restricted the period to 1991-1999, there was a significant increase in crime rates. I also limited the period to 1991-2008 to avoid the probable impact of the great recession on crime rates. Additionally, after 2008 some states started changing their gun-carry laws from "shall issue" to "no restriction". This caused the number of "shall issue" states to drop from 37 in 2008 to 35 in 2015.

In 1991, 16 states were already "shall issue", therefore I always use these 16 states as control states as their status never changes. Between 1991 and 2008, 22 more states also adopted the "shall issue" laws at different times, which form my treatment group. As a result, the control group is composed of two types of states— those that are still not "shall issue" and those that already were "shall issue". Table 1 lists gun carry regulations for all states and also the type of gun carry laws that states had prior to the adoption of the "shall issue" laws.

By 2008, 37 U.S. states had passed "shall issue" laws. In this paper, information about the effective dates and coverage of the concealed carry laws were compiled from a variety of sources. The primary sources were the NRA, each state's legislation, and related news reports. In some cases of ambiguity, I also contacted different state police departments, sheriff's departments, state attorney general offices, and private attorneys who were specialists in gun-related laws to find out the effective dates of the concealed carry weapon laws in different states.

In order to study the effect of the concealed carry weapon laws on the crime rate, I used the FBI's Uniform Crime Report (FBI-UCR) dataset for six different types of crimes (murder, robbery, burglary, aggravated assault, larceny, and motor vehicle theft) for the period of 1991-2008. Following the majority of previous papers, I also chose these crimes because they are the only reported crime dataset by FBI-URC.[11] This dataset allows for yearly variation for each type of crime for all states. I dropped Alaska because they have changed their laws twice during the time span, rendering identification less clean.

Additionally, I control for the effect of other crime preventing policies —add-on gun laws, three-strike legislations, and permit to purchase a handgun laws— that might also affect crime rates. Both add-on gun laws and three-strike legislations are punishment enhancement

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[11] Since FBI recently changed the definition of rape, I did not include rape

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policies that are designed to reduce the crime rate. While the add-on gun laws impose harsher sentences for offenders who possess firearms during the commission of a felony, three-strike legislation imposes harsher sentences on offenders who are previously convicted of two prior serious offenses and then commit a third. States with permit to purchase a handgun laws require their citizens to obtain a permit for buying handguns besides obtaining a permit to carry handguns concealed.

In order to take into consideration the effect of economic conditions on crime rates, following Plassmann and Tideman (2001) and Donohue et al. (2010), I control for unemployment rates obtained from the Bureau of Labor Statistics (BLS). Following Lott and Mustard (1997) and most of the subsequent studies, I also add the log of population by age, race, and sex groups, number of police officers, lagged arrest rates, and states' income per capita as control variables. All demographic data are collected from the US Census. FBI-UCR dataset provides me with the number of police officers and arrest rates, and data for income per capita are retrieved from Federal Reserve Bank of St. Louis. Table 2 reports the mean of crime rates and other control variables in this analysis for both the treatment and control states. According to this table, before adopting "shall issue laws" most of the crimes in treated states had higher rates than those of control states.

5. Methodology

I begin by dividing all states into two groups—those that have changed their laws to "shall issue" by 2008 and those that have not changed their laws since 1991.[12] The goal is to see how adopting the concealed carry weapon laws might affect different types of crimes no matter what types of gun carry regulations states had prior to the adoption of "shall issue" laws. The intent is to replicate existing works with some modest improvements. Specifically, I use updated data, a larger control group, and more appropriate econometric methods. For this analysis I used the following regression model:

$$(1)\, CR_{sy} = S_s + Y_y + (Y*S)_s + \beta CCW_{sy} + \lambda X_{sy} + \varepsilon_{sy}$$

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[12] States could change their laws from "no issue" or "may issue.

Subscript *s* denotes states and subscript *y* denotes years. The terms S_s and Y_y are the state and year fixed effects. In order to provide the most robust estimates, following Donohue et al. (2003a and 2010) I also added $(Y*S)_s$ in order to control for state-specific time trends. The variable *CR* is the log of number of crimes per 100,000 people for the six different categories of crimes that I mentioned earlier. Specifically, I will run the model six times (once for each type of crime) to study the effect of "shall issue" laws on each type of crime separately. Our variable of interest (*CCW*) is the dummy that shows if states adopted the "shall issue" laws or not.[13] The term X_{sy} represents the state-level, time-varying set of control variables that might affect crime. As mentioned, these variables include the log of population by age, race, and sex groups, number of police officers, lagged arrest rates, income per capita, and other crime preventing regulations.

The main contribution of the paper is estimating separate effects by legislation type. Below is the model that I use for this analysis.

$$(2)\, CR_{sy} = S_s + Y_s + (Y*S)_{sy} + \beta MTS_{sy} + \beta NTS_{sy} + \lambda X_{sy} + \varepsilon_{sy}$$

In model (2), the variable *MTS* is set to one if the states changed their laws from "may issue" to "shall issue" laws and is set to zero otherwise. Thus, the treated states are those that adopt "shall issue" laws from "may issue" laws between 1991 and 2008. The variable *NTS* is set to one if the states changed their law from "no issue" to "shall issue" laws and is set to zero otherwise. So, the treatment group are those states that switch to "shall issue" laws from "no issue" laws between 1991 and 2008.

Assuming that control states and treatment states are comparable, the regressions for both models (1) and (2) use weighted least square where the weighting is each state's population. As noted above, standard errors are also clustered at the state level to allow for correlation in errors over time in a given state.

6. Results

This section consists of three parts. The first is essentially a replication exercise of previous approaches, albeit one with a larger control group and more appropriate econometric

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[13] CCW=1 if state is shall issue and zero otherwise

methods. In the second part, I examine if the type of regulations that states had prior to the adoption of "shall issue" laws matter or not. Finally, in the last part, I also apply the Probit estimator to check whether or not the findings are robust with respect to changing econometric methods.

6.1.   *Replication of existing work*

I first estimate model (1) in order to study the effect of adopting "shall issue" laws on the crime rate without considering the kind of regulations states had in the past. As mentioned, in order to prevent non-independence of observations from the same state that might affect the inference, standard-errors are clustered at the state level in all regressions. As table 3 makes it evident, estimations for the impact of "shall issue" laws are insignificant for all types of crime. These results are consistent with those of the NRC committee (2005) and Donohue et al. (2010), which imply there is not enough statistical support for the impact of "shall issue" laws on the crime rate.

6.2.   *Differential effects of moving from May Issue vs. No Issue*

In order to test whether or not the deterrent effect is stronger when the changes in gun carry laws occur from "no issue" rather than "may issue" (which is this paper's hypothesis) model (2) is estimated. Based on model (2) estimations, which are reported in table (4), adopting "shall issue" laws have no effect on crime rates when states change their laws from "may issue". Yet, there will be a significant reduction in theft crimes (robbery, burglary, and larceny) when a law change takes place from "no issue" laws.

For motor vehicle theft (which is another theft crime) the coefficient is still negative, sizable, and very close to being significant. One problem, which might cause the result for motor vehicle theft to be insignificant could be state-level data. Using state-level data one cannot differentiate urban and rural areas. This issue can bias the results toward no effect of the "shall issue" laws because in most rural areas in the states, the crime rate are low already, and there is less room for measurable downward effects of "shall issue" laws. However, this does not mean that the "shall issue" laws are not effective laws. I still must use state level data. Maltz and Targonski (2003) shows that FBI-UCR's county level dataset was less reliable because the law

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enforcement agencies voluntarily report the crime data to the FBI. Their findings also imply that by imputing missing agency data, the FBI's state-level data are less problematic.

Estimates of model (2) do not provide enough statistical support for the impact of "shall issue" laws on murder and aggravated assault. One could assume that murder and aggravated assault are less calculated crimes and more heat of the moment crimes. Thus, their criminals may be less inclined to think through whether victims have a gun or not, which skew estimations toward no effect of "shall issue" laws.

The first approach used in the current study is simple DD, which is common in the literature with clustered standard-errors at the state level. In DD methodology, the basic assumption is that the control group is a good counterfactual for the treatment group. This means that absent the intervention we would expect the same pattern of outcomes to exist over time in each group. To test this, I add leads to my model to determine if there were any significant differences in states by which gun legislation regimes fell and I find no differences. Plotting pretreatment trends can also help to recognize if the control group is a good counterfactual for the treatment group. In this study, as different states adopt the "shall issue" laws at different times, it is difficult to graph one specific pretreatment trend for the treatment group. In order to resolve this issue, I only plot the trends for the 1991-2000 period in which 13 states switched to "shall issue" laws. Looking at graphs 1-6, it can be seen that for all types of crime the pretreatment trends are the same in both control and treatment states. Additionally, as a placebo test to verify the validity of the research design, I drop all post-intervention years. Then I randomly assign fake treatment years to examine whether or not there is still a significant reduction in crime rate. As it can be seen in table (5) obtained results confirm that pre-treatment crime trends do not play a significant role in reducing crime rate, indicating that the results presented throughout are not spurious.

6.3. Robustness check

Plassmann and Tideman (2001) suggest that the count nature of crime data renders simple Weighted Least Square (GLS) to be the most appropriate method to estimate the effect of the concealed carry weapon laws. Using simple GLS is especially problematic for crimes with

low rates, such as murder and robbery. In order to consider this issue I also apply the Probit analysis by estimating:

$$(3)\, Y_{sy} = \mathbf{\Phi}(X_{sy}^{'}\beta + C_i)$$

Y is the percentage of crime rate in state *s* in year *y* and, *X* represents other variables that might affect *Y*, *c* is a state-specific time trend, and $\mathbf{\Phi}(\cdot)$ is the standard normal cumulative distribution function. In table 6, model (3) is estimated using the inverse normal of the crime rate as the dependent variable. As it can be seen in table 6 findings of this paper are not sensitive to change in estimation method and using non-linear methods do not change the findings. Overall, it can be said that not considering the gun carry regulations prior to the adoption of "shall issue" laws was the main reason that studies like Donohue et al. (2010) obtained no statistical support for effect of "shall issue" laws.

As discussed before, less restrictive gun laws (Ex. shall issue laws) likely result in more gun sales. Thus, according to the findings of the current study, for states with "may issue" regulations in place, adopting "shall issue" laws could only impact the gun sales without reducing crime. Since reviewing previous research suggests more guns will lead to more death (Cummings and Koepsell (1997), Mark Duggan (2001), Miller et al. (2002), Grassel and Wintemute (2003)), adopting the "shall issue" laws from "may issue" appears to potentially be an unnecessary and dangerous change.

7. Conclusion

The concealed carry weapon laws were passed in an attempt to reduce the crime rate. Policymakers believed that, although an increase in gun availability might lead to increased crime, the deterrent effect of "shall issue" laws dominates and will eventually reduce the crime rate. In this paper, I used DD methodology to estimate the effect of the "shall issue" laws on six different crime rates. The main difference between the current study and the previous ones is dividing the treated states into a "may issue" group and a "no issue" group.

Findings of this paper confirm that the concealed carry weapon laws likely reduce the crime rate, but only when the law change occurs from "no issue". However states that move from "may issue" to "shall issue" do not see a change because in "may issue" states, there is still a

probability for normal citizens to obtain a concealed carry permit. Additionally, adopting the "shall issue" laws is likely to increase the number of gun sales. Therefore, it is potentially true that moving from "may issue" to "shall issue" is a redundant change in terms of crime deterrence, with potentially dangerous consequences.

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Figure Captions

Figure 1— Pretreatment Trend for Robbery in both control and treated states

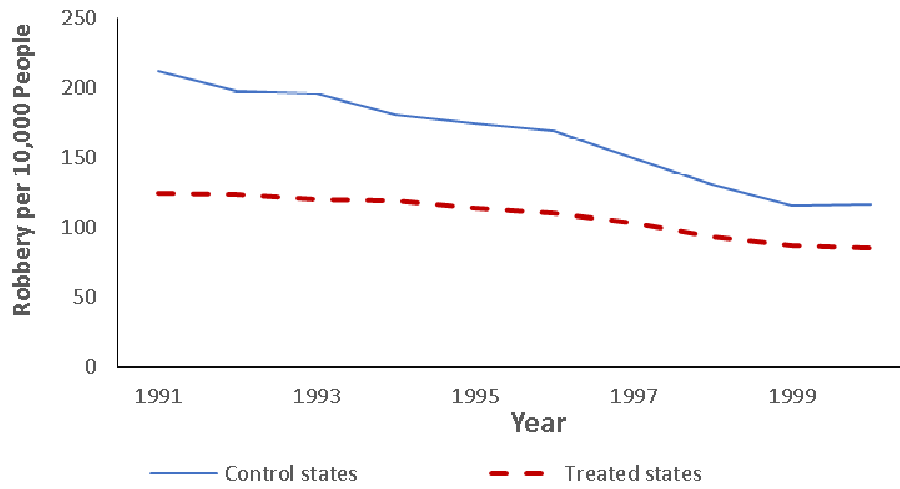Figure 2— Pretreatment Trend for Burglary in both control and treated states
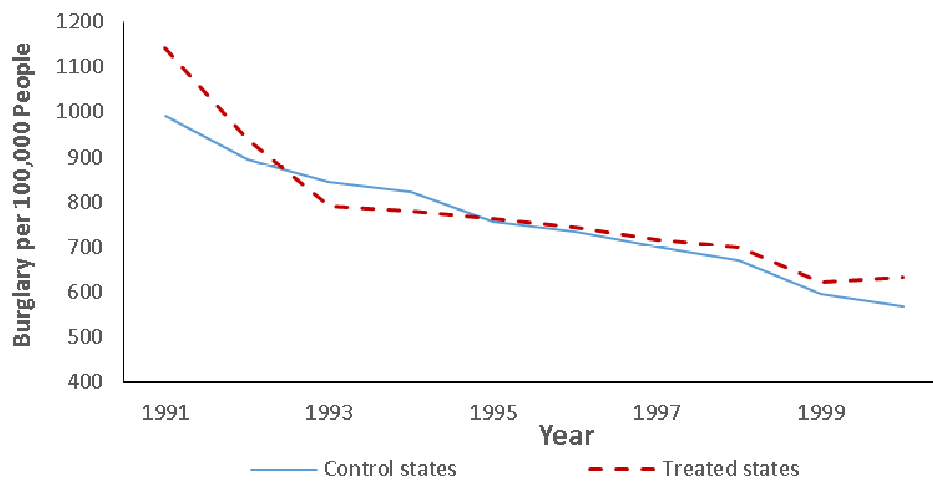

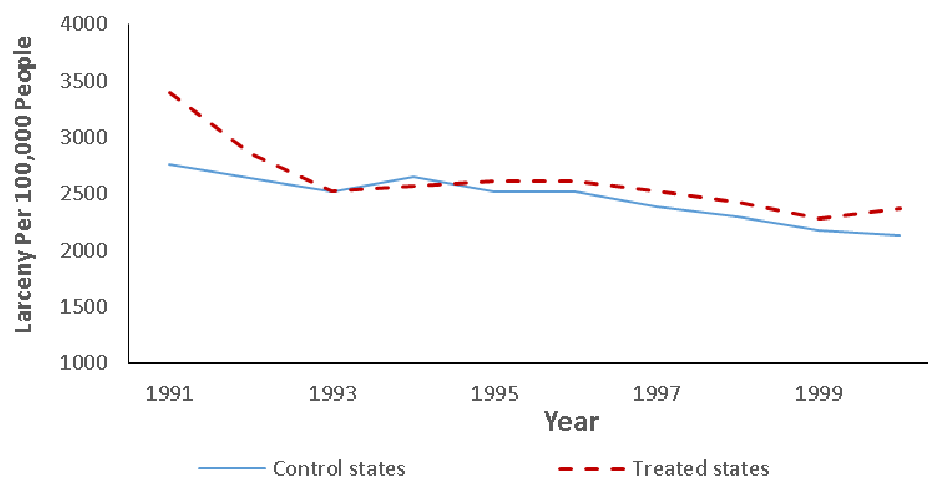Figure 3—Pretreatment Trend for Larceny in both control and treated states

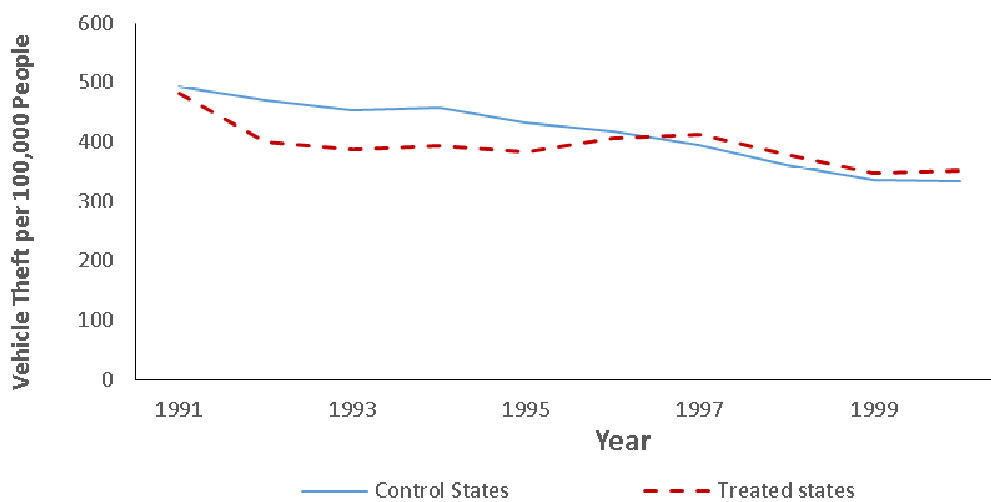Figure 4— Pretreatment Trend for Motor Vehicle Theft in both control and treated states

Figure 5—Pretreatment Trend for Murder in both control and treated states
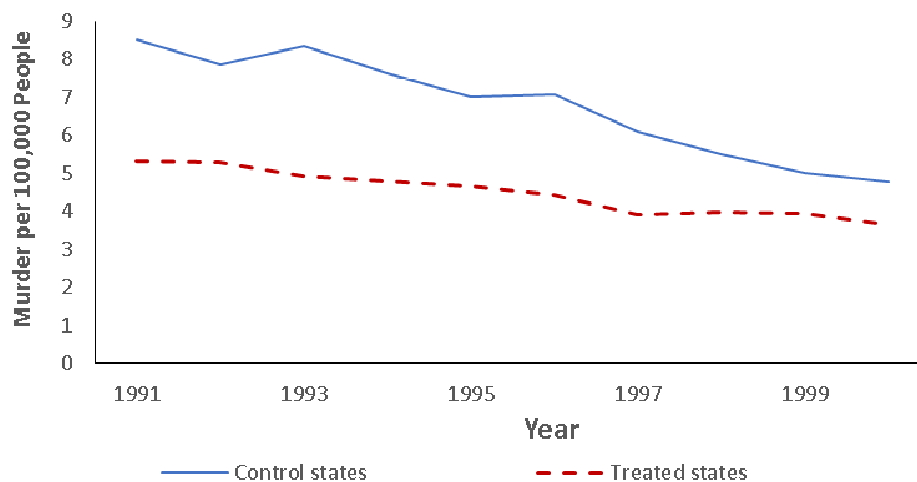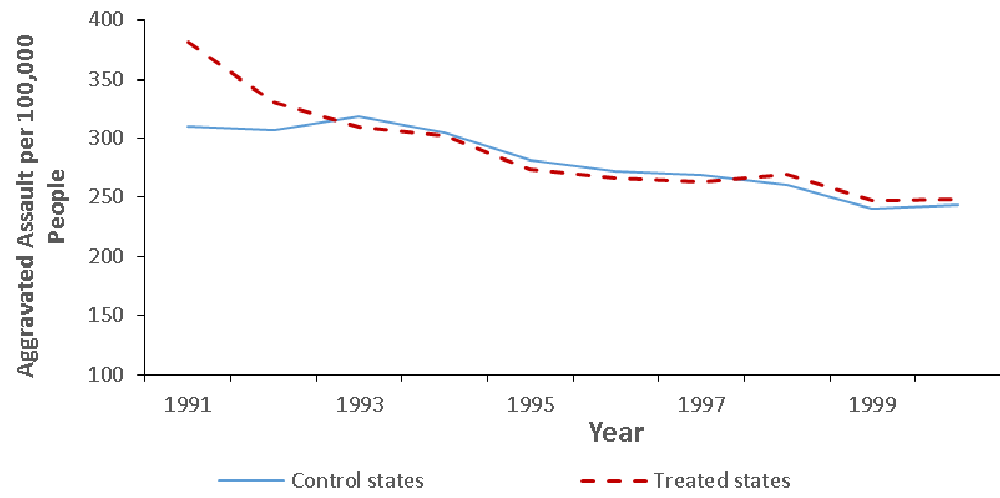

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Figure 6—Pretreatment Trend for Aggravated Assault in both control and treated states

Tables

Table 1 Year of Enactment of ''Shall Issue'' Laws

States	State Gun Carry Laws	Passage date
Alabama	Shall issue	<1991
Alaska*	Shall issue (change from No issue)	1994
Arizona	Shall issue (change from No issue)	1994
Arkansas	Shall issue (change from No issue)	1995
California	May issue	<1991
Colorado	Shall issue (change from May issue)	2003
Connecticut	Shall issue	<1991
Delaware	May issue	<1991
District of Colombia	No issue	<1991
Florida	Shall issue	<1991
Georgia	Shall issue	<1991
Hawaii	May issue	<1991
Idaho	Shall issue	<1991
Illinois	No issue	<1991
Indiana	Shall issue	<1991
Iowa	May issue	<1991
Kansas	Shall issue (change from No issue)	2006
Kentucky	Shall issue (change from No issue)	1996
Louisiana	Shall issue (change from May issue)	1996
Maine	Shall issue	<1991
Maryland	May issue	<1991
Massachusetts	May issue	<1991
Michigan	Shall issue (change from May issue)	2001
Minnesota	Shall issue (change from May issue)	2003
Mississippi	Shall issue	<1991
Missouri	Shall issue (change from No issue)	2003
Montana	Shall issue	<1997
Nebraska	Shall issue (change from No issue)	2006
Nevada	Shall issue (change from May issue)	1995
New Hampshire	Shall issue	<1991
New Jersey	May issue	<1991
New Mexico	Shall issue (change from No issue)	2003
New York	May issue	<1991
North Carolina	Shall issue (change from No issue)	1995
North Dakota	Shall issue	<1991
Ohio	Shall issue (change from No issue)	2004
Oklahoma	Shall issue (change from No issue)	1995
Oregon	Shall issue	<1991
Pennsylvania	Shall issue	<1991
Rhode Island	May issue	<1991
South Carolina	Shall issue (change from May issue)	1996
South Dakota	Shall issue	<1991
Tennessee	Shall issue (change from May issue)	1994
Texas	Shall issue (1change from No issue)	1995
Utah	Shall issue (change from May issue)	1995

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Vermont	Unrestricted	<1991
Virginia	Shall issue (change from May issue)	1995
Washington	Shall issue	<1991
West Virginia	Shall issue	<1991
Wisconsin	No Issue	<1991
Wyoming*	Shall issue (change from May issue)	1994

*Alaska in 2003 changed its laws to unrestricted once again. That is why Alaska is excluded from treatment group.

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Table 2 Mean of key variables in analysis before adopting of "shall issue" laws

Variable	Means for Control States	Means for Treated States
Number of Crime per 100,000 people:		
Robbery	136.15	131.45
Burglary	725.12	863.96
Larceny	2366.98	2708.53
Murder	6.01	6.24
Motor Vehicle Theft	385.12	388.63
Aggravated Assault	276.18	317.63
Number of Arrests per 100,000 people:		
Robbery	36.46	30.36
Burglary	88.00	97.69
Larceny	396.45	493.27
Murder	11.22	6.11
Motor Vehicle Theft	42.28	41.55
Aggravated Assault	112.75	117.70
Other	3470.44	3839.30
Population Characteristic:		
State population	5667791	4798127
Population per square mile	413.37	80.69
Male population	2776931	2341280
Female population	2890860	2456847
Race Age data (% of population):		
White	82.06	87.81
Black	11.69	9.14
Other Race	6.25	3.05
Male 10-19	7.34	7.67
Male 20- 29	6.97	7.10
Male 30-39	7.38	7.96
Male 40-49	7.35	7.05
Male 50-64	7.59	6.57
Male over 65	5.36	5.09
Female 10-19	6.98	7.30
Female 20- 29	6.85	7.03
Female 30-39	7.42	8.02
Female 40-49	7.49	7.18
Female 50-64	8.04	7.01
Female over 65	7.49	7.30
Number Police Officer per 100,000		
Male officers	204.21	185.2964
Female officers	23.66	19.0738
Unemployment rate	5.09	5.14
Income Per Capita ($/year)	29635.74	22759.16

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Table 3 Effect of adopting "shall issue" laws on crime rates without consideration of the type of the regulation states had in place prior to the law change (1991-2008)

VARIABLES	Robbery	Burglary	Larceny	Motor Vehicle Theft	Murder	Aggravated Assault
Shall Issue	-0.0218	-0.0404	-0.0318	-0.00486	-0.0265	0.0562
	(0.0331)	(0.0317)	(0.0289)	(0.0340)	(0.0396)	(0.0342)
Observations	770	770	770	770	770	770
Other Policies	yes	yes	yes	yes	yes	yes
State and Year Fixed Effect	yes	yes	yes	yes	yes	yes
State Specific Fixed Time Trend	yes	yes	yes	yes	yes	yes

**,* denote statistical significance at the 0.05 and 0.10 levels, respectively

The treatment variable is "shall issue" that equals one when a states adopt the "shall issue" laws, regardless of type of the gun carry laws that state had in the past, and zero otherwise. Estimations in every cell are obtained from a separate regression. Standard errors are in parentheses and are clustered at the state level to allow for arbitrary patterns in heteroskedasticity and correlation in errors over time in a given state. All regressions use weighted least square where the weighting is each state's population.

Table 4 Effect of adopting "shall issue" laws on crime rates with consideration of the type of the regulation states had in place prior to the law change (1991-2008)

VARIABLES	Robbery	Burglary	Larceny	Motor Vehicle Theft	Murder	Aggravated Assault
May Issue to Shall Issue	0.0495	0.0323	0.0149	0.0696	0.0225	0.0449
	(0.0449)	(0.0378)	(0.0377)	(0.0465)	(0.0281)	(0.0473)
No Issue to Shall Issue	-0.0727*	-0.0923**	-0.0651*	-0.0581	-0.0615	0.0426
	(0.0413)	(0.0393)	(0.0367)	(0.0415)	(0.0575)	(0.0449)
Observations	770	770	770	770	770	770
Other Policies	yes	yes	yes	yes	yes	yes
State and Year Fixed Effect	yes	yes	yes	yes	yes	yes
State Specific Time Trend	yes	yes	yes	yes	yes	yes

**,* denote statistical significance at the 0.05 and 0.10 levels, respectively

The variable "may issue to shall issue" equals one when a state adopt the "shall issue" laws from

"may issue" laws and zero otherwise. The variable "no issue to shall issue" equals one when a state adopt the "shall issue" laws from "no issue" laws and zero otherwise. Estimations in every cell are obtained from a separate regression. Standard errors are in parentheses, and are clustered at the state level to allow for arbitrary patterns in heteroskedasticity and correlation in errors over time in a given state. All regressions use weighted least square where the weighting is each state's population.

Table 5 Placebo test for the effect of adopting "shall issue" laws on crime rates with consideration of the type of the regulation states had in place prior to the law change (1991-2002)

VARIABLES	Robbery	Burglary	Larceny	Motor Vehicle Theft	Murder	Aggravated Assault
May Issue to Shall Issue	0.0284	0.0655	0.0890	0.197**	0.00501	-0.0620
	(0.0684)	(0.0556)	(0.0613)	(0.0953)	(0.0655)	(0.107)
No Issue to Shall Issue	0.0283	-0.0459	-0.0319	-0.0645	0.0590	-0.197
	(0.141)	(0.0595)	(0.0610)	(0.0836)	(0.0698)	(0.122)
Observations	381	381	381	381	381	381
Other Policies	yes	yes	yes	yes	yes	yes
State and Year Fixed Effect	yes	yes	yes	yes	yes	yes
State Specific Time Trend	yes	yes	yes	yes	yes	yes

**,* denote statistical significance at the 0.05 and 0.10 levels, respectively

The variable "may issue to shall issue" equals one when a state adopt the "shall issue" laws from "may issue" laws and zero otherwise The variable "No issue to shall issue" equals one when states adopt the "shall issue" laws from "no issue" laws and zero otherwise. Estimations in every cell are obtained from a separate regression. Standard errors are in parentheses, and are clustered at the state level to allow for arbitrary patterns in heteroskedasticity and correlation in errors over time in a given state. All regressions use weighted least square where the weighting is each state's population. I drop all post-intervention years. Then I randomly assign fake treatment years to examine whether or not there is still a significant reduction in crime rate.

Table 6 Effect of adopting "shall issue" laws on crime rates with consideration of the type of the regulation states had in place prior to the law change , using the Probit estimator (1991-2008)

VARIABLES	Robbery	Burglary	Larceny	Motor Vehicle Theft	Murder	Aggravated Assault
May Issue to Shall Issue	-0.0222*	-0.0324**	-0.0256*	-0.0191	-0.0178	0.0138
	(0.0127)	(0.0139)	(0.0148)	(0.0139)	(0.0167)	(0.0142)
No Issue to Shall Issue	0.0151	0.0128	0.00739	0.0235	0.00605	0.0148
	(0.0135)	(0.0131)	(0.0156)	(0.0156)	(0.00834)	(0.0153)
Observations	771	771	771	771	771	771
Other Policies	yes	yes	yes	yes	yes	yes
State and Year Fixed Effect	yes	yes	yes	yes	yes	yes
State Specific Time Trend	yes	yes	yes	yes	yes	yes

**,* denote statistical significance at the 0.05 and 0.10 levels, respectively

The variable "may issue to shall issue" equals one when a state adopt the "shall issue" laws from "may issue" laws and zero otherwise The variable "No issue to shall issue" equals one when states adopt the "shall issue" laws from "no issue" laws and zero otherwise. Estimations in every cell are obtained from a separate regression. Standard errors are in parentheses, and are clustered at the state level to allow for arbitrary patterns in heteroskedasticity and correlation in errors over time in a given state. All regressions use weighted least square where the weighting is each state's population.