May 2017

Three Essays on Crime Deterrence Laws

Mehdi Barati
University of Wisconsin-Milwaukee

THREE ESSAYS ON CRIME DETERRENCE LAWS

by

Mehdi Barati

A Dissertation Submitted in

Partial Fulfillment of the

Requirements for the Degree of

Doctor of Philosophy

in Economics

at

The University of Wisconsin-Milwaukee

May 2017

ABSTRACT

THREE ESSAYS ON CRIME DETERRENCE LAWS

by

Mehdi Barati

The University of Wisconsin-Milwaukee, 2017
Under the Supervision of Professor Scott Adams

The main purpose of this dissertation is to apply empirical and theoretical economics methodologies to analyze multiple topics in economics of crime, which have important policy implications. This dissertation consists of three chapters.

Chapter 1 introduces new evidence on the impact of concealed carry weapon laws on crime. 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 "shall issue" laws. Using difference-in-difference methodology, the findings of this study imply that considering the type of regulations that states had prior to passing "shall issue" laws matters and "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.

Chapter 2 investigates the deterrence effect of a marginal increase in punishment severity for illegal gun carrying. I explore New York's 2006 sentence enhancement for illegal gun possession, which effectively added to the sentence for any crime committed with a firearm. Results show that the increase in punishment contributed to the decreasing crime rates in New York after 2006.

Chapter 3 investigates the impact of a marginal change in punishment severity on crime rate. I exploit Arkansas' (AR) 2011 adjustment in the felony threshold for theft from $500 to $1000. The decrease in punishment contributed to an increased theft rates in AR, suggesting criminals responded to the reduced crime-specific cost. Findings also indicate that the likely lower incarceration for theft did not lead to an increase in other crimes.

This dissertation is gratefully dedicated to my wife, Hadiseh Faridiravana, for her kindness, devotion, and for her endless support. Without her support, this would have not been possible

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

ACKNOWLEDGEMENTS

I am using this opportunity to express my gratitude to everyone who supported me throughout the course of this dissertation project. I would like to express my deepest appreciation to my advisor, Prof. Scott Adams for his support, understanding, patience, and most importantly his valuable advice from the very beginning. I would also like to express my gratitude to the members of my dissertation committee (in alphabetical order): Prof. Scott Drewianka, Prof. John Heywood, and Prof. Owen Thompson for their invaluably constructive criticism and friendly advice.

Chapter 1 : New Evidence on the Impact of Concealed Carry Weapon Laws on Crime

1.1. 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

[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)

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 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.

1.2. 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

[4] http://web.archive.org/web/20081218111804/http://www.nraila.org/Issues/FactSheets/Read.aspx?ID=18(Retrieve 2/24/2016)

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 2008.

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 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.

---

[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:
$E(U) = P_j U_j(Y_j - f_j) + (1 - P_j)\, U_j(Y_j)$, 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

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 (ARIMA) 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 "shall issue" laws on the crime rate for the period of 1977-1992. Based on their findings, Lott and Mustard concluded that states with "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 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

---

[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)

findings of Lott and Mustard. On the other hand, Rubin and Dezhbakhsh (1998), Ludwig (1998), and Ayres and Donohue (2003a, 2003b) 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 “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) raise the point that there may be serial correlation in panel data studies. This can lead to the underestimation of standard-errors (Wooldridge, 2003, 2006; 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” process and those that switch from “no issue” process—I conclude that “shall issue” laws decrease the crime rate if states adopt “shall issue” laws from “no issue” laws.

1.3. 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.

1.4. Data

In order to further understand the effect of “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 (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 31 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 “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

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

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 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

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

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. 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.

1.5. 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:

$$CR_{sy} = S_s + Y_y + (\mathrm{Y} * \mathrm{S})_s + \beta CCW_{sy} + \lambda X_{sy} + \varepsilon_{sy} \tag{1.1}$$

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 $(\mathrm{Y} * \mathrm{S})_s$ 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

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

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.

$$CR_{sy} = S_s + Y_y + (\mathrm{Y} \times \mathrm{S})_s + \alpha MTS_{sy} + \beta NTS_{sy} + \lambda X_{sy} + \varepsilon_{sy} \tag{1.2}$$

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.

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

1.6. 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 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.

1.6.1. 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.

1.6.2. 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 data are less reliable because the law 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. That is, 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 "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.

1.6.3. 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:

$$Y_{sy} = \mathbf{\Phi}(X'_{sy}\beta + C_i) \tag{1.3}$$

"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 $\mathbf{\Phi}(.)$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 “shall issue” laws from “may issue” appears to potentially be an unnecessary and dangerous change.

1.7. 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 “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.

1.8. References

Adams, Scott; Blackburn, McKinley L; Cotti, Chad D. (August 2012). Minimum Wages and Alcohol-Related Traffic Fatalities among Teens. *The Review of Economics and Statistics*, 94(3): 828-840.

Angrist, Joshua; Pischke, Jorn-Steffen. (2009). *Mostly Harmless Econometries: : An empiricist's companion.* Princeton: Princeton University Press.

Arellano, M. (November 1987). PRACTITIONERS’ CORNER: Computing Robust Standard Errors for Within-groups Estimators. *Oxford Bulletin of Economics and Statistics*, 49(4): 431-434.

Ayres, Ian; Donohue, John j. (2003b). The Latest Misfires in Support of the "More Guns, Less Crime" Hypothesis. *Stanford Law Review*, 55(4): 1371-1389.

Ayres, Ian; Donohue, John J. (2009). More Guns Less Crime Fails Again: The Latest Evidence from 1977-2006. *Econ Journal Watch*, 6(2): 218-238.

Ayres, Ian; Donohue, John J. (April 2003a). Shooting down the "More Guns, Less Crime" Hypothesis. *Stanford Law Review*, 55(4): 1193-1312.

Bartley, William Alan; Cohen, Mark A. (April 1998). The Effect Of Concealed Weapons Laws: An Extream Bound Analysis. *Economic Inquiry*, 36(2): 258-265.

Becker, Gary S. (1968). Crime and Punishment: An Economic Approach. *The Journal of Political Economy*, 76(2): 169-217.

Black, Dan A.; Nagi, Daniel S. (January 1998). Do Right-to-Carry Laws Deter Violent Crime? *The Journal of Legal Studies*, 27(1): 209-219.

Branas, C. C., Richmond, T. S., Culhane, D. P., Ten Have, T. R., & Wiebe, D. J. (2009). Investigating the link between gun possession and gun assault. *American journal of public health*, 99(11): 2031-2040.

Bronars, Stephen G., and John R. Lott. (May 1998). Criminal Deterrence, Geographic Spillovers, and the Right to Carry Concealed Handguns. *The American Economic Review*, 88(2): 475-479.

Cramer, Clayton E; Kopel, David B. (Spring 1995). Shall Issue": The New Wave of Concealed Handgun Permit Laws. *Tennessee Law Review*, 62(3): 679-757.

Cummings, P., Koepsell, T.D., Grossman, D.C., Savarino, J. and Thompson, R.S. (1997). The association between the purchase of a handgun and homicide or suicide. *American Journal of Public Health*, 87(6): 974-978.

Dezhbakhsh , Hashem ; Rubin, Paul H. (May, 1998). Lives Saved or Lives Lost? The Effects of Concealed-Handgun Laws on Crime. *The American Economic Review*, 88:2 468-474.

Donohue, John J. (2004). Guns, Crime, and the Impact of State Right-to-Carry Laws. *Fordham Law Review*, 73: 623-652.

DonohueIII, John J.; Aneja, Abhay; Zhang, Alexandria. (October 2011). The Impact of Right-to-Carry Laws and the NRC Report: Lessons for the Empirical Evaluation of Law and Policy. *American Law and Economics Association*, 13(2): 565-631.

Duggan, Mark. (October 2001). More Guns, More Crime. *Journal of Political Economy*, 109(5): 1086-1114.

Gius, Mark. (2014). An examination of the effect concealed weapos laws and assault weapons bans on state-level murder rates. *Applied Economics Letter*, 21(4): 265-267.

Grassel, K. M., Wintemute, G. J., Wright, M. A., & Romero, M. P. (2003). Association between handgun purchase and mortality from firearm injury. *Injury Prevention*, 9(1): 48-52.

Grossman, Richard S.; Lee, Stephen A. (April 2008). May Issue Versus Shall Issue: Explaining The Pattern Of Concealed-Carry Handgun Laws, 1960–2001. *Contemporary Economic Policy*, 26(2): 198–206.

Lanza, Steven P. (April 2014). The effect of firearm restrictions on gun-related homicides across US states. *Applied Economics Letters*, 21(13): 902-905.

Lott, Jr, John R; Mustard, David B. (January 1997). Crime, Deterrence, and Right-to-Carry Concealed Handguns. *The Journal of Legal Studies*, 26(1): 1-68.

LUDWIG, JENS. (1998). Concealed-Gun-Carrying Laws and Violent Crime: Evidence from State Panel Data. *International Review of Law and Economics*, 18(3): 239 –254.

Maltz, Michael D. (2003). Cluster-sample Methods in Applied Econometrics. *American Economic Review*, 93(2): 133-138.

Maltz, Michael D.; J. Targonski. (Jun 2003). Measurement and other errors in county-level UCR data: A reply to Lott and Whitley. *Journal of Quantitative Criminology*, 19(2): 199-206.

McDonald, John F. (1999). An economic analysis of guns, crime, and gun control. *Journal of criminal justice*, 27(1): 11-19.

McDowall, David; Loftin, Colin; Wiersema, Brian. (1995). Easing concealed firearms laws: Effects on homicide in three states. *The Journal of Criminal Law and Criminology (1973-)*, 86(1): 193-206.

Miller, M., Azrael, D., & Hemenway, D. (2002). Firearm availability and suicide, homicide, and unintentional firearm deaths among women. *Journal of Urban Health*, 79(1): 26-38.

Moody, Carlisle E. (October 2001). Testing for the Effects of Concealed Weapons Laws: Specification Errors and Robustness. *Journal of Law and Economics*, 44(S2): 799-813.

National Research Council. (2005). Firearms and Violence: A Critical Review. *Washington, DC: National Academies Press*.

Plassmann, Florenz; Tideman, T. Nicholaus. (October 2001). Does the Right to Carry Concealed Handguns Deter Countable Crimes:Only a Count Analysis Can Say. *Journal of Law and Economics*, 44: 771-798.

Plassmann, Florenz; Whitley, John. (April 2003). Confirming "More Guns, Less Crime". *Stanford Law Review*, 55(4): 1313-1369.

Wooldridge , J. M. (2003). Cluster-Sample Methods in Applied Econometrics. *American*, 93: 133-138.

Wooldridge, J. M. (2006). CLUSTER-SAMPLE METHODS IN APPLIED ECONOMETRICS:. *Unpublished manuscript, Michigan State University, Department of Economics.*

Figure 1.1. Pretreatment Trend for Robbery in both control and treated states

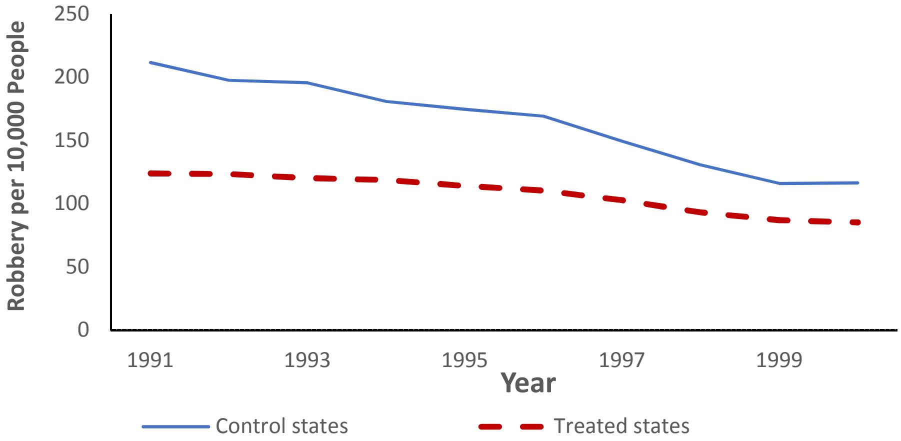

Figure 1.2. Pretreatment Trend for Burglary in both control and treated states

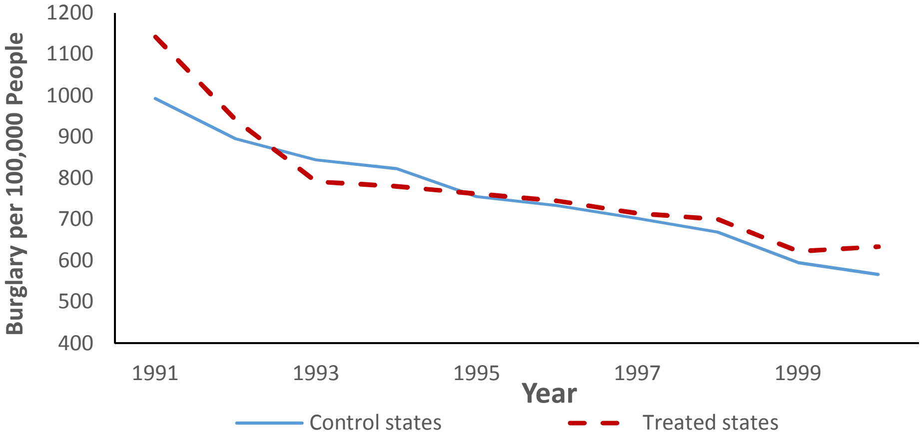

Figure 1.3. Pretreatment Trend for Larceny in both control and treated states

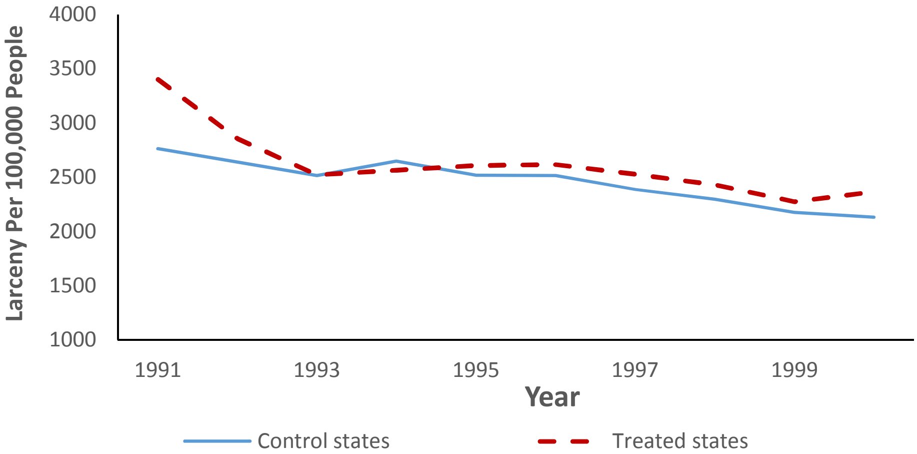

Figure 1.4. Pretreatment Trend for Motor Vehicle Theft in both control and treated states

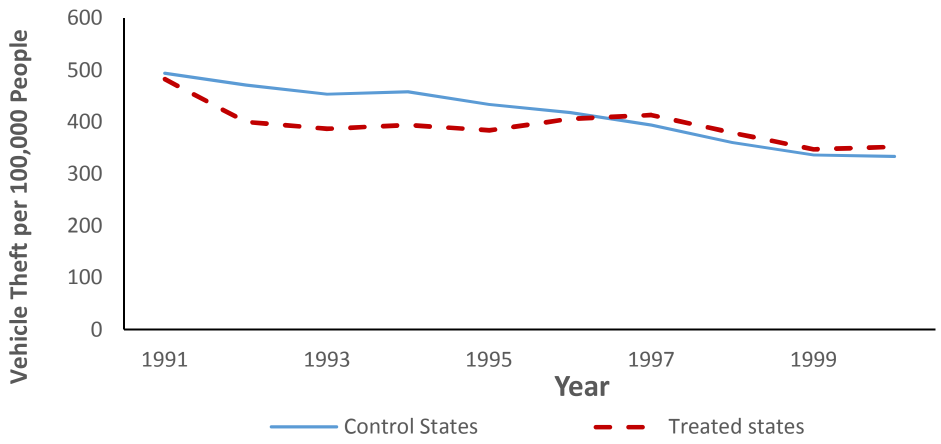

Figure 1.5. Pretreatment Trend for Murder in both control and treated states

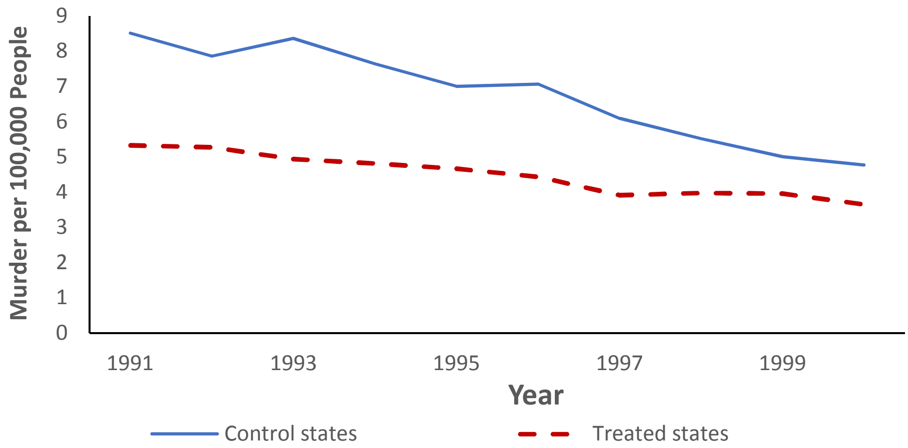

Figure 1.6. Pretreatment Trend for Aggravated Assault in both control and treated states

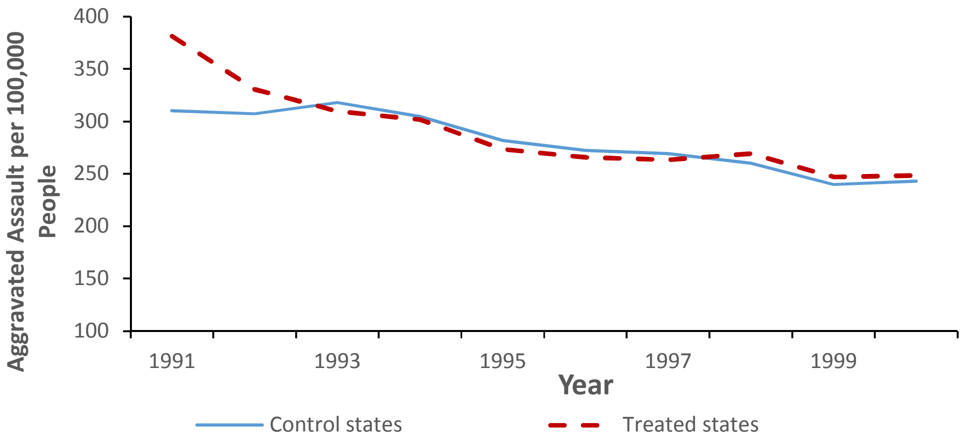

Table 1.1. Year of Enactment of ‘‘Shall Issue’’ Laws

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
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

Table 1.2. 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

Table 1.3. 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 1.4. 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	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 1.5. 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 1.6. 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	0.0151	0.0128	0.00739	0.0235	0.00605	0.0148
	(0.0135)	(0.0131)	(0.0156)	(0.0156)	(0.00834)	(0.0153)
No 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)
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.

Chapter 2 : The Effect of More Severe Punishments for Illegal Gun Carrying on Crime

2.1. Introduction

The United States has the largest prison population per capita in the world. While the U.S. incarceration rate was 693 per 100,000 people in 2014, the incarceration rate was only 114 per 100,000 people in Canada.[14] Despite this mass incapacitation through incarceration, the U.S. also suffers from high rates of violent crime. For the majority types of violent crime (murder, robbery, and forcible rape), the U.S. is rated among the top 10 countries within the Organization for Economic Cooperation and Development (OECD) member countries.[15] To illustrate this, the homicide rate in the U.S. was 4.5 per 100,000 in 2014, while this rate was only 1.4 in Canada.[16,17]

Due to the simultaneously high rates of crime and incarceration in the U.S., enacting policies that reduce crime rates without raising the prison population remain at the top of the research agenda for many criminologists, sociologists, and economists. Among these types of policies, enhancing punishment severity seems to be a very useful tool that could potentially lower crime rates. Punishment enhancement could reduce the crime rate through two different channels—incapacitation and deterrence. However, since incapacitation is costly and puts pressure on taxpayers, it is the latter that will dictate whether or not harsher sentencing is economically efficient.

In order to assess the efficiency of increased sentence length on crime rates, this study takes advantage of the state of New York's (NY) increased sentence length for illegal gun possession.

---

[14] International Center of Prison Population
[15] http://www.civitas.org.uk/content/files/crime_stats_oecdjan2012.pdf (Retrieved 6/17/2016)
[16] https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2014/crime-in-the-u.s.-2014/tables/table-16 (Retrieved 6/17/2016)
[17] http://www.statcan.gc.ca/daily-quotidien/151125/dq151125a-eng.htm (Retrieved 6/17/2016)

NY raised the minimum punishment for carrying handguns illegally from a 1-year prison sentence to 3.5 years in 2006.[18] I use the synthetic control method, along with difference-in-difference (DD) methodology, to analyze the impact of NY's punishment enhancement policy on three different types of crime: robbery, murder, and larceny. Findings indicate that more severe punishment for the illegal possession of firearms does reduce the crime rate, and a large portion of this reduction (both in the short and long run) is due to criminal deterrence rather than incapacitation.

2.2. Literature review

Becker's (1968) paper was the first to introduce an economic model for crime. The crime model implied that the supply of crime is negatively related to the certainty of punishments (in terms of the higher probability of conviction and arrest rate) and the severity of punishment. Since then, there has been an academic debate over the potential advantages of the severity of punishment. Some researchers (Decker & Kohfeld, 1990; Kim et al., 1993; Doob & Webster, 2003; Robinson & Darley, 2004) favor the certainty of punishment over the severity of punishment, while others (Kessler & Levitt, 1999; Mendes & McDonald, 2001; Helland & Tabarrok, 2004; Lee & McCarry, 2009; Abrams 2011) have shown that the severity of punishment is as important as other crime-preventing factors and does reduce the crime rate. They argued that model misspecification is the reason that some prior studies had failed to find statistical support for the impact of punishment severity.

Kessler and Levitt (1999) were one of the first to distinguish between the deterrence effect of harsher sentences and incapacitation. The authors assessed the impact of punishment severity by making use of sentence enhancement in California (CA) for a selected group of crimes.[19] In

[18].http://www.nytimes.com/2013/01/21/nyregion/prison-not-as-mandatory-as-ny-state-gun-laws-say.html?_r=2& (Retrieved 6/17/2016)
[19] California's Proposition 8 passed in 1982.

order to separate the deterrence and incapacitation effects, they argued that deterrence is the only cause of a short-run drop in crime rates because a defendant subjected to new punishment would be imprisoned even in the absence of a law change. According to their findings, more severe types of punishments have an immediate deterrence effect, which is the main reason for the short-run crime drop in CA.

Following Kessler and Levitt's (1999) argument, Abrams (2011) also attempts to separate the deterrence effect associated with punishment severity from that of incapacitation. For this purpose, Abrams (2011) evaluates the impact of add-on gun laws on crime rates. Add-on gun laws impose harsher sentences on offenders who possess firearms during the commission of a felony. Using cross-state variation, he shows that the deterrence effect of add-on gun laws could reduce gun robberies by roughly 5 percent in the short run.

In order to study the efficiency of punishment enhancement policies, the current study makes use of some of the aforementioned papers' strategies to identify the impact on crime rates of NY's increased minimum jail time for illegal possession of a gun. This study, however, differs substantially from other studies in methodology, type of studied crimes, and conclusion.

2.3. Conceptual Framework

According to Becker's crime model, more severe punishment will result in crime reduction. Increasing minimum jail time for illegal possession of a gun in NY could be considered as a more severe punishment because if an arrested violator commits a crime with an illegal firearm, the minimum jail time will be added to the normal punishment. Thus, one could hypothesize that NY's law change should lead to a decreasing crime trend in the state after enactment of the law in 2006.

One important issue that should be taken into consideration is that an increase in minimum jail time for illegal possession of a gun could only deter criminals who are reliant on carrying guns. Among different types of crimes, according to the FBI's Uniform Crime Report (UCR) dataset and previous studies, it could be assumed that for committing murder and robbery, most criminals need their guns.[20] However, for murder, the marginal increase of illegally carrying a gun (2.5 year) pales in comparison to the magnitude of punishment (death penalty or more than 20 years imprisonment) for the crime itself. Therefore, more severe punishment for illegal possession of a gun is only expected to reduce robbery. Yet obtaining no statistical support for murder in a sense is a placebo test that suggests the results for other robbery are likely not spurious.

Another concern regarding NY's sentence enhancement is an unintended spillover effect. If longer prison time for illegal gun carrying causes potential offenders to substitute gun crime with non-gun crime, looking at only gun-related crimes will not show the complete picture. NY's law change is expected to either have a positive effect on non-firearm crime, implying a spillover effect, or should have no effect at all. If the estimates show a negative and significant impact on non-firearm crimes, it could be concluded that there exist unobservable criminogenic factors that induce a spurious correlation with crime rates. By testing for larceny, which is a non-gun crime, I also check for this possibility.

Using NY's punishment enhancement for illegal possession of a gun in a sense is analogous to add-on gun laws used in Abrams (2011) but differs in important ways. First, compared to add-on gun laws, harsher sentencing for illegal possession of a gun could potentially have a stronger effect on crime rates. Unlike add-on gun laws, according to NY's laws, whether or not an arrested

---

[20] Mendes and McDonald (2001), Lee and McCray (2009), Abrams (2011), and May (2014)

violator commits a crime with an illegal handgun, s/he still has to serve at least minimum jail time for carrying the gun (which has increased to 3.5 years instead of 1 year). This could deter criminals directly from possessing illegal guns, which, correspondingly, would lead to fewer gun-related crimes.

Second, in order to estimate the effect of punishment severity, the common approach used in the literature is simple DD methodology. In DD methodology, the basic assumption is that the control group is a good counterfactual for the treatment group. In this study, the synthetic control method is used along with DD to ensure that control states are good counterfactuals for NY. This means that absent the intervention, we would expect the same pattern of outcomes to exist over time in both group types.

2.4. Data and Background

Gun carry regulations differ across the U.S. Different states have enacted either "shall issue", "may issue", "no issue", or "unrestricted" gun carry laws.[21] Except unrestricted states, in all other states, it is illegal to carry a handgun without the required permit and violators will be punished by fines or imprisonment. Depending on state-specific regional characteristics, crime history, and government priorities, the severity of punishment for illegal handgun carrying varies across states. These punishments vary from up to a $500 fine (Oklahoma) to up to a $15,000 fine (Pennsylvania). The punishment also includes mandatory minimum imprisonment in different states, which range from 10 days (Oklahoma) to 3.5 years (NY).[22]

---

[21] *A "*May-Issue*" Law is one that requires a permit to carry a concealed handgun, and where the granting of such permits is at the discretion of local authorities (frequently the sheriff's department or police)
*A "*No-Issue*" Law is one that does not allow any private citizen to carry a concealed handgun in public
*An "*Unrestricted* gun carry" Laws are those that allow any private citizen to purchase, sell, and carry a concealed handgun in public without any restrictions. Before 2003 Vermont was the only state with No-Control law. Alaska (2003), Arizona (2011), and Wyoming (2013) switch back to unrestricted laws as well.
[22] http://www.criminaldefenselawyer.com/topics/gun-possession-and-use (Retrieved 6/17/2016)

In this study, I used the FBI-UCR dataset, which provides quarterly variation for 3 different types of crime (murder, robbery, and larceny). I removed Alabama, Colorado, Florida, Kansas, Minnesota, District of Colombia, and Vermont because the quarterly data was not available. Table 1 reports the mean of crime rates and other control variables in the analysis for both NY and other potential control states for the period of 2001-2010. Except for the robbery rate, which is relatively high in NY, the means of other crimes is relatively lower in NY than other states.

Apart from punishment severity, there are also many other factors (e.g., economic conditions or demographic information) that could possibly change the level of crime in NY. Following most of the studies in crime literature, in order to capture the impact of other factor on crime rates, I added a log of population by age, race, and sex groups as control variables to this study's models. All demographic data is obtained from the U.S. Census database. Since the quarterly data is not available for demographic variables, I used yearly measures for all demographic variables (inherently making the assumption that the quarterly variations in population are relatively small). I also controlled for unemployment rates obtained from the Bureau of Labor Statistics (BLS) in order to capture the effect of the great recession that took place in early 2008.

2.5. Methodology

As NY is the only treated state that increased the minimum jail time for illegally carrying a gun, using simple DD methodology is problematic because the control states are not good counterfactuals for NY. For instance, crime in NY is not comparable to crime trend in small states like MS, ND, SD, etc. Thus, I use the synthetic control method (Abadie and Gardeazabal 2003), which is the most appropriate model in the literature to determine a more accurate control group (i.e., the synthetic control) for NY. The synthetic control is simply a weighted average of all potential control states. These weights are chosen such that the resulting synthetic control states best reproduce the values of a set of predictors of the crime rate in NY before the change in penalty occurs in 2006.

Therefore, the synthetic control method is used as the main model to study the effect of this event. Using synthetic control states and their associated weights for each crime (reported in Table 2) I also apply the DD technique to analyze the effect of more severe punishment on crime rates as a robustness check. For this purpose, I estimate

$$CR_{syq} = S_s + Y_y + Q_q + (\mathrm{T} * \mathrm{S})_s + \beta Punish_{syq} + \lambda X_{syq} + \varepsilon_{syq} \tag{2.1}$$

Subscript "s" denotes states, subscript "y" denotes years, and subscript "q" denotes quarter. The terms S_s ,Y_y, and Q_m are the state, year, and quarter fixed effect dummy variables. I also added $(T * S)$ in order to control for state-specific time trends, where *T* is a quarterly linear time trend. While state-specific time trends likely provide the most robust estimates, I also present the evidence without state trend. The variable CR is the log of number of crimes per 100,000 for the 3 different categories of crimes that were mentioned earlier.

I use the natural log of the crime rate because it is easier to interpret the results and coefficients. Vector X contains a set of predictors of the crime rate, which is added to the model to increase the validity of estimations. The predictors of crime rate are unemployment, population density, and population by age groups, race, and sex. The variable of interest (*punish*) is a dummy that is set to one for NY after 2006 and zero otherwise. Parameter β should be negative and statistically significant for robbery, which indicates to a negative impact of more severe punishment on crime rates.

2.6. Results

As explained, the synthetic control method is being used in order to find states that most closely matched NY in terms of pre-intervention values of crime predictors. Utilizing the synthetic control method, control states and their associated weights for each type of crime separately are found and outlined in Table 2. Among these control states, California (CA), Illinois (IL), Maryland (MD), and New Jersey (NJ) are among the closest states to NY in terms of demography, unemployment rate, and crime characteristics.

Figure 1 is plotted using obtained control states and their associated weights. This figure displays the robbery trends in NY and its respective synthetic control during the period of 2001-2010. As figure makes it apparent, before 2006 the rates of robbery are very similar in both NY and its synthetic control. Right after NY raised the punishment severity in 2006, the two lines (robbery trend in NY and its synthetic control) begin to diverge noticeably. These discrepancies are indicative of the fact that the increase in punishment severity for illegal gun possession is effective in terms of crime reduction and has contributed to the decreasing crime trend in NY post 2006.

Figure 2 plots the quarterly estimate of the impacts of NY's law change on robbery rates, which is the quarterly gap in robbery rates between NY and its associated synthetic control. This figure suggests that punishment enhancement in NY had a large effect on robbery rate. According to Figure 2, the magnitude of the estimated impact of NY's punishment enhancement appears substantial and this impact increased over time.

In order to distinguish between the short-run deterrence effects of NY's harsher punishment from that of incapacitation, I use Kessler and Levitt's (1999) argument, which implies that the crime rate falls in the short run only due to the deterrence. Based on their argument, defendants subjected to the law change in NY would be imprisoned for at least 1 year even in the absence of law change. Thus, any impact on crime rates during the year after enhancing the punishment must be solely due to the immediate deterrence. As Figure 1 makes it evident, crime rates in NY fall immediately after the adoption of harsher sentences in 2006, which insinuating that a large portion of crime reduction in the short run is due to a criminal deterrence. Yet, as expected, the reduction pace increases over time as the incapacitation effect comes into play as well.

In order to determine whether or not deterrence effect still plays a substantial role in the long run, I track NY's prison population after 2006. If NY's prison population does not increase dramatically after 2006, it could be hypothesized that NY's punishment enhancement does not influence the effect of incapacitation on crime rates. If this is the case, the long-run impact of NY's harsher punishment on crime rates could also be attributed mostly to criminal deterrence. Surprisingly, NY's incarceration trend shows that in spite of existence of longer sentence time for illegal gun possession, NY's incarceration rate has been decreasing since 2006.[23] Assuming no significant change in non-gun related crimes, this observation would support the hypothesis of the long-run negative impact of criminal deterrence on crime rates. Larceny results, which is indicative of non-gun related crimes and will be explained in section 6.2, provides evidence in support of the assumption of little to no change in overall non-gun related crimes.

As of this point, findings indicate that increased punishment severity for illegal gun carrying in NY is an efficient policy since it appears to have reduced crime rates and large portion

[23] http://www.sentencingproject.org/the-facts/#map (Retrieved 6/17/2016)

of this reduction is mostly through the deterrence channel. However, some sort of significance test is required to show that these finding are not obtained by chance. To this end, in the next section I run falsification tests, which are considered significance tests for the synthetic control method in the literature.

2.6.1. Inference about the effect of the New York punishment enhancement

In the synthetic control studies, there is always the question of whether the outcomes could be driven entirely by chance. To answer this question, similar to Abadie and Gardeazabal (2003) and Bertrand, Duflo, and Mullainathan (2004), I run a placebo tests. To assess the significance of estimates, I iteratively apply synthetic control method to every state in the donor pool that did not change their punishment severity during the sample period of this study. In each iteration, I assign one of the 43 states in the former control group as a treated state as if it would have passed the punishment enhancement in 2006, instead of NY. If the placebo studies for other states create gaps similar to the one estimated for NY, it could be concluded that the analysis of this paper does not provide significant evidence to support the negative effect of NY's punishment enhancement on crime rates. If on the other hand the estimated gaps for NY is unusually larger than the gaps estimated in placebo studies for other states that did not change their law, then it could be concluded that the analysis provides significant evidence for negative effect of NY's punishment enhancement on crime rates.

Figure 3 displays the results for the placebo tests. The placebo procedure provides a series of estimated gaps for the states in which no intervention took place. The gray lines show the difference between robbery rates in each state in the donor pool and that of associated synthetic version. The black line represents the estimated gap for NY, which is also shown in Figures 2. In order to have a clean picture, I dropped states with poor pre-intervention fit. For this purpose, I calculate the pre-intervention mean square prediction error (MSPE) for all states including NY. MSPE is the average of the square discrepancies between the actual robbery rate in NY and its synthetic counterpart during the period 2001-2006. The pre-intervention MSPE for robbery in NY is about 7. I exclude all states with pre-intervention MSPE two times higher than NY's.

As Figure 3 indicates, the estimated gap for NY outsizes all the estimated gaps for other states in the donor pool during the entire post-treatment period. In both figures there are lines for few states (NM and NC) that still negatively deviate considerably from the zero after 2006. However, as mentioned, the estimated gap for NY is still noticeably larger than their estimated gap. These findings corroborate the fact that the NY results, presented in Figures 1 and 2, have not been driven by chance and the more severe punishment for illegal gun carrying is the reason that robbery rate has fallen in NY after 2006.

Following Abadie, Diamond, and Hainmueller (2009), the final way to assess the significance of the estimates is to look at the distribution of the ratios of post/pre-intervention MSEP. Figure 4 displays this distribution for robbery rate in NY and also in all other control states. NY's post-intervention MSPE for robbery is about 13 times the pre-intervention MSEP. None of the control states in the donor pool has such a large ratio. This means that if one randomly assigns the intervention in different states, the probability of obtaining a post/pre-intervention MSEP ratio as large as NY's is 1/44.

2.6.2. Robustness check

As discussed before, larceny and murder should not be affected by NY's harsher punishment for illegal gun possession. Using the synthetic control method, Figures 5 and 6 are plotted for murder and larceny. As these figures indicate, murder and larceny trends almost always follow the same trend both in NY and its respective synthetic control. Unlike robbery, they do not diverge noticeably after 2006. These findings confirm the hypothesis that the severity of punishments for illegal possession of firearms reduces the crime rate, but it only affects what prior research has identified as gun-related crimes. Obtaining no alteration for larceny rate after 2006 also suggests that criminals do not shift from gun crimes to non-gun crimes and there is no spillover effect.

Additionally, considering the states which are obtained from the synthetic method as a control states for NY, I apply the WLS method to estimate model (1) for each type of crime. In this estimation, the Cluster-Robust Variance-Matrix Estimation (CRVE) techniques are used to estimate standard deviations. Following the previous papers in crime literature, I control for unemployment rate, population density, and population by age, race, and sex groups to have more precise estimations. Results for all 4 types of crime (both with and without state-specific time trends) are reported in Table 3. While the DD's coefficient are negative for all types of crime, they are only statistically significant for robbery. The DD results verify the synthetic control method's findings, which imply that more severe punishments for illegal gun possession are effective and do reduce gun-related crimes.

2.7. Conclusion:

Using different econometric techniques, many researchers have tried to determine how more severe punishments can affect crime rates. While the majority of these studies conclude that the punishment severity plays an important role in reducing crime, not many pay attention to the distinction between deterrence and incapacitation. Using NY as a treated state, this paper studies the effect of increasing the minimum jail time for carrying a gun illegally on crime rates.

Additionally, it analyzes how much of the potential reduction could be attributed to the deterrence effect of harsher punishment.

The state of NY increased the minimum jail time for carrying loaded handguns without the required permit to 3.5 years in 2006. Findings of both the synthetic control methods and DD regressions imply that gun-related crime rates (robbery) in NY dropped after 2006. This means more severe punishment deters criminals from carrying a gun illegally, which may eventually result in lower gun-related crime rates. Findings also suggest that most of the crime reduction is due to the deterrence of law change rather that incarceration. This confirms that harsher sentences for gun law violators are economically efficient, as they could reduce the crime rate without incurring higher cost on tax payers.

2.8. References

Abadie, A., & Gardeazabal, J. (2003). The Economic Costs of Conflict: A Case Study of the Basque Country. *The American Economic Review*, 93(1): 113-132.

Abadie, A., Diamond, A., & Hainmuller, J. (2014). Conparative Politics and the Synthetic Control Method. *Midwest Political Science Association*, 00(0): 1-16.

Abadie, A., Diamond, A., & Hainmuller, J. (Jan 2012). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, 105: 493-505.

Abrams, D. S. (2011). Estimating the deterrent effect of incarceration using sentencing enhancements. *U of Penn, Inst for Law & Econ Research Paper*, 11-13.

Becker, G. S. (1968). Crime and Punishment: An Economic Approach . *The Journal of Political Economy* , 76(2) 169-217.

Decker, S. H., & Kohfeld, C. W. (1990). Certainty, severity, and the probability of crime: A logistic analysis. *Policy Studies Journal*, 19(1) 2-21.

Doob, A. N., & Webster, C. M. (2003). Sentence Severity and Crime: Accepting the Null Hypothesis. *Crime and Justice*, 30 143-195.

Helland, E., & Alexander , T. (2004). Using placebo laws to test" more guns, less crime. *Advances in Economic Analysis & Policy*, 4(1).

Huang, C., Laing, D., & Wang, P. (2004). CRIME AND POVERTY: A SEARCH-THEORETIC APPROACH. *International Economic Review*, 45(3) 909-938.

Jarrell , S., & Howsen, R. M. (1990). Transient Crowding and Crime: The More 'Strangers' in an Area, the More Crime except for Murder, Assault and Rape. *American Journal of Economics and Sociology*, 49(4) 483-494.

Kessler, D., & Steven , L. D. (1998). Using sentence enhancements to distinguish between deterrence and incapacitation. *National Bureau of Economic Research*, No. w6484.

Kim, I. J., Branson, B. L., Rasmussen, D. W., & Zuehlke, T. W. (1993). An economic analysis of recidivism among drug offenders. *Southern Economic Journal*, 169-183.

Kramer, R. C. (2000). Poverty, inequality, and youth violence. *The ANNALS of the American Academy of Political and Social Science*, 567(1) 123-139.

Lee, D., & McCrary, J. (2009). The Deterrence Effect of Prison: Dynamic Theory and Evidence. *Industrial Relations Section, Princeton University*.

Levitt, S. (1998a). Why do increased arrest rates appear to reduce crime: deterrence, incapacitation, or measurement error? *Economic inquiry 36.3 (1998): 353-372.*, 36(3) 353-372.

Levitt, S. D., & Miles, T. J. (2007). Empirical study of criminal punishment. *Handbook of law and economics*, 1 455-495.

May, D. (2014). Have sentence enhancements had a deterrent effect on crime and what are the social welfare implications of these laws?

Mendes, S. M., & McDonald, M. D. (2001). Putting severity of punishment back in the deterrence package. *Policy Studies Journal*, 29(4), 588-610.

Robinson, P. H., & Darley, J. M. (2004). Does criminal law deter? A behavioural science investigation. *Oxford Journal of Legal Studies*, 24(2) 173-205.

Trumbull, W. n. (1989). Estimations of the Economic Model of Crime Using Aggregate and Individual Level Data. *Southern Economic Journal*, 56(2) 423-439.

Figure 2.1. Trends for Robbery per 100,000 people: New york vs. Synthetic Control

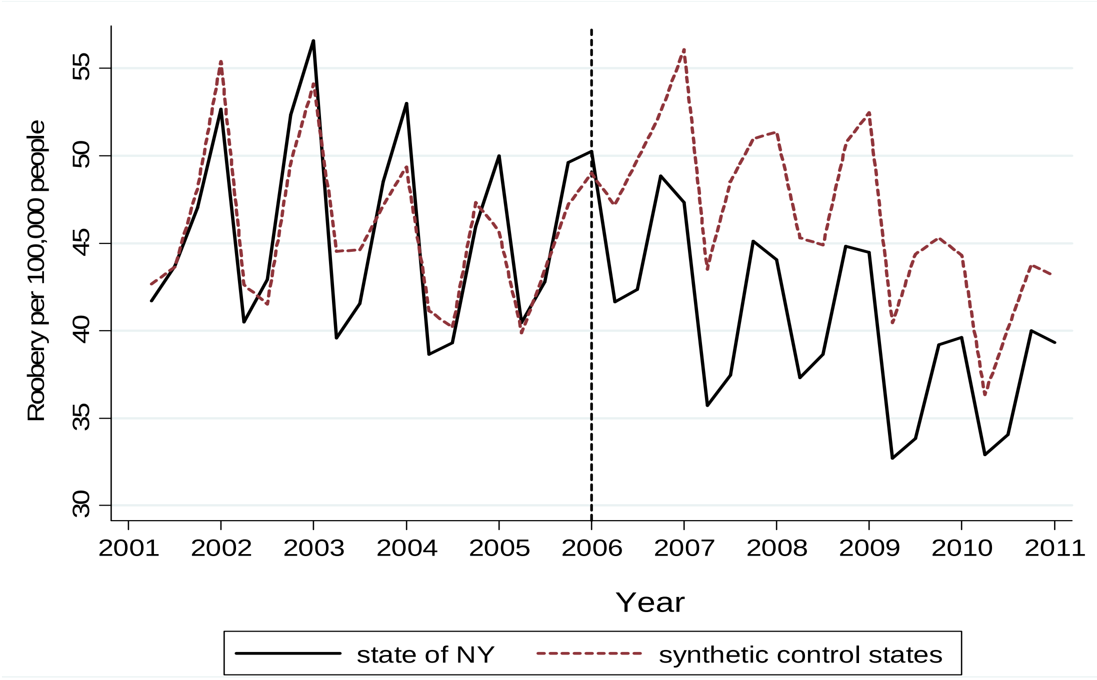

Figure 2.2. Robbery per 100,000 People: Gap Between New York and Synthetic Control

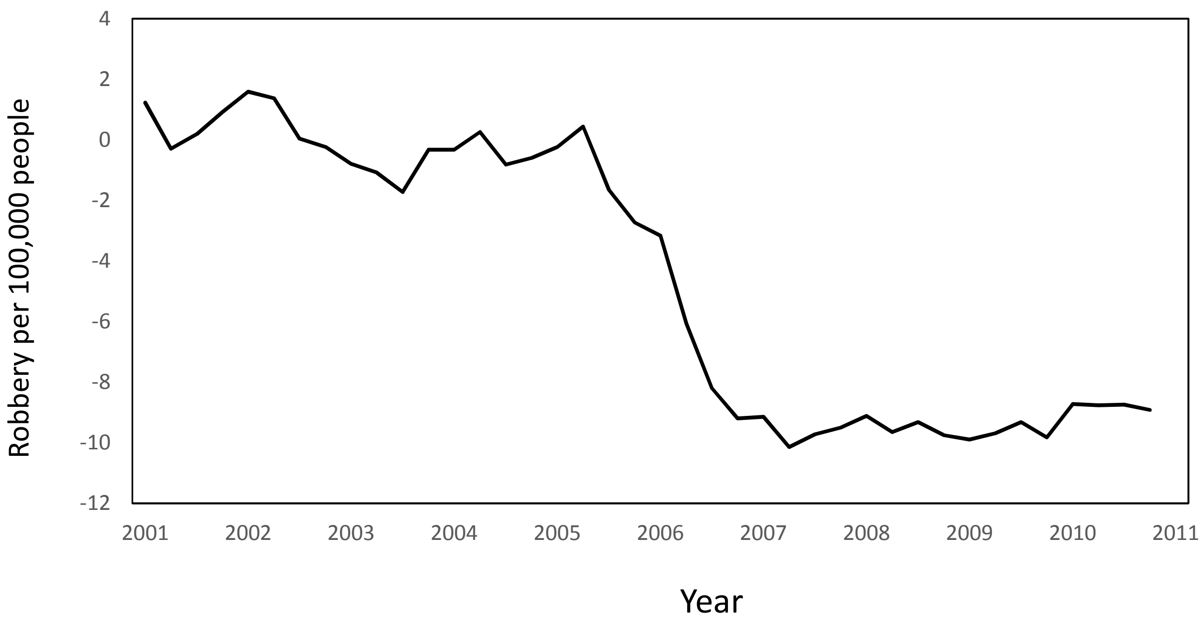

Figure 2.3. Robbery Per 100,000 People: The gray lines show gaps for placebo states. The black line represent the estimated gap for NY

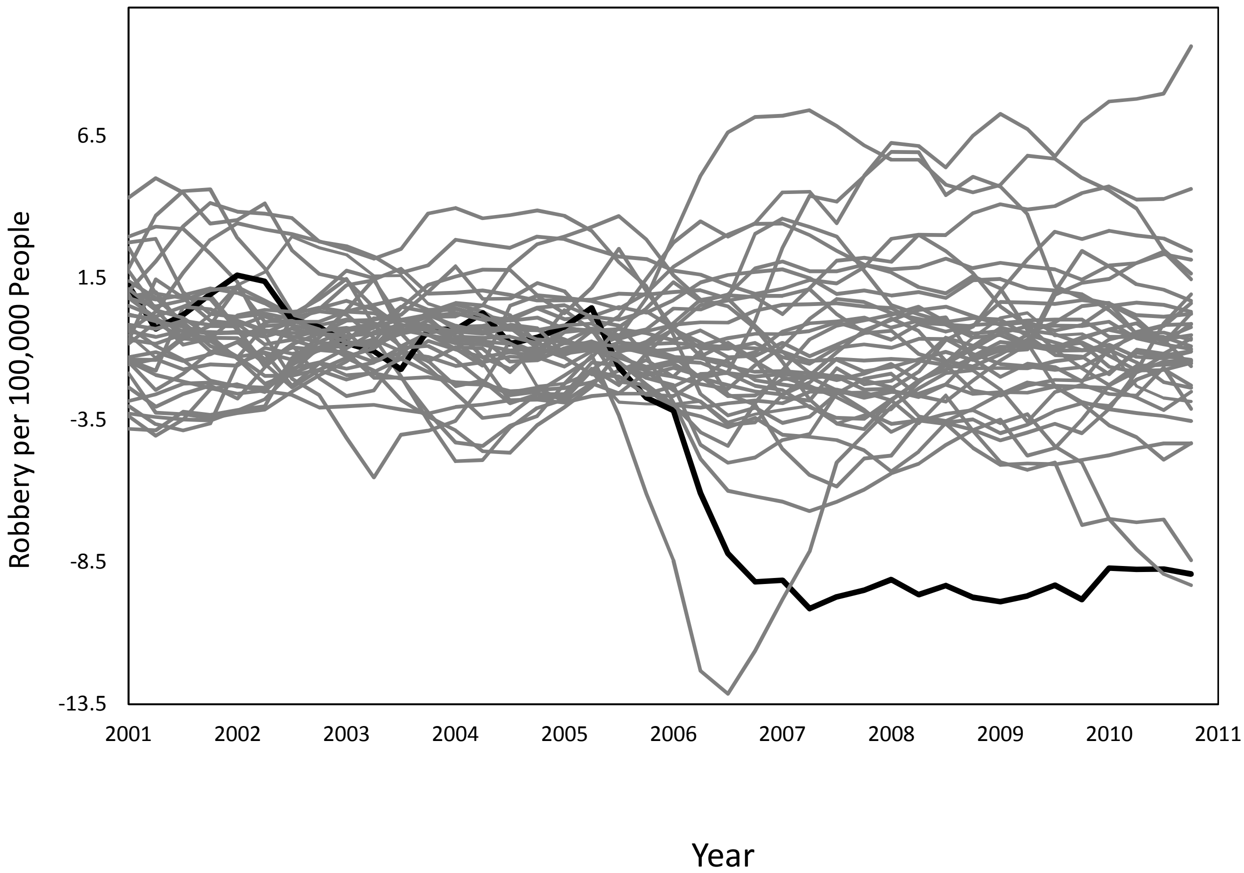

Figure 2.4. Ratio of Post-intervention MSPE and Pre-intervention MSPE for Robbery: New York and 43 Control States

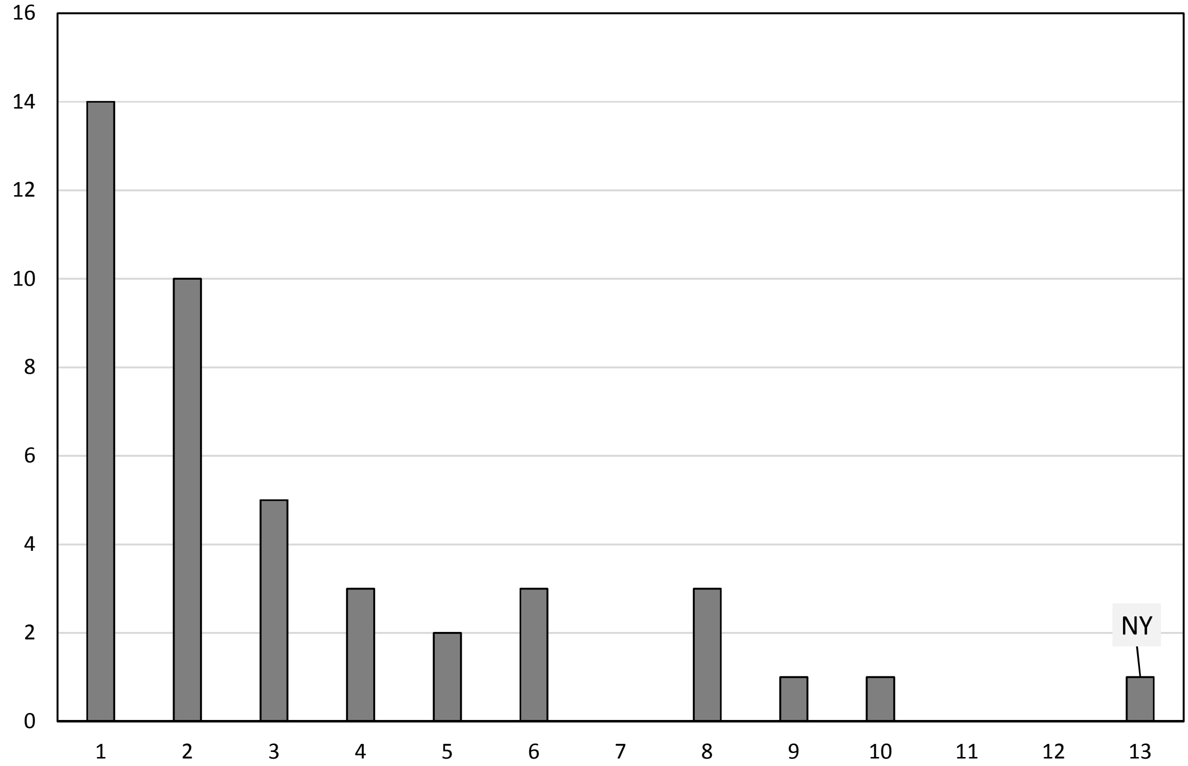

Figure 2.5. Trends for Larceny per 100,000 people: New york vs. Synthetic Control

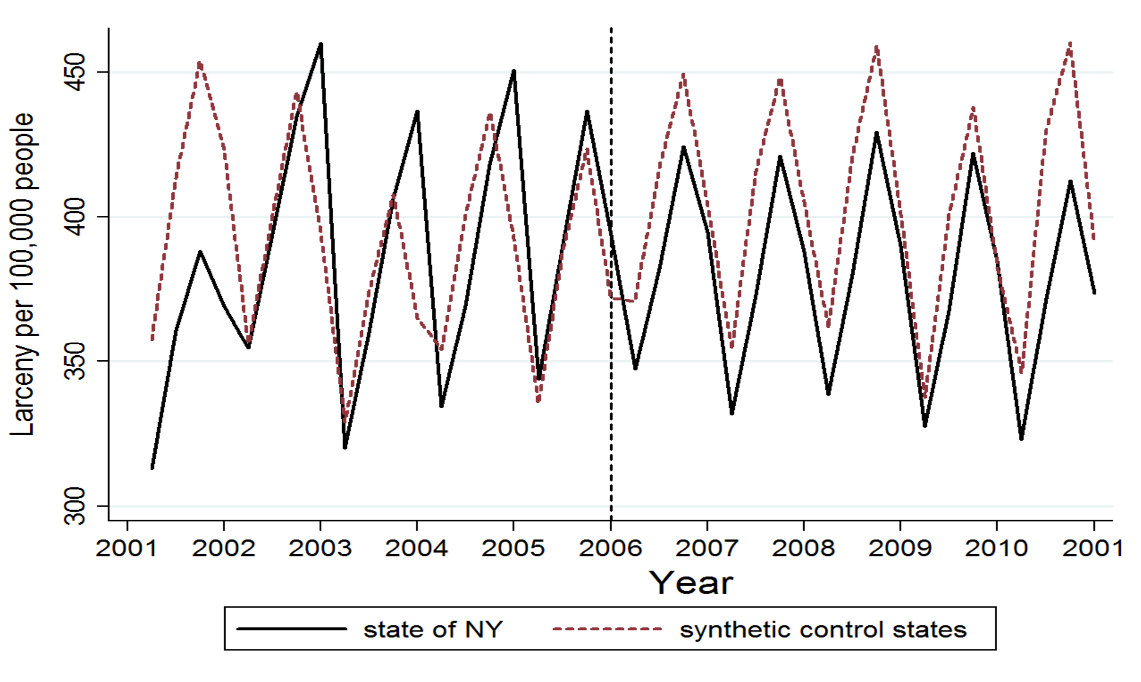

Figure 2.6. Trends for Murder per 100,000 people: New york vs. Synthetic Control

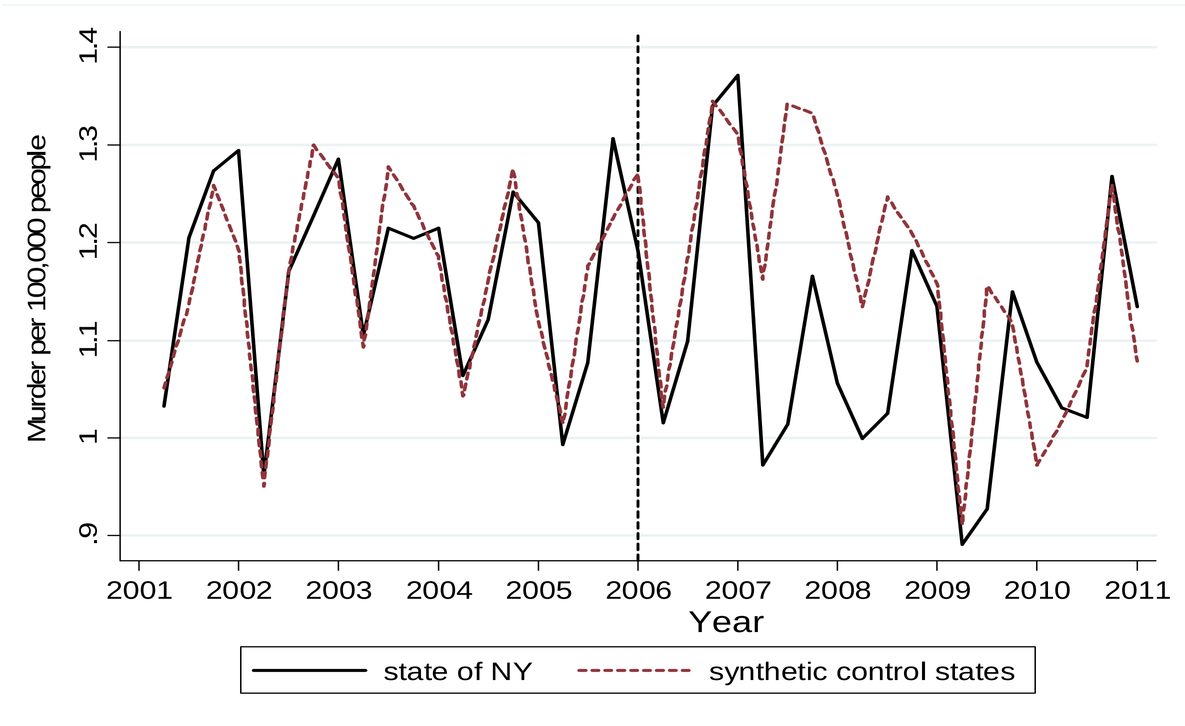

Table 2.1. Means and properties of key variable in analysis Before Treatment

Variable	N	Means for Synthetic	Means for New York
Crime rates are defined per 100,000 people:			
Robbery	1696	25.6515	39.9698
Larceny	1696	526.4328	379.2405
Murder	1696	1.1277	1.094194
Population Characteristic:			
State population	1696	5895456	19200000
Population per square mile	1696	203.8588	408.0758
Male population	1696	2905924	9301425
Female population	1696	2997458	9929717
Race Age data (%of population)			
White	1696	81.62	72.31
Black	1696	10.62	17.41
Other Race	1696	7.79	10.25
Hispanic	1696	9.43	17.11
Male 10-19	1696	7.35	6.99
Male 20- 29	1696	6.93	7.06
Male 30-39	1696	6.67	6.55
Male 40-49	1696	7.51	7.29
Female 10-19	1696	6.96	6.67
Female 20- 29	1696	6.68	7.09
Female 30-39	1696	6.67	6.80
Female 40-49	1696	7.51	7.65
Unemployment rate:	1696	5.67	6.31

Table 2.2. Synthetic control states for different types of crime

Robbery		Larceny		Murder	
State	Weight	State	Weight	State	Weight
AZ	0.04	CA	0.393	AZ	0.092
CA	0.114	GA	0.171	DE	0.039
DE	0.226	HI	0.002	GA	0.226
HI	0.010	MS	0.001	HI	0.058
MD	0.180	OH	0.173	MA	0.243
NV	0.139	PA	0.261	MS	0.063
NJ	0.211			NJ	0.209
NC	0.079			NC	0.070

Table 2.3. Effect of more severe punishments on crime rates in the state of NY (2001-2010)

VARIABLES	Robbery	Larceny	Murder
Estimation using CRVE			
Without State-specific Trend			
Punishment	-0.130**	-0.111	-0.007
	(0.049)	(0.141)	(0.013)
With State-specific Trend			
Punishment	-0.0966*	-0.078	-0.0149
	(0.0450)	(0.134)	(0.0279)
Year and Quarter Fixed Effect	yes	yes	yes
State Fixed effect	yes	yes	yes

***, **,* denote statistical significance at the 0.01, 0.05 and 0.10 levels, respectively

The treatment variable is “punishment” that equals one for the state of NY after 2006 (when NY raised the punishment for illegal gun carrying from 1 year to 3.5 years) and zero otherwise. 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.

Chapter 3 : Evaluating the Effect of Punishment Severity on Crime Rate

3.1. Introduction

The U.S. houses a greater share of its population in prison than any other country in the world. The U.S. incarceration rate was 693 per 100,000 people in 2014.[24] This rate for more than half of the countries in the world is below 150 per 100,000 and is only 114 per 100,000 people in Canada.[25] This mass incarceration is very costly and puts pressure on tax payers. The average cost of an inmate in the U.S is over $30,000 a year.[26] Additionally, negative externalities from incarceration extend to prisoners' families as well as their social networks and communities. Not only do inmates' family income fall, many of them have dependent minors who are likely to be expelled or suspended from school (Western and Pettit, 2010). Ironically, decades of mass imprisonment have coincided with the U.S. being consistently rated among the top 10 in terms of violent crime among developed countries.[27]

Punishment enhancement is a policy that has been adopted regularly by governments in order to reduce crime rates. A common conjecture is that a rise in punishment severity could reduce crime and most of the reduction is through the deterrence channel rather than incapacitation. However, there is a strong disagreement among researchers about the effectiveness of such policies as most of these policies have so far largely contributed to increases in prison population. One thing researchers do agree on is that policies that could reduce prison population without increasing the crime rate are most welcome.

[24] International Center of Prison Population
[25] http://www.prisonstudies.org/sites/default/files/resources/downloads/wppl_10.pdf (retrieved on 10/22/2016)
[26] https://www.federalregister.gov/documents/2015/03/09/2015-05437/annual-determination-of-average-cost-of-incarceration (retrieved on 10/22/2016)
[27] http://www.civitas.org.uk/content/files/crime_stats_oecdjan2012.pdf (Retrieved 10/22/2016)

To this end, some states have altered sentencing for low-level nonviolent offences (e.g., low-level drug and thefts offences).[28] Arkansas (AR), the subject of this analysis, increased the felony threshold for theft from $500 to $1000 in 2011, moving most theft cases to district court.[29] This achieved a reduction in the prison population in 2012.[30] However, AR might experience two effects. First, the lower punishment for theft might increase the incidence of that crime. Second, fewer thieves in prison might mean an increase in other types of crime (i.e. if thieves are more prone to committing other types of crime as well).

Unlike previous studies, which tend to analyze the impact of harsher sentences on the crime rate, I exploit AR's less punitive sentence for theft crimes. The findings of this paper indicate that the change led to higher theft rates in AR after 2011 but not for other crimes. These findings reveal that crime-specific punishment severity plays a pivotal role in crime prevention through deterrence. This is valuable information that could help policy makers to adopt efficient policies in order to achieve their anti-criminogenic goals. For serious and violent offences, punishment enhancement could act like preventive policies that could reduce the crime rate without putting more people in prison. For low-level offenses however, adopting less punitive senesces are far more cost-effective than putting offenders in prison.

3.2. Literature review

Becker (1968) posited that the crime rate is negatively related to the punishment severity and the probability of conviction. Both conviction and punishment could reduce crime rates through two different channels—incapacitation and deterrence. Since incapacitation is costly,

---

[28] Arkansas (2011), Kentucky (2011), Georgia (2012), California (2013)
[29] http://fivethirtyeight.com/datalab/what-to-expect-california-prop-47/ (retrieved on 10/22/2016)
[30] http://www.sentencingproject.org/the-facts/#map?dataset-option=SIR (retrieved on 10/22/2016)

crime prevention policies could be economically efficient when a sufficient number of crimes are reduced solely through deterrence.

Becker’s findings prompted a large number of academic studies to analyze the role of punishment severity on crime rates. After reviewing the existing (and conflicting) literature and undertaking their own evaluation, Mendes and McDonald (2001) argued that the severity of punishment is as important as other crime-preventing factors and does reduce the crime rate. Yet, Doob and Webster (2003) noted that all previous studies suffer from endogeneity, thereby providing limited statistical support for the deterrence effect of punishment severity.

The next wave of studies, within which this study falls, uses quasi-experimental analysis to identify deterrence effects of more severe punishment. Helland and Tabarrok (2007) use California's three-strike legislation and conclude crime rates are reduced among the class of criminals with two strikes. Lee and McCarry (2009) use the fact that offenders younger than a certain age (typically 18) are subject to less punitive sentences than adults. They claim that states with larger jumps in punishment tend to have lower adult crime rates. Abrams (2011) examines the deterrence effect of add-on gun laws that impose harsher sentences for offenders who possess firearms during the commission of a felony. He shows add-on gun laws reduce gun robberies by roughly 5 percent.

To the best of my knowledge, all previous studies investigate harsher sentences to analyze the impact of punishment severity on crime rates. A problem associated with using harsher sentences is that one could not distinguish between the deterrence effects of harsher punishment from that of incapacitation. In this paper however, AR’s milder sentence is exploited to analyze the effectiveness of punishment policies. It could be assumed that any increase on theft rates after lowering the punishment could be mostly due to the lower level of deterrence, especially if we do not see a corresponding increase in other crimes.

From a practical standpoint, the increase in crime due to AR's higher felony threshold for theft would be expected to affect larceny (bicycle theft, shoplifting, and pick-pocketing). Specifically, larceny offenders perceive the costs of stealing items between $500 and $1000 to be lower than before the threshold increase. According to this hypothesis, AR‘s 2011 adjustment of the felony threshold for theft from $500 to $1000 should lead to an increasing larceny rate, suggesting criminals respond to the reduced crime-specific cost of committing a crime.

Since incapacitation of potential criminals is likely to be reduced (fewer people would be incarcerated for theft) we might also expect to witness an increase in other types of crime in AR. However, if rates of crime unrelated to theft are unchanged after AR's 2011 law change, one could hypothesize a stronger role for deterrence over incapacitation in sentencing. To this end, I analyze the impact of AR's law change on motor vehicle theft and aggravated assault. Motor vehicle theft because a vehicles' value is almost always greater than $1000 and aggravated assault because it is completely a non-theft crime. Additionally, obtaining null results for these types of crime in a sense is a placebo test that would suggests the results for larceny is likely not spurious.

3.3. Data and Methodology

Since AR is the only treated state that increased the felony threshold for theft, using simple difference-in-difference (DD) methodology is problematic because the control states are not good counterfactuals for AR. For instance, crime in AR could not be compared to crime trends in big states like NY, IL, CA, etc. Therefore, I use the synthetic control method, which is the most appropriate model according to the literature. Using this method enables one to find a more accurate control group (synthetic control) for AR. That is, instead of comparing AR with all states that did not change their laws, AR will be only compared with one control unit (synthetic control), which is a weighted average of all non-treated states. These weights are chosen such that the resulting synthetic control best reproduces criminogenic characteristics of AR before the change in felony threshold occurs in 2011. The mathematical proof for the synthetic control group is beyond the scope of this paper (for the full proof see, Abadie and Gardeazabal, 2003).

Synthetic control is used as the main model to study the effect of AR's milder punishment on crime. Yet, as a robustness check, I also apply the DD technique to analyze the effect of less severe punishment on crime rates. For this purpose, I estimate:

$$CR_{sym} = S_s + Y_y + M_m + \beta lesspun_{sym} + \lambda X_{sym} + \varepsilon_{sym} \tag{3.1}$$

Subscript *s* denotes states, subscript *y* denotes years, and subscript *m* denotes months. The terms S_s ,Y_y, and M_m are the state, year, and month fixed effect dummy variables. Variable *X* contains a set of predictors of the crime rate, which is added to the model to increase the efficiency of estimation. The predictors of crime rate are: unemployment, population density, population by age groups, race and sex. "*Lessspun*" is the variable of interest and is set to one for AR after 2011 and zero otherwise.

In this paper, crime data (larceny, motor vehicle theft, and aggravated assault) are collected from FBI's Uniform Crime Report (UCR) dataset for the period between 2008 and 2013. I use monthly crime rates for all states where information is available. This excludes Alabama, Florida, Hawaii, Kansas, Minnesota, and New York. To predict crime trends, I follow the conventional use of the log of population by age groups, race and sex. All demographic data are collected from the U.S. Census. As monthly data are not available for demographic variables, I assume that such variation in the population is small and use yearly measures for all demographic variables. I also control for unemployment rates obtained from the Bureau of Labor Statistics in order to capture the effect of economic conditions. The mean of crime rates and other control variables used in the analysis are reported in Table 1 for both AR and other non-treated states.

3.4. Results

3.4.1 Effects of AR's increased larceny threshold

As explained, the synthetic control method, which is the most appropriate model, is used in this paper to find states that most closely resemble AR in terms of pre-intervention demography, economy and crime characteristics. The synthetic control states and their associated weights for each type of crime are reported in Table 2. Among these control states, Iowa (IA), Louisiana (LA), Mississippi (MS), Missouri (MO), Oklahoma (OK), and South Carolina (SC) are among the closest states to AR.

Figure 1 illustrates the larceny trends in AR and its relevant synthetic control states for the period of 2008-2013. Due to the noise in the monthly crime dataset, I use a moving average to smooth the crime trends. According to Figure 1, before 2011 the rates of larceny are very similar in both AR and its respective synthetic state. However, once AR raised the felony threshold for theft in 2011, AR's larceny rate diverges from that of the synthetic state and shows an increasing trend.

Figure 2 plots the monthly estimates of the impacts of AR's punishment reduction on larceny rate, which is the monthly gap in larceny rate between AR and its associated synthetic states. This figure suggests that the magnitude of the estimated impact of AR's law change is substantial and this impact increases over time. Looking at Figures 1 and 2, it could be concluded that on average, AR's milder punishment for theft increases larceny rate by approximately 6 percent.

3.4.2. Inference

Similar to other statistical models, the synthetic control studies must provide some sort of significance test to prove that the outcomes are not driven by chance. Following Abadie and Gardeazabal (2003) and Bertrand, Duflo, and Mullainathan (2004), I run falsification tests, which are considered significance tests for the synthetic control method in the literature. To assess the significance of estimates, I iteratively perform the synthetic control method for every state in the donor pool that did not change their punishment severity during the sample period of this study. In each iteration, I assign one of the 44 states in the former control group as a treated state as if it increased the felony threshold for theft, instead of AR. If the placebo studies for other states create gaps similar or bigger than the one estimated for AR, it could be concluded that there is not enough evidence to support the augmenting impact of AR's punishment adjustment on larceny rate. On the other hand, if the estimated gap for AR is unusually larger than the gaps estimated for other states that did not change their law, then it could be concluded that the analysis provides significant evidence for the impact of punishment severity on the crime rate.

Figure 3 displays the results of the placebo test. The placebo procedure provides a series of estimated gaps for AR as well as all other states that have not changed their laws. The gray lines show the difference in rate of larceny per 100,000 and its synthetic version for the states in which no intervention took place. The black line represents the estimated gap for AR, which is the line that was also shown in Figure 2. For clarity, following Abadie, Diamond, and Hainmueller (2009), states with poor pre-intervention fit were dropped. For this purpose, I calculate the pre-intervention mean square prediction error (MSPE) for all states including AR (the average of the square discrepancies between the actual larceny rate in AR and its synthetic counterpart during the period 2008-2013). The pre intervention MSPE for larceny in AR is about 0.006. I exclude all staes with pre-intervention MSPE three times higher than AR's. As the Figure 3 makes apparent, the estimated gaps for AR outsize all the estimated gaps for other states in the donor pool during the entire post-treatment period. This confirms that outcomes have not been driven by chance and the less severe punishment for low-level theft in AR is the reason that larceny rate has rose in AR after 2011.

Following Abadie, Diamond, and Hainmueller (2009), the final method of assessing the significance of the estimates is to look at the distribution of the ratios of post/pre-intervention MSEP. Figure 4 depicts the distributions for larceny rate in AR as well as in all other control states. According to this figure, AR's post-intervention MSPE for larceny is about 14 times the pre-intervention MSEP. None of the control states in a donor pool have such a large ratio. This means, if one randomly assigns the intervention in different states, the probability of obtaining a post/pre-intervention MSEP ratio as large as AR's is 1/45.

3.4.3 Robustness check

As mentioned earlier, motor vehicle theft and aggravated assault should not be affected by the rise in the felony threshold for theft in AR. Using the synthetic control method, Figures 5-6 show trends for motor vehicle theft and aggravated assault. Motor vehicle theft and aggravated assault trends in AR and in its respective synthetic states do not diverge after the law change occurs in 2011. These findings confirm the hypothesis that the deterrence role of crime prevention is more crucial than that of incapacitation. Although incarceration rates might have decreased in AR, the lower incapacitation rate does not provoke potential offenders to commit other crimes. Obtaining no significant deviation for motor vehicle theft and aggravated assault rates after 2011 also verifies that the results for larceny are not driven by chance and are likely not spurious.

Additionally, considering the states which are obtained from the synthetic method as control states for AR, I apply the Weighted Lease Square (WLS) method to estimate model (1) for each type of crime. In this estimation, the Cluster-Robust Variance-Matrix Estimation (CRVE) techniques are used to estimate standard deviations. Following the crime literature, to have more precise estimates, I control for unemployment rate, population density, and population by age, race and sex. Results for all 3 types of crime are reported in Table 3. For larceny and motor vehicle theft the DD's coefficients are positive but it is negative for aggravated assault. However, only larceny's coefficient is statistically significant. The DD results verify the synthetic control methods' findings, which imply criminals respond to the severity of crime-specific punishment.

3.5. Conclusion:

Unlike all previous studies, which use harsher sentences to determine the impact of punishment severity on the crime rate, I use AR's punitive reduction for the analysis of punishment severity. AR increased the felony threshold for theft from $500 to $1000 in 2011 to achieve a reduction in the prison population. The advantage of using milder punishment over harsher sentences is distinguishing between the deterrence and incapacitation effects of punishment severity. It could be assumed that any change in crime rates after a decrease in punishment severity (like what was done in AR), could be due mostly to a change in the criminal deterrence. That is, a decrease in punishment severity reduces the expected cost of committing low-level thefts, which will affect criminals' behavior

The findings in/of this paper imply that, even though AR's punishment adjustment led to a decrease in the prison population in 2012, it also resulted in higher larceny rates (low-level theft crime). These findings confirm that criminals respond to the crime-specific costs. Results also show that despite the likely lower incarceration rate, rates of crime unrelated to theft do not change, suggesting that criminal deterrence has a greater ability to reduce crime than incapacitation.

According to these findings, reducing punishment severity for low-level crimes is an efficient policy that could be adopted by governments in order to reduce the prison population, but comes with the added implication of increased low-level crime rates. This suggest the inclusion of other supplementary policies (e.g., higher fines, home prisoned, or mandatory rehab programs) in order to control this likely rise in the number of low-level offences.

3.6. References

Abadie, A., et al. (2014). Conparative Politics and the Synthetic Control Method. *Midwest Political Science Association*, 00(0): 1-16.

Abadie, A.,et al. (2012). Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. *Journal of the American Statistical Association*, 105: 493-505.

Abraham, D. S. (2011). Estimating the deterrent effect of incarceration using sentencing enhancements. *U of Penn, Inst for Law & Econ Research Paper*, 11-13.

Becker, G. S. (1968). Crime and Punishment: An Economic Approach . *The Journal of Political Economy* , 76(2) 169-217.

Doob, A. N., & Webster, C. M. (2003). Sentence Severity and Crime: Accepting the Null Hypothesis. *Crime and Justice*, 30 143-195.

Helland, E., & Alexander , T. (2004). Using placebo laws to test" more guns, less crime. *Advances in Economic Analysis & Policy*, 4(1).

Lee, D., & McCrary, J. (2009). The Deterrence Effect of Prison: Dynamic Theory and Evidence. *Industrial Relations Section, Princeton University*.

Levitt, S. D., & Miles, T. J. (2007). Empirical study of criminal punishment. *Handbook of law and economics*, 1 455-495.

Maltz, M. D. (2003). Cluster-sample Methods in Applied Econometrics. *American Economic Review*, 93: 133-138.

May, D. (2014). Have sentence enhancements had a deterrent effect on crime and what are the social welfare implications of these laws?

Robinson, P. H., & Darley, J. M. (2004). Does criminal law deter? A behavioural science investigation. *Oxford Journal of Legal Studies*, 24(2) 173-205.

Silvia, M. M., & McDonald, M. D. (2001). Putting severity of punishment back in the deterrence package. *Policy Studies Journal*, 29(4), 588-610.

Western, B., & Pettit, B. (2010). *Collateral costs: Incarceration's effect on economic mobility.* Washington, DC: The Pew CharitableTrusts.

Figure 3.1.Larceny Trends: Arkansas vs. Synthetic States

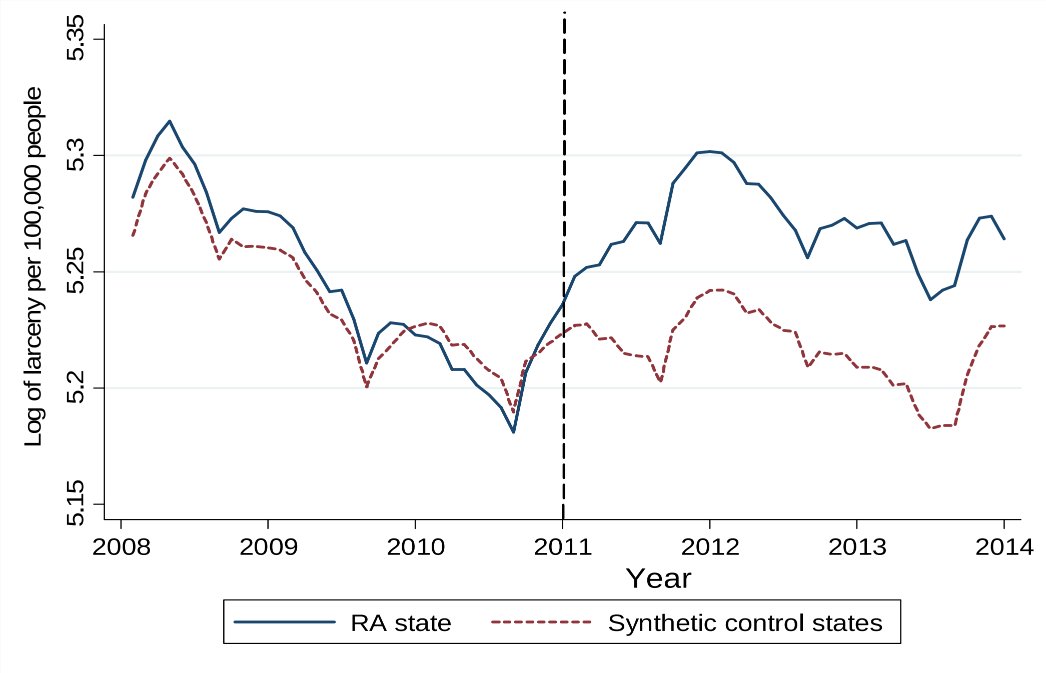

Figure 3.2. Larceny per 100,000 People: Gap Between Arkansas and Its Synthetic Control

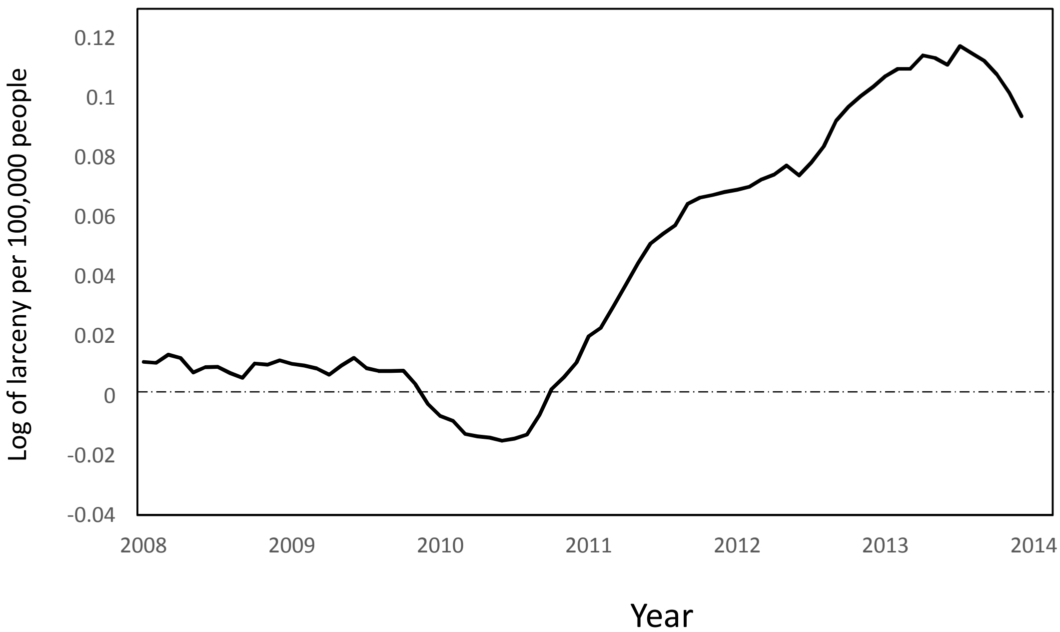

Figure 3.3. Larceny per 100,000 People: Gap for all states including AR (24 states)

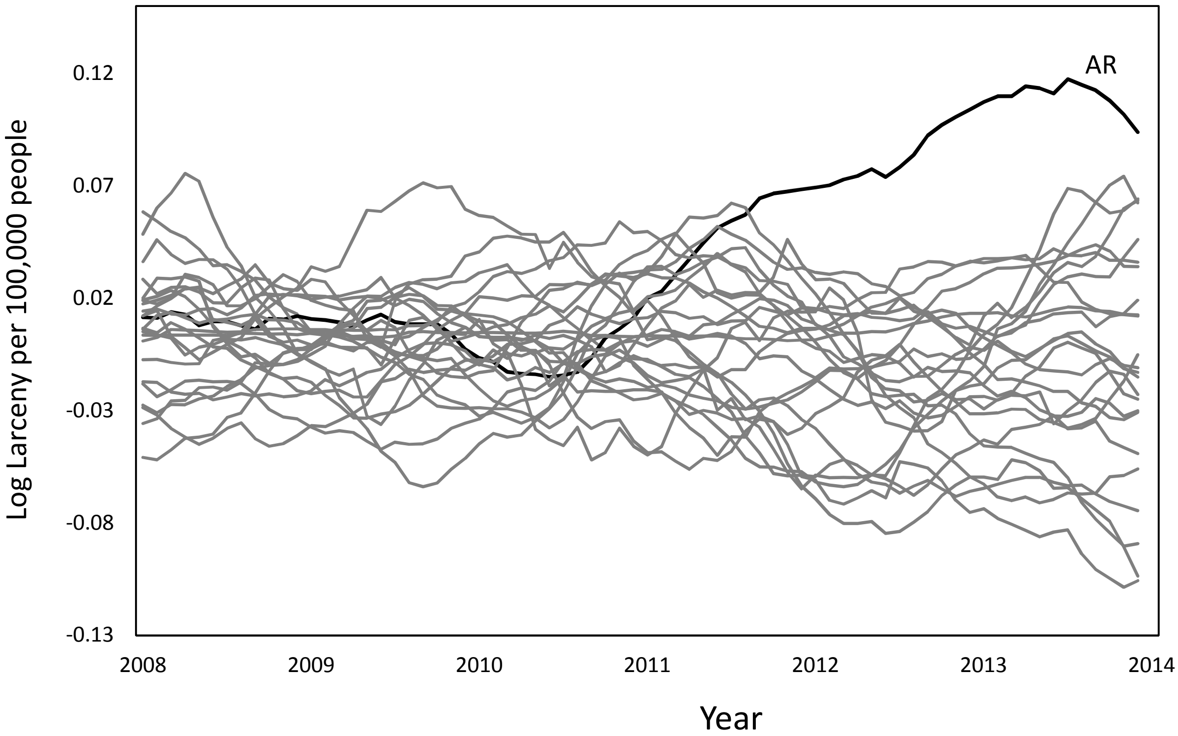

Figure 3.4. Ratio of Post-intervention MSPE and Pre-intervention MSPE for Larceny: Arkansas and 44 Control States

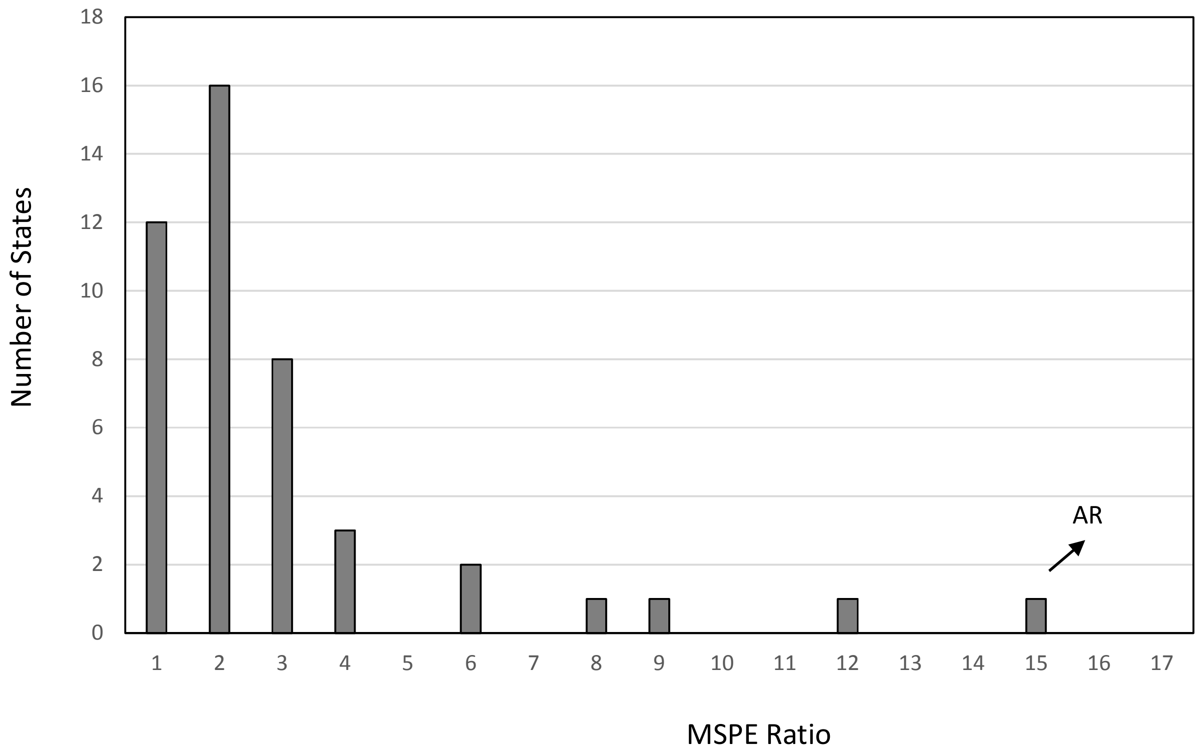

Figure 3.5. Vehicle-Theft Trends: Arkansas vs. Synthetic States

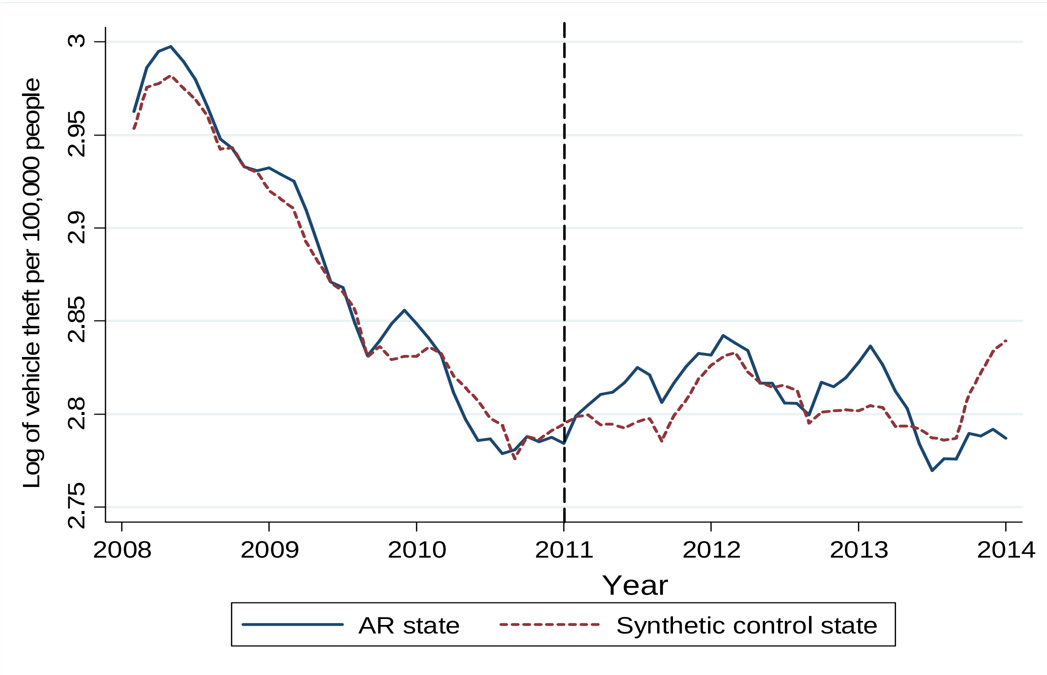

Figure 3.6. Aggravated Assult Trends: Arkansas vs. Synthetic States

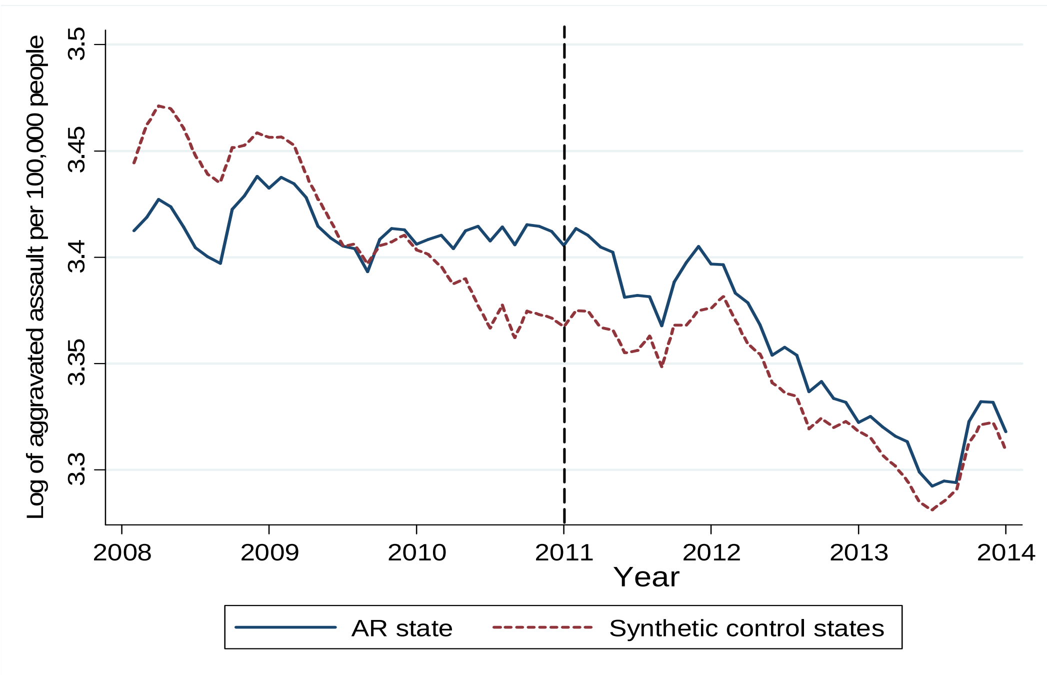

Table 3.1. Means and properties of key variable in analysis Before Treatment

Variable	N	Means for Synthetic states	Means for Arkansas
Crime rates are defined per 100,000 people:			
Larceny	3240	162.46	187.99
Motor Vehicle theft	3240	20.07	16.74
Aggravated Assault	3240	20.23	29.31
Population Characteristic:			
State population	3240	5722346	2900182
Population per square mile	3240	414.0256	55.73
Male population	3240	2845624	1423539
Female population	3240	2904889	1476643
Race Age data (%of population)			
White	3240	81.50	80.44
Black	3240	11.23	15.58
Other Race	3240	7.38	3.98
Hispanic	3240	10.41	6.20
Male 10-19	3240	7.11	7.04
Male 20- 29	3240	7.12	6.80
Male 30-39	3240	6.48	6.30
Male 40-49	3240	7.06	6.66
Male 50-64	3240	9.51	9.12
Male over 64	3240	5.71	6.21
Female 10-19	3240	6.72	6.73
Female 20- 29	3240	6.85	6.73
Female 30-39	3240	6.38	6.28
Female 40-49	3240	7.10	6.78
Female 50-64	3240	9.84	9.67
Female over 64	3240	7.37	8.10
Unemployment rate:	3240	7.59	6.96

Table 3.2. Synthetic control states and their associated weights

Larceny		Motor Vehicle Theft		Aggravated assault	
State	Weight	State	Weight	State	Weight
AZ	0.027	IA	0.286	DC	0.013
DE	0.093	MS	0.090	MO	0.202
LA	0.132	MO	0.027	NM	0.215
MO	0.028	NC	0.194	OK	0.347
OK	0.191	OK	0.363	SC	0.080
SC	0.485	SC	0.040	TN	0.034
WV	0.043			UT	0.025
				WV	0.084

Table 3.3. Effect of punishment severity using synthetic control results

VARIABLES	Larceny	Motor Vehicle Theft	Aggravated Assault
Lesspun	0.109***	0.018	-0.021
	(0.0110)	(0.0325)	(0.0160)
Observations	576	504	648

***, **,* denote statistical significance at the 0.01, 0.05, and 0.10 levels, respectively

CURRICULUM VITAE

Mehdi Barati **January 2017**

FIELD Of INTEREST

Primary: Applied Microeconomics, Labor Economics, Health Economics, Public Policy
Secondary: Applied Econometrics, Law and Economics

EDUCATION

Ph.D. (Economics), University of Wisconsin – Milwaukee, Jan/2017

Master of Science (Economics), University of Tehran, Tehran, Iran, 2010

Bachelor of Science (Economics), Shahid Beheshti University, Tehran, Iran, 2008

RESEARCH

***Peer-reviewed Publication*:**

"New evidence on the impact of concealed carry weapon laws on crime" (2016).*International Review of Law and Economics 47(2016), 1–8*

"Extended perioperative antibiotic coverage in conjunction with intraoperative bile culture, decrease infectious complications after pancreaticoduodenectomy" (2016). *(With Amir H. Fathi, Terence Jackson, Babak Eghbalieh, Kelly A. Siegel, and Christopher T. Siegel) HBP Surgery Vol (2016), Article ID 3031749*

"Validation and testing of the AquaCrop model under full and deficit irrigated wheat production in Iran" (2011).*(with B. Andarzian, M. Bannayan, P. Steduto, H. Mazraeh, A. Rahnama, and M.A. Barati) Agricultural Water Management, 100 (1), 1-8*

"Investigation of Replacing the Oil's Revenues from Dollar to Euro" (2011). *(With Mohsen Mehrara and Farkhondeh Jabalameli)The Journal of Economic Policy, Vol 2(4)*

Working papers:

"The deterrence effect of more severe punishments for illegal gun carrying on crime rate" (2016). *Under Review, Journal of Quantitative Criminology*

"Factors predicting colorectal liver metastasis recurrence after metastasectomy in patients with synchronous liver metastases from colorectal cancer" (2016). *(With Dr. Amir Fathi)*

"Evaluating the deterrence effect of punishments severity" (2016).

TEACHING INTEREST

Microeconomics, Macroeconomics, Labor, Health, Public Economics

TEACHING EXPERIENCE

Lecturer, University of Southern Mississippi, Department of Economics
Principal of Economics, 2 sections, 2016-current
Principal of Microeconomics (online), 2 section, 2016-current
Principal of Microeconomics (online), 1 section, 2017

Lecturer, University of Wisconsin-Whitewater, Department of Economics
Business Statistics, 2 sections, 2015
Principle of Microeconomics, 2 sections, 2016

Instructor, Department of Economics, University of Wisconsin-Milwaukee
Principle of Macroeconomics, 4 sections, 2011-2013
Economic Statistics, 1 section, 2013
Economic Employment and Labor Relations, 2 sections, 2013-2014
Introductory Economics, 2 sections, 2014-2015

Teacher Assistant, Department of Economics, University of Wisconsin-Milwaukee
Economics statistics (Online), 4 sections, 2012- 2015
Research Methods for Economics (Econometrics), 4 sections, 2012-2015

HONORS AND AWARDS

Seed Grant, California State University, Fresno, 2016
Robotic Research Grant, Intuitive Surgical operations, INC., 2015
Graduate Student Teaching Assistantship, UW-Milwaukee, 2011-2015
Best Master Thesis Award from the Institute for International Energy Studies (IIES), IRAN, 2010
Shahid Beheshti University, Undergraduate Fellowship, 2001-2007

COMPUTER SKILS

STATA, E-Views, R, MS-Office

Reference
Professor Scott Adams Email: sjadams@uwm.edu