ERASMUS UNIVERSITY ROTTERDAM

Erasmus School of Economics

The effect of universal background checks on gun violence in the United States.

- Is there a smoking gun? -

Master Thesis Policy Economics

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Abstract

This paper evaluates whether a causal relation exists between the number of homicides in the United States and the implementation of universal background checks that have been implemented in 20 US states and the District of Columbia. By using a fixed effects estimation and a set of control variables it will be tested whether the number of homicides per 100,000 people per state and per year differs for states that have implemented universal background checks compared to states that did not. The observed period is 2005 to 2019, which is over 10 years after the federal Brady act was signed into law. The data is coming from the FBI Uniform Crime Reporting Program (UCRP). An additional regression model will be executed to see whether a change in treatment status in a neighboring state effects the number of homicides in a state that has already implemented universal background checks. The results show a positive and significant relation between the implementation of universal background checks and the number of homicides. The implementation of a universal background check policy is associated with 0.5 additional murders for every 100,000 persons per year.

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1. Introduction

Gun control is a controversial topic in the United States, both in society and in academics. In 2003 there were already around 300 different federal and state laws on the purchase and possession of guns, and it is only counting (Cook & Ludwig, 2003). However, the question which of these laws are effective and to what extent cannot be answered precisely, although many estimations have been made.

In 1994 the federal Brady-act was signed into law. This law requires background checks for all purchases through Federal Licensed Firearm Dealers (FFL). A large part of gun sales, however, does not take place through licensed dealers but through private dealers, which do not have to perform a background check according to the Brady act. Gius (2018) estimates that around 40% of gun sales are done through private dealers. Federal background checks are executed by the FBI or happen through the National Instant Criminal Background Check System (NICS).

States can demand stricter gun policies, including additional background checks for private sellers. These are called universal background checks. Universal background checks can be performed in the same way as the federal background checks but can also be performed by the state's Department of Justice (DOJ). 20 states and the district of Colombia have passed such legislation. In the states in Table 1 and Figure 1 background checks are mandatory for all gun sales.

From Table 1 and Figure 1 it can be concluded that most of the 20 states that switched gun policies in the years under evaluation switched between 1991 and 2020. Two states, Massachusetts and New Jersey, implemented universal background checks much sooner than the other states, namely in 1927 and 1969 respectively.

In addition, it has been suggested that states with strict gun rates experience more import of guns from states with less strict gun laws (Knight, 2013). This effect may even hold internationally (Chicoine, 2011). Knight (2013) finds that gun trafficking is decreasing in distance between states.

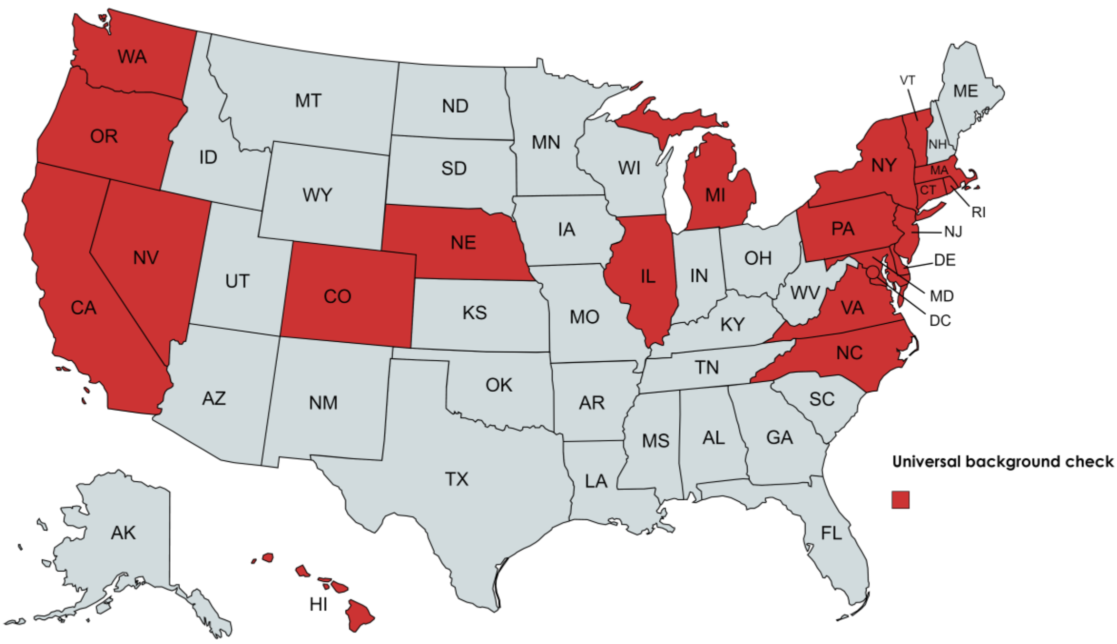
This study will investigate whether a causal relationship exists between the implementation of private background checks on a state level and the level of homicides in a state. Additionally, it will be checked whether the implementation of background checks in a neighboring state changes the number of homicides in a state that has already implemented universal background checks. In the first section a literature review is presented to summarize the relevant literature so far, and the methods they used. Then, empirical research will be conducted to assess whether a causal effect exists in this context.

Table 1. *States that implemented universal background checks and the year of implementation*

State	Implementation universal background checks
California	1991
Colorado	2013
Connecticut	2014
Delaware	2013
Hawaii	2016
Illinois	2013
Maryland	1996
Massachusetts	1969
Michigan	2000
Nebraska	1991
Nevada	2020
New Jersey	1927
New York	2013
North-Carolina	1995
Oregon	2015
Pennsylvania	1997
Rhode Island	1990
Vermont	2018
Virginia	2020
Washington	2014

Source: RAND Corporation. Obtained via: https://www.rand.org/research/gun-policy/law-navigator.html

Figure 1. *States that implemented universal background checks until 2020.*



2. Literature review

There have been many academic publications on the topic of gun control policies and their effects. The estimations of these studies differ, and the definition of what type of policy was reviewed exactly is not clear among all studies as well.

Most studies use the number of homicides in a state as the dependent variable in their estimation (Cook, 2003; Gius, 2015; Chicione, 2011). Some scholars use a broader definition of gun related crime, such as suicides and violent burglaries (Fleegler, 2013; Cook, 2000). The perk of using the number of homicides instead of all gun related crime is that better records are kept of homicides, both in numbers and way of killing. However, this can also be misleading, since one large massacre would have a larger effect than a single murder, although this can be done with the same gun.

Many scholars use a form of OLS regression for their estimation (Gius, 2013 & 2018; Knight, 2013). Fleegler (2013) and Ludwig & Cook (2000) use Poison and weighted least squares, respectively. In a 2003 study Ludwig and Cook use a difference-in-difference model for the period immediately following the implementation of the brady act (1994-1997). Some scholars also conducted secondary research to give an overview of the effects of various sorts of gun policies (Sen and Panjamapirom, 2012; Cornell, 2019). These studies both predict negative effects from universal background checks. These results are however not supported yet by quantitative studies.

Frequently used control variables in a (fixed effects) regression are race (Moody & Marvell, 2009; Gius, 2015; Sumner et al., 2008; Pelli, 2016), what political party is currently in charge (Pelli, 2016) and the unemployment rate (Moody & Marvell, 2009; Gius, 2015; Pelli, 2016; Knight, 2013). Income and alcohol consumption have also appeared to have significant effects on the number of killings (Gius, 2015). These variables are believed to be of influence on the number of homicides in a state and can therefore be relevant in the estimation of the effect of gun policies on the number of homicides.

The large plurality in gun laws can also be seen in the existing literature. Most relevant studies are post-Brady and check for the effects of the Brady-act. This law, however, still has a large loophole, since private sales can still be made without background check. So far, states have found different possible remedies. Apart from background checks, there are also concealed carry permits, child-access-prevention-laws, firearm and ammunition taxes, and countless others (Cornell, 2019).

When measuring the effects of the Brady act, Cook and Ludwig (2000) use states that already implemented background checks for licensed dealers as control states, and states that didn’t have a background check requirement pre-Brady as treatment states.

Fleegler (2013) and Knight (2013) use different scores that show how strict gun laws are in place in a state. Both authors used scores assigned by the Brady campaign, a non-governmental organization that advocates informed gun policy. Lower scores are affiliated with the federal minimum legislation, and a score of five represents a wide range of gun control policies, including universal background checks on all gun sales.

Gius (2015 & 2018) uses dummy variables for three different levels of state-required background checks: only through licensed sellers or through both licensed and unlicensed dealers.

All studies control in some way for common trends and typical characteristics. Cook and Ludwig (2000 & 2003) and Gius (2014, 2015 & 2018) use a fixed effects model to account for underlying differences between states. Cook and Ludwig also point out that the United States experiences a major crime drop in the 1990’s as the result of several nationwide events, such as abortion legalization and a narcotics prevention program.

Gun policy is not established overnight, and Americans who are anticipating on such a policy tend to anticipate on its implementation (Jones et al., 2015). In their study, Jones et al. (2015) found that when stricter gun policies are announced gun and ammunition purchases surge.

When states differ in how restrictive their gun policy is, this may lead to gun trafficking from states without universal background checks to states with universal background checks (Webster et al. (2009). This has been studied by Knight (2013), who finds that gun trafficking occurs from states with a non-restrictive gun policy to states with a restrictive gun policy. The extent of which gun trafficking occurs is decreasing in distance and primarily occurs between neighboring states.

3. Data & Methods

In this paper a fixed effects regression will be used to evaluate the effect of universal background checks on homicides in the United States. This is in line with previous research, as shown in the previous section. The fixed effects regression will be executed using yearly panel data on the number of homicides per year in each US state. The dataset will also contain a dummy whether a state has universal background checks instated. Different sets of control variables will be added to check the robustness of the results and/or to learn about the underlying mechanisms.

The following equation will be estimated to measure the effect of universal background checks on homicides:

$$Y_{i,t} = \beta_0 + \beta_1 UBC_{i,t} + \beta_2 UBC_{i,t-s} + \beta_3 UBC_{i,t+s} + \sigma_i + \tau_t + \beta_{i,t}\boldsymbol{X} + \varepsilon_{i,t} \qquad \textbf{(1)}$$

The variable UBC denotes a dummy variable that takes value 1 for each year that a universal background policy is in place in state ***i***. σ_i represents state fixed effects and τ_t the time fixed effects. The estimation contains a vector of control variables denoted by $\boldsymbol{X}$**.** The different control variables will be discussed later in this section.

As mentioned before, gun trafficking may emerge if a state has stricter gun laws than another state. Knight (2013) found that gun trafficking is more likely for states that are located near each other. As an additional method to test the effect of universal background checks it is evaluated whether the number of homicides in a state change when a neighboring state implements universal background checks.

Table 2 gives an overview of all treatment states and what direct neighboring states also implemented universal background checks, and the timing of the implementation. To measure the potential effect of a change in treatment, whether a neighboring state implements universal background checks will be added as a dummy variable in equation 1. This dummy variable will take value 1 when a state has at least one neighbor that has implemented universal background checks. Potential interaction effects between the effect of universal background checks in state A and the implementation in neighboring state B will also be evaluated by adding a model that includes an interaction term.

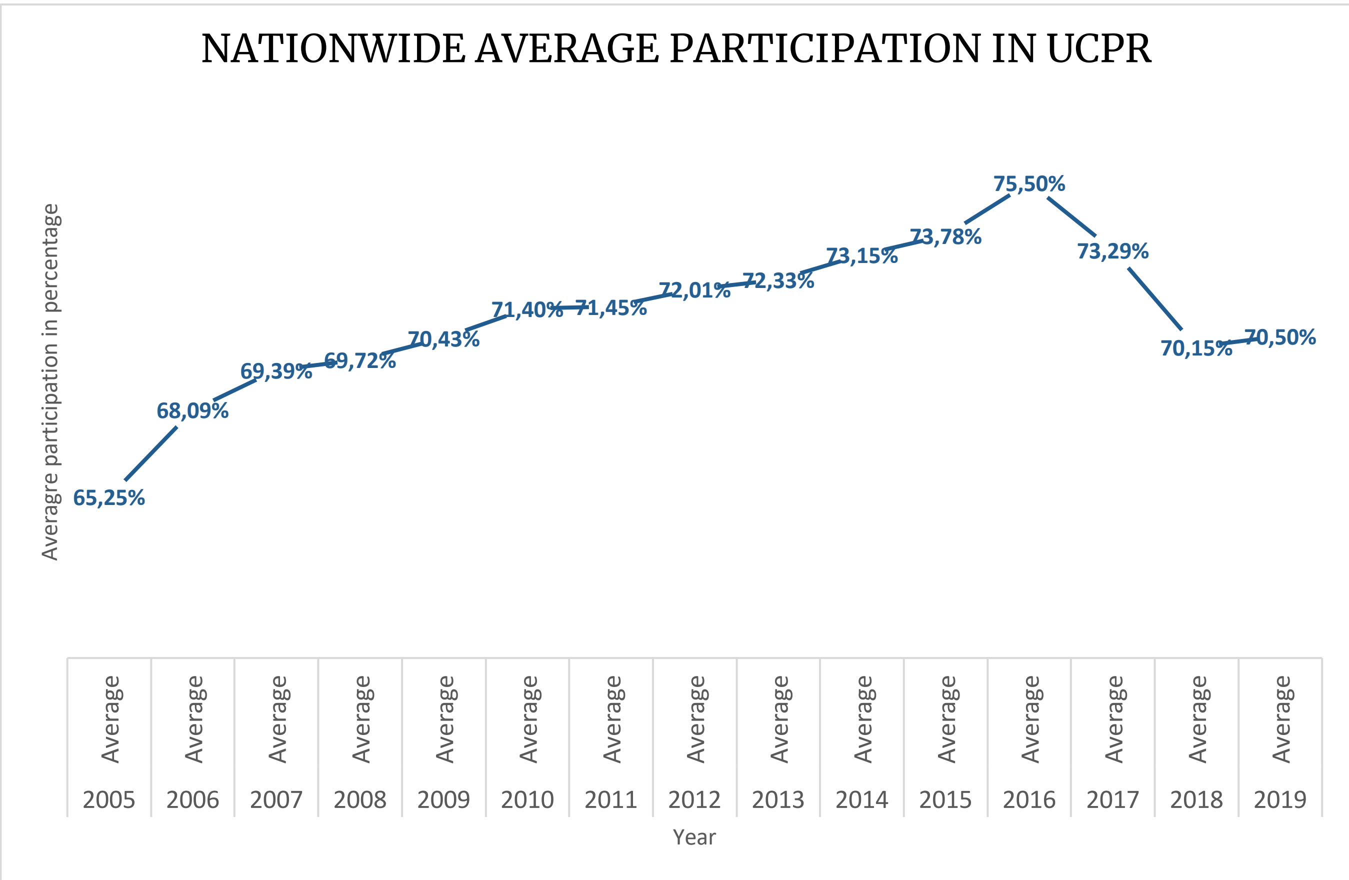
Table 2. *Overview of states and their direct neighboring states that implemented universal background checks.*

State	Implementation year	Neighboring states	Implementation year
California	1991	*Nevada*	2020
		Oregon	2015
Colorado	2013	-	
Connecticut	2014	*Rhode Island*	1990
		Massachusetts	1969
		New York	2013
Delaware	2013	*Maryland*	1996
		Pennsylvania	1997
		New Jersey	1927
Hawaii	2016	-	
Illinois	2013	-	
Maryland	1996	*Pennsylvania*	1997
		Delaware	2013
Massachusetts	1969	*New York*	2013
		Vermont	2018
		Rhode Island	1990
		Connecticut	2014
Michigan	2000	-	
Nebraska	1991	*Colorado*	2013
Nevada	2020	*California*	1991
		Oregon	2015
New Jersey	1927	*Pennsylvania*	1997
		New York	2013
		Delaware	2013
New York	2013	*New Jersey*	1927
		Pennsylvania	1997
		Connecticut	2014
		Massachusetts	1969
		Vermont	2018
North-Carolina	1995	*Virginia*	2020
Oregon	2015	*California*	1991
		Nevada	2020
		Washington	2014
Pennsylvania	1997	*New York*	2013
		Maryland	1996
		New Jersey	1927
Rhode Island	1990	*Massachusetts*	1969
		Connecticut	2014
Vermont	2018	*New York*	2013
		Massachusetts	1969
Virginia	2020	*North-Carolina*	1995
		Maryland	1996
Washington	2014	*Oregon*	2015

Source: Rand Corporation. Obtained via: https://www.rand.org/research/gun-policy/law-navigator.html

The data on homicides comes from the Uniform Crime Reporting Program (UCRP) by the Federal Bureau of Investigation (FBI). Local law enforcement agencies report relevant crimes and information on the different types of crime, victim, and offender. This includes the data on homicides in each of the 50 states. The database was accessible between 2005 and 2019. Before 2005 the UCRP only publishes averages, which cannot be used for this study. The UCRP collects data from local law enforcement on local, state, tribal and federal level. Law enforcement agencies submit this data voluntarily. The number of participating agencies differs per year but has been between 11,500 and 16,500 between 2005 and 2022. The total number of law enforcement agencies varies around 18,500, with 18,806 at the latest count in 2022 (FBI, 2022). The average participation rate across all states between 2005 and 2019 is 68,76%. The average number of participating agencies is presented in Figure 2. Although this number is substantial, it does not say anything about the distribution of these participating agencies. Therefore, a control variable containing the percentual participation per state and per year should be added to the regression.

Figure 2. *Nationwide average participation in UCPR*



The dataset does not contain data for the state of Florida. The data for the number of homicides in Florida is obtained via the Florida Health Service (Florida Health Service, 2023). The dataset contained one outlier in 2011 for the number of homicides in California. The total number of murders, which is usually around 200 people per year for the state of California, suddenly dropped to 2 in 2011, and returned to a more normal value in 2012. Since no sensible explanation could be found for this outlier, the observation was deleted. No other alterations were made to the data.

Data on the gun policies of states comes from the RAND corporation, a former government thinktank that went private as an NGO. The data on state's gun policies is obtained from RAND's *Gun Policy in America Initiative,* which aims to establish a factual basis on which debates on gun policies can be based.

Several control variables will be added to test the robustness of the models. In line with previous research, it will be tested whether the political party in charge in a state influences the number of homicides. This effect is measured by the variable democrat, which takes a value of 1 if a state is governed by a democratic governor and a value zero if the state has a republican or third-party governor. Third party governors, however, very rarely occur. Local law enforcement offices voluntarily submit data to the UCR. The variable participation contains the percentage of agencies that participated for each year in the dataset. This variable will be added as a control variable.

The model will also include anticipation effects up to three years before treatment. Similarly, it will be tested whether there was a delayed effect several years after the treatment. Different lengths for the delay and anticipation will be tested after which a preferred model will be selected. The variable measuring the delayed treatment effect shows if and to what extent there is a stronger treatment effect *t* years after the policy was implemented than in other years after the implementation.

As mentioned, a delayed treatment effect up to three years is implemented in the model. In the robustness check section, it will be checked whether including a longer time frame changes the results.

Many scholars of different disciplines disagree about the usage of lagged dependent variables in panel data regressions. While many academics believe adding lagged dependent variables can bias the estimates (Plümper, 2005; Nickell, 1981; Kievit et al., 1993), some argue that lagged dependent variables can prevent autocorrelation from biasing the estimates (Wilkins, 2018; Keele & Kelly, 2006). Opponents of the use of lagged dependent variables claim that adding the lagged dependent variables lead to an underestimation of the evaluated policy (Plümper, 2005). This could be worthwhile if autocorrelation is a plausible concern in the estimation. Previous papers on the same topic as this one refrained from the use of lagged dependent variables. In order to keep the results of this analysis comparable to earlier outcomes the main model of this paper will not include lagged dependent variables.

Previous research by Gius (2015) indicates that adding other types of gun control policies as control variables has significant effects. However, the variety in the timing of implementation is very low for the most common policies, such as concealed carry permits, child-access-prevention-laws and firearm and ammunition taxes. Therefore, the addition of these policies as controls would not matter in a fixed effects analysis.

In Figure 3 the total murders in all treatment states that change from the control group to the treatment group is plotted. The year of treatment is displayed by a vertical line. The total murders for the United States and the treatment and control group are shown in the last plot. The second Figure shows the total murders per 100,000 people in all states, regardless of their treatment status. Table 3 shows the number of democratic governors, which serves as a control variable to check the political preference for a state.

3.1 Definition universal background check

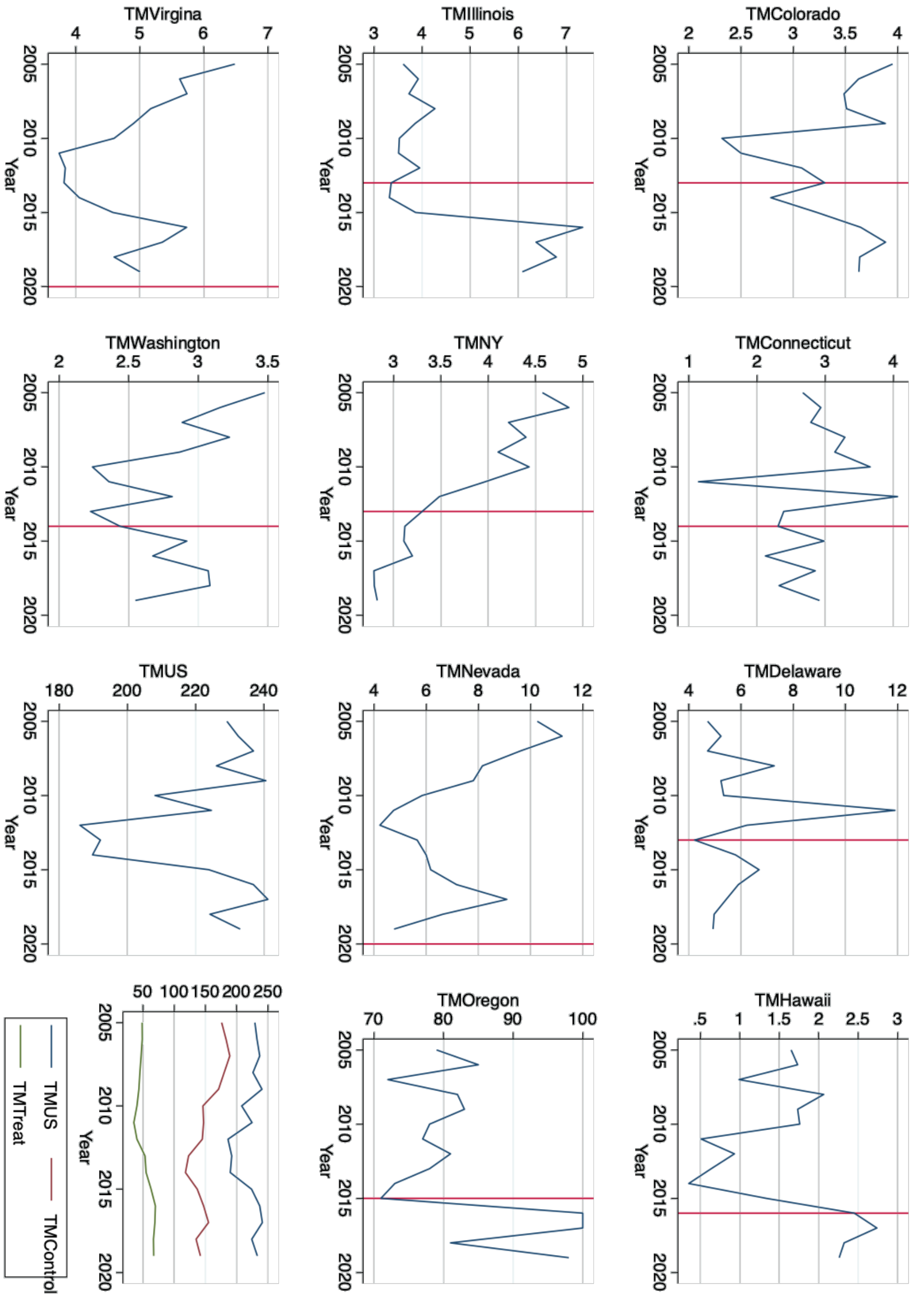
For this research it is critical to define the treatment specifically. Universal background checks are considered as treatment in this study. Universal background checks are background checks that are being performed every time a gun is transferred to a new owner. This can be both through licensed dealers and through private sales.

Universal background checks appear in two ways. The first method is to perform a background check every time a gun is purchased. The second method is a background check that is performed when a potential buyer requests a permit to purchase. In states with such a policy, guns can only be bought by a permitholder. Both these methods will be considered as a universal background check for the purpose of this research.

4. Summary statistics

Figure 3 visually presents the development of the total number of homicides in treatment states that implemented an UBC policy between 2005 and 2019. The red line is the year of implementation. The variance in implementation years will be exploited to execute a fixed effects regression. The last graph displayed in Figure 3 shows the total development for the treatment group, and control group and the entire United States.

Figure 3. *Total murders per 100,000 citizens for all states that implement universal background checks during period of observation, as well as the development for the entire US and the treatment and control group.*



The fourth Figure displays the development of the total number of homicides per 100,000 people for each US state and the district of Columbia. The total number of murders fluctuates between zero and fifteen homicides per 100,000 people.

Figure 4. *Total murders per 100,000 people in each state between 2005 and 2019.*

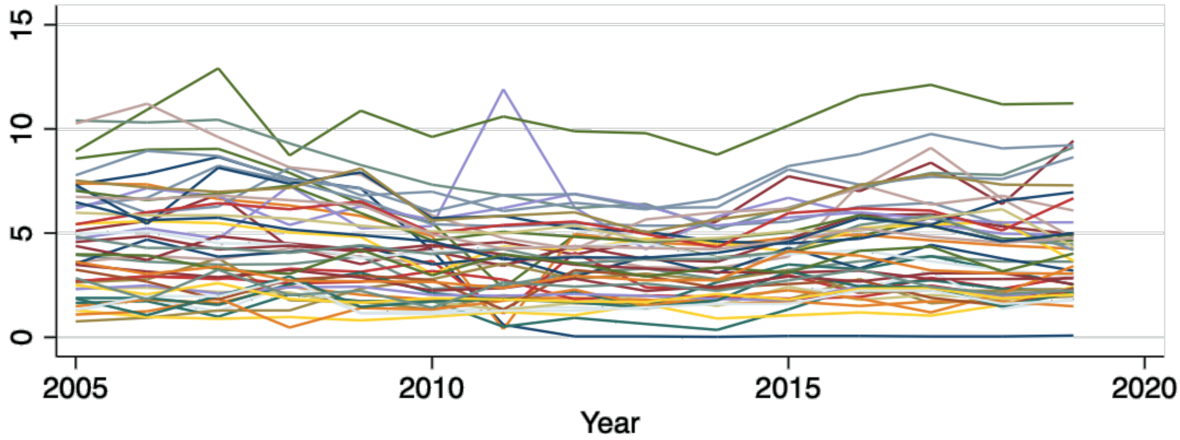


Table 3 shows the number of democratic governors, which is added as a control variable. The average participation in the UCRP was already showed visually in Figure 2.

Table 3. *Number of democratic governors in the United States. Source: National Governors association*

Year	Number of democratic governors
2005	22
2006	28
2007	28
2008	29
2009	26
2010	20
2011	20
2012	20
2013	20
2014	18
2015	18
2016	18
2017	16
2018	16
2019	23

5. Results

5.1 The effect of universal background checks

The results of five models with different sets of control variables are presented in Table 4. The corresponding p-values are added as well between brackets.

Table 4. *Results of the fixed effects regression with murders per 100,000 people per state as the dependent variable.*Significant under the 5% significance level, **Significant under the 10% significance level.*

Variable name	(I)	(II)	(III)	(IV)	(V)	(VI)
Universal background checks	0.485** (0.081)	0.600* (0.016)	0.497** (0.063)	0.643* (0.019)	0.572* (0.033)	0.443 (0.108)
1-year anticipation	0.314 (0.410)	0.324 (0.374)	0.326 (0.385)	0.382 (0.318)	0.327 (0.389)	0.274 (0.470)
2-year anticipation	1.051* (0.006)	1.061* (0.005)	1.062* (0.005)	1.094* (0.004)	1.043* (0.006)	1.000 (0.008)
3-year anticipation	-0.066 (0.862)			-0.057 (0.880)	-0.092 (0.808)	-0.113 (0.746)
1-year treatment	-0.062 (0.862)	-0.169 (0.664)	-0.062 (0.877)	-0.064 (0.875)	-0.075 (0.853)	-0.067 (0.867)
2-year treatment	-0.040 (0.929)	-0.172 (0.672)	-0.063 (0.880)	0.023 (0.956)	0.007 (0.988)	-0.068 (0.872)
3-year treatment	0.447 (0.286)		0.447 (0.285)	0.511 (0.225)	0.496 (0.239)	0.444 (0.290)
Participation	-0.801 (0.617)	-0.788 (0.172)	-0.805 (0.163)	-0.703 (0.227)		-0.737 (0.201)
Democrat	-0.117 (0.258)	-0.118 (0.250)	-0.119 (0.247)	-0.158 (0.124)	-0.146 (0.155)	
Density	-0.104 (0.000)	-0.104* (0.000)	-0.104* (0.000)	-0.091* (0.000)	-0.090 (0.000)	-0.103* (0.000)
Median income	0.033* (0.003)	0.034* (0.002)	0.033* (0.003)			0.035* (0.000)
Constant	9.337	9.312	9.358	10.196	9.697	9.135
Year FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Overall R^2	0.001	0.001	0.001	0.000	0.000	0.001
Observations	734	734	734	734	734	734

The results in Table 4 show positive, significant effects of universal background checks on the total number of homicides. The magnitude of this, intuitively surprising, result lies between 0.497 and 0.643 additional homicides per 100,000 people. Most models also show significant, positive anticipation effects two years prior to the implementation. The magnitude of this effect lies between 1.051 and 1.094 persons that have fallen victim to a homicide per 100,000 people. This effect is additional to the effect universal background checks.

The density of a state shows negative, significant effects. This is a contra intuitive result that is not in line with previous results by Gius (2015). A potential explanation for this result is that the average density was used. Another potential measurement for density could be the percentage of the population that lives in cities, since cities are associate with higher homicide rates than smaller towns. However, this method didn't lead to significant effects in previous research (Gius, 2015).

A rise in median income is also associated with a larger homicide rate. The median income, which is measured in absolute terms, was expected to have a negative effect. Based on the results in Table 4 a $1 rise in a state's median income is associated with roughly 0.035 additional homicides per 100,000 people.

The participation variable does not show significant results. Not including this variable would change very little to the results. However, some measurement of the participating agencies is required to account for the fact that data is submitted voluntarily by the agencies.

The dummy variable Democrat, which measures which political party is in power in a state, does not show significant results. However, omitting this variable changes the results for the universal background checks, as the p-value increases above the 10% significance level.

5.2 The effect of neighboring states

As described in the methodology section of this paper, it will also be checked whether the implementation of universal background checks in a neighboring state effects the number of homicides in a state that has already implemented universal background checks. The results of this regression are shown in Table 5.

The implementation of universal background checks would, intuitively, decrease the number of imported guns in a state where universal background checks are already in effect. No proof of this hypothesis can be found in the dataset. The first model, where the dummy variable 'neighboring state'(NS), is added to model I in from the previous section, does not show significant effects for the implementation in neighboring states. The NS dummy variable takes value 1 when at least one neighboring state switches from the control to the treatment group. Results for the control variables are comparable to the results in Table 4.

In the second model of Table 5 an interaction term between the universal background check variable and the neighboring states variable is added instead of just the dummy variable. This interaction term measures the effect for states that have implemented universal background checks and of which at least one neighbor has implemented such checks. This effect is positive and significant, just as was the case in the models in Table 4. The results for the control variables are comparable to the first model in Table 5 and Table 4.

Table 5. *The effect of universal background checks and the effect of the implementation of universal background checks in a neighboring state.* *Significant under the 5% significance level. **Significant under the 10% significance level.

Variable name	(I)	(II)
Universal background checks	0.328 (0.123)	
Neighboring states	0.289 (0.524)	
Interaction term		0.980** (0.052)
1-year anticipation	0.164 (0.802)	-0.047 (0.943)
2-year anticipation	0.518 (0.425)	0.493 (0.445)
3-year anticipation	0.609 (0.347)	0.584 (0.366)
1-year treatment	0.035 (0.961)	0.016 (0.982)
2-year treatment	0.161 (0.820)	0.142 (0.840)
3-year treatment	-0.339 (0.634)	-0.354 (0.618)
Participation	-0.607 (0.293)	-0.872 (0.137)
Democrat	-0.098 (0.343)	-0.093 (0.369)
Density	-0.101* (0.000)	-0.098* (0.000)
Median income	0.035* (0.002)	0.034* (0.000)
Constant	8.935	8.755
Year FE	Y	Y
State FE	Y	Y
Overall R^2	0.001	0.001
Observations	734	734

5.3 Robustness checks

In this section the robustness of the results will be tested by altering some specifications of the model. Two additions will be made and tested separately. First the dependent variable will be changed to the number of gun related homicides, instead of overall homicides. Secondly, the main model will be altered by allowing a longer delay in treatment effects.

5.3.1 Changing the dependent variable from 'murders per 100,000 people' to 'gun related murders per 100,000 people'.

Another method to test for the effect of stricter gun policies is by using the number of homicides in which a gun rather than the number of homicides in general. A downside of this method is that the number of murders is more accurately accounted for than the number of murders that is committed with a gun. For completeness it will be shown in this section how the results change when the same regression is executed but with the number of gun related homicides instead of the number of homicides. To obtain these results formula number 1 is slightly altered. The dependent variable now becomes the total number of murders that is committed with a gun. The results of this altered regression are shown in Table 7.

The outcome of the altered regeression is presented in Table 6. The result for universal background checks is not significant when using the number of gun related homicides instead of overall homicides. The results for most other control variables are comparable to the models in Tables 4 and 5, other than that the median income no longer shows significant results.

Table 6. *The coefficients (and p-values) after changing the dependent variable from the total murders per 100,000 people to the total number of gun related murders per 100,000 people. *Significant under the 5% significance level, **Significant under the 10% significance level.*

Variable name	Coefficients
Universal background checks	0.361 (0.481)
1-year anticipation	0.136 (0.847)
2-year anticipation	0.744 (0.288)
3-year anticipation	0.007 (0.991)
1-year treatment	-0.189 (0.798)
2-year treatment	-0.007 (0.993)
3-year treatment	0.293 (0.704)
Participation	-0.826 (0.438)
Democrat	-0.002 (0.989)
Density	-0.049 (-0.025)*
Median income	0.025 (0.220)
Constant	3.706
Year FE	Y
State FE	Y
Overall R^2	0.001
Observations	734

5.3.2 Allowing for a longer delay in treatment effects.

In the models in Table 4 dummy variables have been included that measure a delayed treatment effect up to three years after a state implemented a universal background check. This may raise the question whether a delayed effect can be found longer than three years after implementation. To test for this effect three new dummy variables will be included in the regression, in addition to the three dummy variables that were already in the model of Table 4. In Table 7 the full model of Table 4 will be presented with varying lengths of a delayed treatment effects.

Table 7. *The coefficients (and p-values) after including more dummy variables for delayed treatment effects to model (I) in Table 4. *Significant under the 5% significance level, **Significant under the 10% significance level.*

Variable name	(I)	(II)	(III)	(IV)	(V)	(VI)
Universal background checks	0.529* (0.026)	0.587* (0.024)	0.484** (0.081)	0.437 (0.148)	0.377 (0.310)	0.076 (0.836)
1-year anticipation	0.311 (0.413)	0.312 (0.413)	0.314 (0.410)	0.314 (0.409)	0.317 (0.406)	0.320 (0.400)
2-year anticipation	1.049* (0.006)	1.050* (0.006)	1.051* (0.006)	1.051* (0.006)	1.053* (0.006)	1.059* (0.006)
3-year anticipation	-0.067 (0.858)	-0.066 (0.861)	-0.066 (0.861)	-0.066 (0.861)	-0.064 (0.865)	-0.056 (0.881)
1-year treatment		-0.168 (0.665)	-0.062 (0.878)	-0.011 (0.977)	0.091 (0.837)	0.350 (0.456)
2-year treatment		-0.172 (0.672)	-0.063 (0.880)	-0.011 (0.979)	0.094 (0.840)	0.350 (0.472)
3-year treatment			0.447 (0.2876	0.499 (0.255)	0.604 (0.192)	0.861** (0.077)
4-year treatment				0.186 (0.686)	0.294 (0.544)	0.554 (0.275)
5-year treatment					0.191 (0.643)	0.572 (0.238)
6-year treatment						0.920** (0.090)
Participation	-0.788 (0.172)	-0.780 (0.178)	-0.798 (0.168)	-0.795 (0.170)	-0.779 (0.179)	-0.729 (0.209)
Democrat	-0.117 (0.258)	-0.116 (0.262	-0.117 (0.258)	-0.117 (0.259)	-0.118 (0.256)	-0.122 (0.238)
Density	-0.104* (0.000)	-0.104* (0.000)	-0.104 (0.000)	-0.104* (0.000)	-0.104* (0.000)	-0.105* (0.000)
Median income	0.034* (0.002)	0.034* (0.002)	0.033* (0.003)	0.033* (0.003)	0.032* (0.004)	0.034* (0.002)
Constant	9.304	9.291	9.337	8.322	9.394	8.501
Year FE	Y	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Overall R^2	0.001	0.001	0.001	0.001	0.001	0.001
Observations	734	734	734	734	734	734

Most of the newly added dummy variables do not show significant effects that indicate a delay in treatment effects. The effects for universal background checks, however, decreases in magnitude and loses its significance when a delayed treatment effect of four years or more is tested. Moreover, when six dummy variables are added, there appears to be a significant additional effect three years and six years after treatment. Both these effects are on top of a potential effect of the universal background check dummy. The explanatory power from these delayed effects is most likely coming from the universal background check variable, which decreases in magnitude and significance.

6. Conclusion and discussion

This study aims to test whether a causal effect can be obtained for the effect that universal background checks on gun sales have on the number on homicides using a fixed effects regression. Based on the results of this regression, a positive relation exists between the implementation of universal background checks and homicides. The magnitude of this effect lies around 0.485 and 0.643 homicides for every 100,000 people. This counterintuitive result is in line with earlier results obtained by Gius (2015), who also found a positive relationship between the implementation of the Brady act and the number of homicides in treatment states.

Another result of this study is that there seems to be evidence of an anticipation effect two years prior to treatment. The magnitude of this effect lies around 1 person for every 100,000 people. This effect comes on top of the effect of universal background checks. This can be considered a confirmation of the study by Jones (2015), who found gun sales increase when further restrictions are expected. Now there seems to be evidence the expected restrictions can lead to an increase in the number of homicides.

The other aim of this study is to check whether a causal relation exists between the number of homicides in a state that already implemented universal background checks and the implementation of a universal background check policy in a neighboring state. According to the results of the fixed effects regression there appears to be a positive interaction effect between the implementation of universal background checks and the implementation of such a policy in a neighboring state. This effect has a magnitude of around 0.980 homicides for every 100,000 people.

The obtained results do not hold when the explanatory variable, the number of homicides, is replaced by the number of gun related homicides. This regression yields no significant effects. Based on the available information and resources it cannot be distinguished whether this discrepancy in the results comes from missing data or whether this is the true effect.

When adding more variables to the regression to allow for a further delay in treatment effects it was found that three years and six years after implementation the number of homicides increases. However, further research into the long-term effects of universal background policies should be conducted.

There are some important limitations to this research. Some control variables that would have been interesting to add were not included due to missing data. This includes the alcohol consumption per capita and race, as used by Gius (2015; 2018), Cook (2000) and Sumner et al., (2008). Previous research indicates these controls may have significant effects and omitting these variables may lead to omitted variable bias and an overestimation of the effects. Another limitation is that the data on homicides is submitted voluntarily to the FBI Uniform Crime Reporting Program (UCRP). Although the participation rate is not low, one could be concerned that agencies self-selected into the program, which would lead to an over- or underestimation. This should be considered by anyone interpreting these results. Lastly, this research tested solely for the effect of universal background checks, not the effect of other policies or a cluster effect of various gun policies. This may lead to omitted variable bias and a misestimation of the effects.

Further research is required to deepen our understanding of the dynamics of gun policy in the United States and homicides. An important topic for further research can be the long-term effect of universal background checks, using a dataset with a longer time frame than the one available for this paper. The effects of gun trafficking could also be further investigated by empirically studying the effect distance between states has on gun trafficking, as suggested by Knight (2013). Currently not much research has been conducted of the potential effect gun trafficking has on the number of homicides, and the results of this paper are merely a starting point for more future research into this topic.

7. References

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