



# **Effect of Firearm Purchase Delay Laws on Handgun-Related Suicides in the United States**

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# Abstract

The focus of this paper is on examining the effect of firearm purchase delay laws on handgun-related suicides in the United States. Using a difference-in-differences approach, the main research question exploited within-state variation across time when four states repealed restrictive delay laws. The results provide evidence of an increase in the rate of handgun-related suicides by between 17 and 26 percent. However, a 6.5 percent decrease in non-handgun-related suicide rates is also observed. These findings suggest that substitution is occurring where suicidal individuals switch towards handguns once they are more easily available. No evidence was found that the observed increase in handgun-related suicide rates led to an increase in overall suicide rates. An additional research question explored the effect of delay laws on handgun-related suicides during the economic recession of 2008. The results provide evidence of a 4 percent increase in handgun-related suicide rates in states without restrictive delay laws. However, a decrease in both non-handgun-related and overall suicide rates is also observed, questioning the mechanisms by which the 2008 recession affected different states.

**Key words:** Handguns, Suicide, Gun Control, Difference-in-Differences

# Contents

<b>1</b>	<b>Introduction</b>	<b>1</b>
<b>2</b>	<b>Literature Review</b>	<b>4</b>
2.1	Suicides and access to means . . . . .	4
2.2	Relationship between firearms and suicides in the United States . . . . .	6
2.3	Firearm purchase delay laws and their effect on firearm-related suicides . .	7
<b>3</b>	<b>Data</b>	<b>9</b>
3.1	Firearm purchase delay laws . . . . .	9
3.2	Handgun-related suicides . . . . .	13
3.3	Control variables and handgun purchases . . . . .	14
3.4	Descriptive statistics . . . . .	16
<b>4</b>	<b>Empirical Strategy</b>	<b>19</b>
4.1	Research question 1 . . . . .	19
4.1.1	Callaway and Sant’Anna (2021) . . . . .	21
4.2	Research question 2 . . . . .	23
4.3	Validity of identifying assumptions . . . . .	23
<b>5</b>	<b>Results</b>	<b>27</b>
5.1	Research question 1 . . . . .	27
5.1.1	Robustness checks . . . . .	36
5.2	Research question 2 . . . . .	39
5.2.1	Robustness checks . . . . .	42
<b>6</b>	<b>Conclusion and Discussion</b>	<b>45</b>
6.1	Limitations . . . . .	46
	<b>Whole bibliography</b>	<b>47</b>
<b>A</b>	<b>Additional analyses</b>	
A.1	Research question 1 . . . . .	
A.2	Research question 2 . . . . .	
<b>B</b>	<b>Data cleaning</b>	
<b>C</b>	<b>Comprehensive list of delay laws</b>	

## List of Figures

1	Mechanism of means restriction (From “Reducing a Suicidal Person’s Access to Lethal Means of Suicide”, by Catherine W. Barber and Matthew J. Miller, 2014, American journal of preventive medicine, p. 2) . . . . .	5
2	NoDelay states (RQ1) . . . . .	11
3	NoDelay states (RQ2) . . . . .	13
4	Handgun BGC rates in the <i>NoDelay</i> states (RQ1) . . . . .	27
5	Event study plot - Handgun BGCs (RQ1) . . . . .	29
6	Baseline event study plots (TWFE, RQ1) . . . . .	32
7	Baseline event study plots (CS, RQ1) . . . . .	33
8	Placebo test (RQ1) . . . . .	38
9	Evolution of outcome variable (RQ2) . . . . .	39

## List of Tables

1	Data sources . . . . .	9
2	Summary of repealed laws (RQ1) . . . . .	12
3	Descriptive statistics (RQ1) . . . . .	16
4	Descriptive statistics (RQ2) . . . . .	18
5	Handgun BGCs (RQ1) . . . . .	28
6	Baseline results (TWFE,RQ1) . . . . .	30
7	Baseline results (CS, RQ1) . . . . .	35
8	Robustness checks (RQ1) . . . . .	37
9	Baseline results (RQ2) . . . . .	41
10	Robustness checks (RQ2) . . . . .	43

# 1 Introduction

For years, gun control policies in the United States have been a contentious and divisive issue. After every major incidence of firearm violence, public debate lights up attracting commentators from both sides of the political spectrum. However, it is not as widely known that suicides are the most common firearm-related death in the United States. Specifically, in 2020 there were 24,292 firearm-related suicides compared to 19,384 firearm-related homicides (Centers for Disease Control and Prevention, 2022). Furthermore, firearms are used in 52.8 percent of suicides making it the most common suicide method (Centers for Disease Control and Prevention, 2022). These numbers provide evidence that firearm-related suicides are the “silent killer” that often does not receive enough media coverage and public attention. The main assumption of the mechanism at play, which will be further addressed in the literature review, is that the ease of access to firearms greatly increases suicide risk, immediately or during a suicidal episode in the future (Kellermann et al., 1992; Studdert et al., 2020). Suicides are often impulsive decisions (Williams et al., 1980), and if people had the time to “cool off”, they would have the opportunity to change their mind or seek help, which could result in saving their life (World Health Organization, 2014). Even if an individual is not considering self-harm at the time of a firearm purchase, this could unintentionally provide a mechanism by which one could commit lethal self-harm in the future. Finally, if people decide to continue their suicidal plan regardless, they could use a less lethal suicide method if a highly lethal method, such as a firearm, was not available (Florentine and Crane, 2010). This could, in turn, increase their overall chances of surviving.

The current legislative setting in the United States provides a great opportunity to research this topic and there is a wide range of different firearm policies. Smart et al. (2020) classified these policies into 3 main groups:

1. Policies regulating who may legally own, purchase, or possess firearms
2. Policies regulating firearm sales and transfers
3. Policies regulating the legal use, storage, or carrying of firearms

This paper will focus on *2. Policies regulating firearm sales and transfers*. Specifically, the focus is on two policies of interest to the topic - *waiting periods* and *licensing and permitting requirements* since these policies provide delayed access to firearms for individuals looking to make a purchase and obtain a firearm. These policies could also effectively

discourage currently non-suicidal individuals from making purchases, which could unintentionally save their life in the future. Specifically, waiting periods impose mandatory delays between the purchase and handover of a firearm, which could act as a cooling-off mechanism for a suicidal individual (Edwards et al., 2018). On the other hand, licensing and permitting requirements create delays through other mechanisms. Individuals have to go through the effort of obtaining a permit and the processing time accompanying this procedure in itself causes a delay which could dissuade a person from purchasing a firearm (Smart et al., 2020). Further proof that these policies could deter suicidal individuals from obtaining a firearm is that suicidal individuals often have lower problem-solving skills (Pollock and Williams, 2004). Given this information, it would be hard to imagine them having the patience and capabilities to solve a highly administrative process and consequently obtaining a firearm in a short time frame. In addition, licensing and permitting requirements have the potential to decrease the number of suicides associated with firearms by restricting access to firearms for individuals at a higher risk of misusing them (Smart et al., 2020). The above-mentioned delays would not be present in states in which there are no delay laws present, meaning that an individual can walk into a gun store and purchase a firearm immediately. Hence, in this work, a state is considered as having a delay if it imposes a waiting period and/or a permitting requirement for a firearm. More specifically, the main research question exploits variation in which 4 states (Michigan, Missouri, Virginia, and Wisconsin) switched from states with some form of delays to no-delay states from 1999 until 2020. Furthermore, the focus in this paper is on a specific type of firearms - handguns. This is because the laws which were repealed are laws that specifically targeted handguns and because it has been shown that around 75 percent of all firearm-related suicides are made with handguns (Hanlon et al., 2019). Hence, the first research question of this paper aims to estimate the effect of repealing delay laws on handgun-related suicides. To extend this research question, the author examined a specific time period that provided a shock to all U.S. citizens: the economic recession of 2008. This period was of particular interest because it was a time of widespread financial distress, characterized by high rates of unemployment, foreclosure, and bankruptcy. According to the National Bureau of Economic Research (NBER), the economic recession in the United States began in December 2007 and lasted until June 2009 (National Bureau of Economic Research, 2010). During this period, suicide rates increased in European and American countries, especially in countries with high levels of job loss (Chang et al., 2013). To be more precise, Chang et al. (2013) estimated that there were around 2500 excess suicides in

the United States in 2009. This effect was particularly strong for males, who accounted for around 90 percent of excess deaths by suicide. By investigating the relationship between delay laws and handgun-related suicides during this time period, this paper can shed light on the potential impact of gun control policies on suicide rates during periods of economic crisis. Therefore, the aim of the additional research question is to estimate the effect of delay laws on handgun-related suicide rates during the 2008 recession.

Several studies have already tried providing causal evidence of the effects of delay laws on firearm-related suicides. However, when reviewing the critical synthesis of all relevant studies, the conclusion is that there is still only moderate evidence that waiting periods decrease firearm-related suicide rates, and evidence is even more limited for the effects of licensing and permitting requirements (Smart et al., 2020). Since evidence on the effect of delay laws on handgun-related suicides is still inconclusive, this paper aims to provide opportunities to expand the frontiers of knowledge in this field. Furthermore, the findings can provide policymakers with additional evidence on how to address rising handgun-related suicide rates through relatively simple measures such as delay laws. Finally, to the best of the author's knowledge, this is also the first study in this field that incorporated a new advance in the difference-in-differences literature, *Difference-in-Differences with multiple time periods* (Callaway and Sant'Anna, 2021).

In conclusion, my research question is two-fold. The first and main research question will try to provide evidence of the effect of repealing firearm purchase delay laws on handgun-related suicide rates in the United States (Research Question 1). Additionally, the second research question will examine the impact of firearm purchase delay laws on handgun-related suicide rates during the 2008 economic recession (Research Question 2). For the remainder of this paper and for the sake of clarity, these research questions will be referred to as *Research Question 1* and *Research Question 2*.

This paper is structured as follows. Section 2 presents a comprehensive literature review. Section 3 explains the main data sources, while Section 4 outlines the empirical strategy, including the methods and econometric techniques used to analyze the data. Section 5 presents the results of the analysis, and Section 6 offers a concluding discussion of the findings and limitations of the study.



## 2 Literature Review

This section reviews the existing literature relevant to the topics of the paper. It is thematically focused, starting broadly, with each subsection gradually narrowing down the material to the exact sub-field of research in which this paper lies, i.e., *how firearm purchase delay laws affect firearm-related suicides*. First, the general evidence on the relationship between suicides and access to means is presented. Second, the focus will be on a specifically lethal and common means of suicide in the United States - firearms, and their relation to suicides. The section concludes with a summary of existing evidence of how firearm purchase delay laws affect firearm-related suicides.

### 2.1 Suicides and access to means

The official definition of suicide is “Death caused by self-directed injurious behavior with any intent to die as a result of the behavior” (Crosby et al., 2011, p. 23). Multiple cultural and individual factors determine suicide risk such as discrimination, trauma, sense of isolation and lack of social support, mental disorders, harmful use of alcohol and other substances, family history of suicide, and other factors (World Health Organization, 2014). Economists have also tried to model and understand suicidal behavior, with the first notable work being done by Hamermesh and Soss (1974). They used a utility maximization model which assumed that individuals commit suicide when the total discounted lifetime utility to that individual reaches zero, that is, when the remaining lifetime utility becomes negative after this point. However, there is a substantial body of evidence suggesting that a large percentage of suicide attempts are “impulsive”<sup>1</sup> acts. One such study interviewed patients who attempted self-harm in Australia and found that 40 percent of the patients spent 5 minutes or less on suicide attempt premeditation (Williams et al., 1980). Two other studies that interviewed survivors of nearly lethal suicide attempts provided evidence that the time between suicidal ideation and suicide attempt for up to 50 percent of individuals ranged from 5 to 10 minutes in total (Deisenhammer et al., 2009; Simon et al., 2001). This evidence raises the question of whether most suicides can be viewed as a consequence of a detailed cost-benefit analysis performed by the suicidal individual. On the other hand, results based on interviews could be confounded due to survivorship bias, i.e., interviewed survivors and their methods could be different than those who did not survive. Furthermore, there is evidence that it can be highly efficient to restrict the most

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<sup>1</sup>There are different criteria for “impulsivity” depending on the specific study. In this paper, a suicide attempt will be considered impulsive if the premeditation was 10 minutes or less.

lethal methods of suicide with the general goal of reducing the total number of suicides and providing enough time for impulsive individuals to reconsider their actions (Lewiecki and Miller, 2013). This was noted by the World Health Organization (WHO) in their report *Preventing suicide: A global imperative*: “Means restriction (restricting access to the means of suicide) is a key component of suicide prevention efforts because it provides an opportunity for these individuals to reflect on what they are about to do and, hopefully, for the crisis to pass.” (World Health Organization, 2014, p. 24). The mechanism by which means restriction works is also summarized in Figure 1 (Barber & Miller, 2014, p. 2):

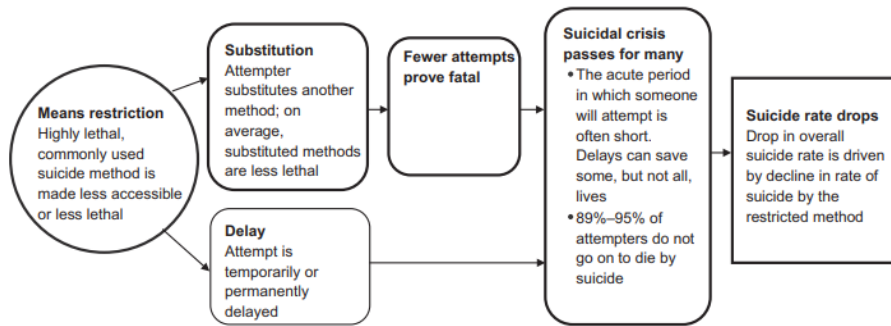


Figure 1: Mechanism of means restriction (From “Reducing a Suicidal Person’s Access to Lethal Means of Suicide”, by Catherine W. Barber and Matthew J. Miller, 2014, American journal of preventive medicine, p. 2)

In some cases, reducing access to means results in substitution, i.e., suicidal individuals will replace the restricted method with other available methods (Florentine and Crane, 2010). In this regard, it is also important to consider that suicide methods have varying levels of lethality. Among all the methods available, firearms are by far the most deadly, up to 3 times more deadly than suffocation (Shenassa et al., 2003). The methods chosen by suicidal individuals depend greatly on the cultural context and availability during the time of crisis (Ajdacic-Gross et al., 2008). Furthermore, evidence from a systematic review of patients who experienced an episode of self-harm found that only around 10 percent die by subsequent suicide (Owens et al., 2002). This provides further evidence that the decision to attempt suicide was not made by a utility-maximizing individual because this would assume that one unsuccessful suicide attempt would not discourage them from ending their life in the future<sup>2</sup>.

<sup>2</sup>Assuming that the future utility of living is negative, a “rational” person should try to end his life immediately after the first unsuccessful attempt, given that future utility will never become positive.

## 2.2 Relationship between firearms and suicides in the United States

Compared to any other country in the world, the United States has the highest number of civilian firearms per capita, with an estimated number of civilian firearms surpassing its population. Specifically, the estimated rate of civilian firearms holdings in 2017 was 120.5 firearms per 100 people (Karp, 2018). There is a large body of evidence from multiple studies that firearm prevalence is associated with an increased risk of suicide-related death (Anglemyer et al., 2014; Briggs and Tabarrok, 2014; Cummings et al., 1997; Kellermann et al., 1992). The results of a cohort study conducted in California in 1991 compared the mortality of persons who purchased handguns with that of the general population of the state. According to the study, the rate of firearm-related suicide was 56 times higher among those who bought handguns (Wintemute et al., 1999). In the same way, a more recent cohort study that followed first-time handgun owners for up to 12 years also provided evidence that handgun ownership is associated with a higher risk of suicide by firearm, with the risk peaking immediately after the first acquisition (Studdert et al., 2020). This strongly suggests that some individuals buy handguns with the sole purpose of committing self-harm. However, there are some limitations to these findings. People who purchase firearms could also be more likely to consider suicide, essentially there is a process of self-selection that could be driving some of these results (Wintemute et al., 1999). Furthermore, another limitation of most studies is that data on the prevalence and ownership of firearms are lacking and most researchers rely on proxy measures when trying to control for this important variable (Smart et al., 2020). In the United States, suicide attempts involving firearms are by far the most lethal, with 96 percent of such attempts being fatal (Shenassa et al., 2003). In terms of the preferred type of firearm, handguns have commonly been used in about three-fourths of firearm-related suicides (Hanlon et al., 2019; Wintemute et al., 1988). Regarding demographic characteristics, men, and especially white men, have 4 to 7 times higher rates of firearm-related suicides compared to women of all races (Centers for Disease Control and Prevention, 2022; Fowler et al., 2015). However, this demographic group is also more likely to own firearms in the first place (Parker et al., 2017). Finally, the rate of firearm-related suicides among males has been increasing since 2006, and since recently, firearm-related suicides have also become the leading means of suicide for females as well, replacing poisoning (Garnett et al., 2022).

## 2.3 Firearm purchase delay laws and their effect on firearm-related suicides

A large body of previous research exploited the variation created by The Brady Handgun Violence Prevention Act, which became effective on February 28, 1994. This law imposed a waiting period of 5 business days before a licensed dealer could sell or transfer a firearm to an unlicensed individual<sup>3</sup>. However, this change did not apply to states that already required a background check and waiting period. Furthermore, the change lasted only for a few years since it ceased to apply on November 30, 1998. Many researchers used this variation in waiting periods to test the effect of statutory delays on firearm-related suicides. One such work was done by Ludwig and Cook (2000). They found no evidence of changes in suicide rates except for a significant decrease when looking at a subgroup of the overall population - males aged 55 years or older. However, other studies found more consistent evidence of a significant decrease in firearm-related suicides exploiting the Brady Act. To be more precise, Luca et al. (2017) found that waiting periods led to a 7 to 11 percent reduction in firearm-related suicides, depending on the control variables used in the specification. Similarly, Edwards et al. (2018) found a smaller but statistically significant effect of reductions in firearm-related suicides by about 2 to 5 percent. Only a handful of studies have analyzed the effect of changes in permit-to-purchase laws on firearm-related suicides. Crifasi et al. (2015) focused on permit-to-purchase law changes in Connecticut and Missouri. Using a synthetic control method, they found evidence of a 15.4 percent decrease in firearm-related suicide rates once the law was implemented in Connecticut and a 16.1 percent increase when the law was repealed in Missouri. The above-mentioned Luca et al. (2017) paper, which found a 7 to 11 percent reduction in firearm-related suicides, also incorporated permitting requirements since they looked at states which had either a waiting period and/or a permitting requirement. Comparably, Koenig and Schindler (2018) estimated the effect of having either a waiting period and/or a permitting requirement on handgun-related suicide rates during the firearm demand shock in 2012/2013. They found negative point estimates which were statistically insignificant, most likely the consequence of the small time window used in their study. Lastly, when creating a critical synthesis of research evidence, Smart et al. (2020) found only moderate evidence on the effectiveness of waiting periods in curbing firearm-related suicides and the evidence is even more limited for permit-to-purchase laws. This was because most of the studies in the field used data sets that focused on the same time period. For example,

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<sup>3</sup>Brady Handgun Violence Prevention Act. 18 U.S.C. § 921 (1993)

the above-mentioned studies on waiting periods all focused on the period during and after the implementation of the Brady Act. Edwards et al. (2018) and Luca et al. (2017) also had very similar empirical specifications, using the two-way fixed-effects difference-in-differences estimator. On the other hand, Crifasi et al. (2015) only focused on changes in two specific states. Finally, even though US legislation provides great opportunities for research because of differing laws between a large number of states, determining causality has been problematic. When trying to measure the effects of changes in one law or a small set of laws on firearm-related suicides, it is hard to conclude that the results of a respective study are driven just by one specific policy change. Lastly, firearm laws are not set exogenously, and they highly depend on the social and cultural attitudes towards firearms within a population, making cross-state comparisons even more difficult.

### 3 Data

Both research questions use the same data sources<sup>4</sup>. Research Question 1 uses yearly data from 1999 to 2020, while Research Question 2 uses yearly data from 2000 to 2009. Table 1 presents the sources that are used for the main variables of interest.

Variable	Source
Firearm purchase delay laws	Cherney et al. (2022) and Siegel et al. (2017)
Suicide rates	Centers for Disease Control and Prevention (CDC)
Demographic control variables	The United States Census Bureau (USCB)
NICS firearm background checks	Federal Bureau of Investigation (FBI)
Unemployment rate	U.S. Bureau of Labor Statistics (BLS)
Estimate of firearm prevalence	Schell et al. (2020), by way of CDC

Table 1: Data sources

#### 3.1 Firearm purchase delay laws

Siegel et al. (2017) created a comprehensive database containing detailed annual information on firearm-related laws for the 50 states of the United States from 1991 until 2020. This is one of only a few balanced databases on firearm-related laws in the United States containing a total of 133 provisions of firearm laws. As the paper focuses on handgun-related suicides, the focus will be on handgun-purchase delay laws. The two main variables of interest are:

- *waitingh* - Binary variable for if a waiting period is required on all dealer-issued handgun purchases.
- *permith* - Binary variable for if a license or permit is required to purchase handguns.

A new variable *Delay* was created using these two binary variables. This variable is equal to 1 if a state imposes a waiting period and/or a permitting requirement for a handgun. In other words, if either of the two above-mentioned variables is equal to 1, the assumption is that there is a non-zero waiting time between the purchase and availability of a handgun. However, since the “treatment” in Research Question 1 is repealing a handgun purchase delay law, the binary variable *NoDelay* was created (the opposite of *Delay*). *NoDelay* is

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<sup>4</sup>With only minor deviations that will be noted in this section.

the main and only treatment variable that will be used in this work, and it takes a value of 1 for the first period when a delay state becomes a no-delay state, and it stays 1 until the end of the panel since no state changed treatment status multiple times. Consequently, all of the states in the control group are states that have some kind of delay law throughout the whole duration of the panel, and the treatment group are states that switched from a delay state to a no-delay state until the end of the panel. For consistency purposes, this notation also holds for Research Question 2. The only exception is that during this sub-period, the focus is only on states that either had or did not have handgun purchase delay laws throughout the entire duration of the sub-period (no states changed treatment status in this research question). Therefore, for the remainder of this paper, the treatment states will be referred to as *NoDelay* states, and the control states will be referred to as *Delay* states.

To check for discrepancies, the author double-checked the laws of interest using the RAND State Firearm Law Database (Cherney et al., 2022). An important discrepancy was identified between the two databases. According to Siegel et al. (2017), Pennsylvania was coded as having a waiting period for handguns throughout the panel (1999-2020). On the other hand, the RAND database compiled by Cherney et al. (2022) suggested that this waiting period was repealed in 1998. Further checks of state legislation favored the evidence in Cherney et al. (2022) and Pennsylvania was removed from both samples to avoid any mistakes. There were no other relevant discrepancies in the laws of interest between the two data sets.

Figure 2 provides a graphical representation of the *NoDelay* and *Delay* states in Research Question 1. The following 4 states represent the *NoDelay* group: Michigan, Missouri, Virginia, and Wisconsin. On the other hand, the following 13 states are part of the *Delay* group: California, Connecticut, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Minnesota, Nebraska, New Jersey, New York, North Carolina, and Rhode Island. As can be seen, most of the states in this sample are from the Northeast and Midwest statistical regions of the United States.

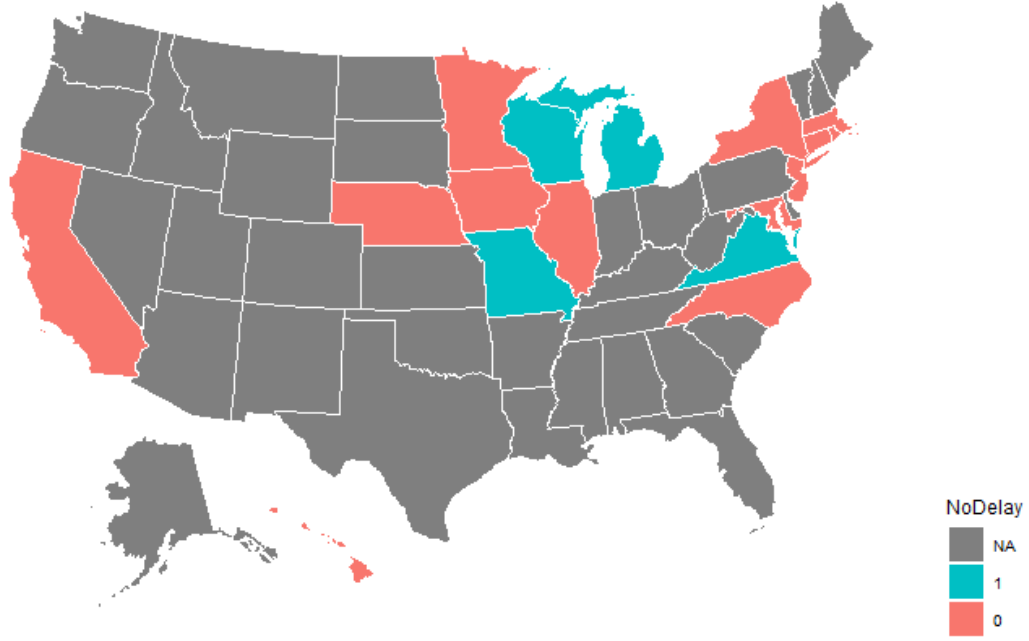


Figure 2: NoDelay states (RQ1)

Table 2 shows which delay laws were repealed in *NoDelay* states and the date on which the repeal was effective. Wisconsin repealed a mandatory 48-hour waiting period before firearm dealers could transfer possession of a handgun to any person who made a purchase (Cherney et al., 2022). On the other hand, Michigan, Missouri, and Virginia repealed handgun permit-to-purchase legislature. Specifically, Missouri repealed legislation that required applicants to be granted a permit by the sheriff of the county in which they reside. Before the repeal, the sheriff had to make a decision on the permit within a maximum of seven days after receiving the application (Cherney et al., 2022). Michigan repealed its permit requirement in mid-December 2012. Therefore, the state was coded as a *NoDelay* state in 2012 in the Siegel et al. (2017) database. However, it seems unfeasible that the law repeal would have any effect in such a short period. For that reason, Michigan was re-coded in the data cleaning process and its first period of treatment was set to 2013. Finally, Virginia repealed its permit-to-purchase legislation, which required permits for inhabitants of any county with a population density of more than 1,000 people a square mile (General Assembly of Virginia, 2004). The final classification and additional information regarding the specific laws present in *NoDelay* and *Delay* states for Research Question 1 are reported in Table 15 (Appendix C).



State	Effective On	Summary	Source
Wisconsin	25.06.2015	Repealed state legislature which required a 48-hour waiting period for handgun purchases.	Cherney et al. (2022) and Siegel et al. (2017)
Michigan	18.12.2012	Repealed handgun permit requirement.	Siegel et al. (2017)
Missouri	28.08.2007	Repealed permit requirement for the purchase of a concealable handgun.	Cherney et al. (2022) and Siegel et al. (2017)
Virginia	31.07.2004	Repealed permit requirement in any county having a density of population of more than 1,000 people a square mile.	General Assembly of Virginia (2004) and Siegel et al. (2017)

Table 2: Summary of repealed laws (RQ1)

Similarly, Figure 3 shows all states that are in the *NoDelay* (highlighted in light blue) and *Delay* group (highlighted in red) for Research Question 2. As can be seen, there are 46 states in total. Out of the 46 states, the following 31 states are part of the *NoDelay* group: Alabama, Arizona, Arkansas, Colorado, Delaware, Florida, Georgia, Idaho, Indiana, Kansas, Kentucky, Louisiana, Maine, Mississippi, Montana, Nevada, New Hampshire, New Mexico, North Dakota, Ohio, Oklahoma, Oregon, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Washington, West Virginia, and Wyoming. The remaining 15 states are part of the *Delay* group: California, Connecticut, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Michigan, Minnesota, Nebraska, New Jersey, New York, North Carolina, Rhode Island, and Wisconsin. Four states were excluded from this analysis. Virginia and Missouri were excluded because they changed treatment status in 2004 and 2007 as noted in Table 2. On the other hand, Alaska had a highly varying number of handgun-related suicides during the years of the sample. Due to this, the observations for this state were excluded from the analysis<sup>5</sup>. Finally, Pennsylvania was excluded due to the discrepancies between the two firearm law databases, as mentioned above. As can be seen, this sample is more diverse than in the previous research question and it covers a larger number of states from each of the four statistical regions in the United States. Again, the final classification and additional information on the specific laws present in the treatment and control groups for Research Question 2 are presented in Table 16 (Appendix C).

<sup>5</sup>Figure 13 in Appendix B plots how handgun-related suicide rates evolved during the years of the sample.

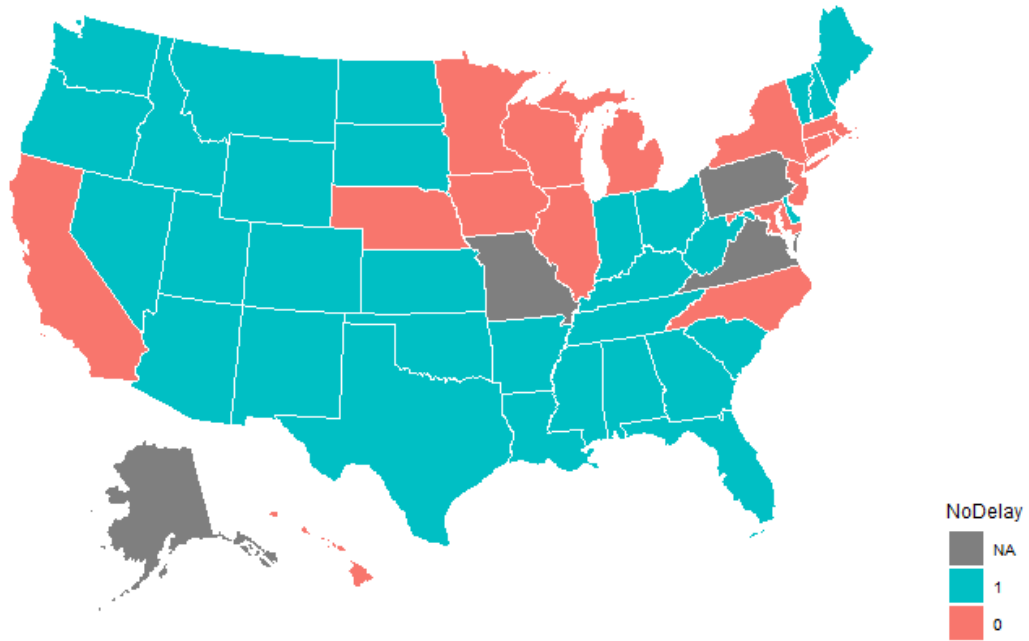


Figure 3: NoDelay states (RQ2)

### 3.2 Handgun-related suicides

The main outcome variable of interest is the rate of handgun-related suicides per 100,000 people. This variable was compiled by aggregating two specific ICD-10<sup>6</sup> codes using CDC's Underlying Cause of Death database<sup>7</sup>:

- *X72* - Intentional self-harm by handgun discharge
- *X74* - Intentional self-harm by other and unspecified firearm discharge

It is important to note that the code *X73* (Intentional self-harm by rifle, shotgun, and larger firearm discharge) is not included in the aggregate of the outcome variable of interest. This is because the laws which were repealed focus on handgun purchases. However, data from this ICD-10 code is used as a robustness check. The intuition behind this is that even if handgun purchase delay laws were repealed there should preferably not be an increase in long-gun-related suicide rates because no specific legislation changed for this type of firearm. As an additional important note, this code had very low values and some of the data were suppressed by the CDC. To obtain a proxy measure of this variable, the author downloaded the aggregate of codes X72-X74 and took the difference with respect to the

<sup>6</sup>The International Classification of Diseases, Tenth Revision (ICD-10)

<sup>7</sup>This database contains mortality statistics for all states in the United States from 1999 to 2020. Death certificates for US citizens are used as the basis for the data, each of which specifies a single cause of death.

aggregate of only X72 and X74:  $(X72 + X73 + X74) - (X74 + X72)$ . This provides a proxy measure of long-gun-related suicide rates for each state. Furthermore, when states repeal restrictive delay laws, it is important to understand how suicide rates with other means change and what the net effect of repealing a restrictive delay law is. To test this, the author used the aggregation of codes X60-X84 (all intentional self-harm ICD-10 codes). All suicide rate variables were log-transformed in the analysis since this is customary in the literature and because suicide rate values are skewed<sup>8</sup>. As a final note, when downloading CDC data, the data sets included population estimates for each state and year. These population estimates were used in the analysis when weighting the regressions, as will be specified in Section 4.

### 3.3 Control variables and handgun purchases

The main control variables are covariates that do not vary over time. To be more specific, these are covariates that have been measured before treatment started. This is important because we cannot rule out the possibility that post-treatment covariates may be potentially affected by the treatment, which could lead to all kinds of bad control problems (Roth et al., 2022). The main source for time-invariant covariates is the U.S. Census Bureau. All of the control variables are compiled for the year 2000 as the joint first year for both research questions and are as follows: percentage of white population, percentage of rural households, percentage of population with only a high-school degree, and median income. These control variables were selected through a comprehensive analysis of the literature, taking into account the most important societal differences that could affect the outcome of interest, handgun-related suicide rates. In addition to demographic variables, it is important to control for the existing rate of firearm prevalence in different states. However, the lack of reliable and valid state-level estimates of firearm prevalence is a known problem in this field of research. This paper measures pre-existing firearm prevalence levels using data collected by the CDC through the Behavioral Risk Factor Surveillance System (BRFSS) telephone survey, and later compiled by Schell et al. (2020). The variable of interest estimates the proportion of respondents who reported living in a household with a firearm for each state (Schell et al., 2020). However, this data set is not without its limitations. The data were collected through a telephone survey and the overall sample size and reliability differ between states. Furthermore, data are only

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<sup>8</sup>The author had to log-transform long-gun-related suicide rates by using the formula  $\log(1 + x)$ , where  $x$  is the long-gun-related suicide rate. This kind of transformation was necessary given the low values of long-gun-related suicide rates in some states (values below 1 give negative values when log-transformed).

available for a given set of years, and the author uses only data for 2004, because it is the only year in which data are available for all states in the sample. Given these limitations, this variable will only be used in further robustness checks of the main findings and not in the main specifications. The only time-varying control variable was added to the sample for Research Question 2. This was the state unemployment rate for each year in the sample (2000-2009), and the source for this variable was the U.S. Bureau of Labor Statistics (BLS). This time-varying control variable was included because it is deemed logical that higher unemployment rates during the 2008 recession could have contributed to higher suicide rates between states. Given the above-mentioned bad control problem, this variable is only used in one specification for strictly exploratory purposes. Finally, in the case of Research Question 1, it is important to show how handgun sales responded to the repeal of restrictive delay laws. Since one of the main assumptions of this paper is that the ease of obtaining a handgun greatly increases the risk of handgun-related suicide, a logical precondition for that assumption to hold is that we should observe an increase in handgun sales once the restrictive policy is repealed. This was measured by using data obtained through FBI's National Instant Criminal Background Check System (NICS). Since 1998, the NICS has been used to assess if a person trying to purchase a firearm is eligible to acquire one, and data is available for all states of interest. However, the NICS data are not without problems. Firstly, the data indicates the number of firearm background checks that were initiated via the NICS, rather than the precise number of firearm sales that took place (Federal Bureau of Investigation, 2023). Therefore, background checks are a proxy variable for the number of firearm sales. Furthermore, the data set had to be cleaned because it is divided between different type of transactions and types of firearms. In terms of transaction types, the focus is on data for background checks initiated by officially licensed firearm dealers, since these transactions account for the vast majority of background checks. In terms of the type of firearms, the author used the data for handguns. However, the NICS documentation states that due to varying state laws relating to handgun permits, some states may reflect lower than expected numbers for handgun checks, but these low numbers are often offset by higher numbers of handgun permits (Federal Bureau of Investigation, 2023). Given this information, the author combined handgun permit checks with the data available for handgun background checks, with the exclusion of permit rechecks<sup>9</sup>.

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<sup>9</sup>Permit rechecks can inflate the number of background checks. Furthermore, they are not part of the main mechanism in which this paper is mostly interested in, since they refer to already existing permit holders.

### 3.4 Descriptive statistics

Descriptive statistics of state characteristics for Research Question 1 are provided in Table 3. As can be seen, there are some differences in observable characteristics between the *NoDelay* and *Delay* states. First, the *NoDelay* states have on average a lower population than the *Delay* states (by about 1.54 million). This could be because California is a *Delay* state. California has a population of around 39 million people, and it could be driving the overall average upward significantly. Furthermore, we can see that suicide rates are on average higher in the *NoDelay* states for all observed forms of suicide (13.23 per 100,000 compared to 10.18 per 100,000). This is especially interesting when considering handgun-related suicide rates (5.4 per 100,000 compared to 3.22 per 100,000). Since there is evidence that the delay laws that were repealed could potentially have an effect on handgun-related suicides, it is unclear why states with unfavorable rates of handgun-related suicides would repeal these laws. In terms of demographic composition, the largest difference between the *NoDelay* and *Delay* states is that on average there is a higher percentage of rural households in the *NoDelay* states (the ratio of rural households is higher by 13 percentage points). Furthermore, on average, there is a higher percentage of white individuals and

	<i>NoDelay</i>	<i>Delay</i>	<i>Diff.</i>
Number of states	4	13	−9
Population (1000s)	7354.89	8894.64	−1540
Waiting period	0.18	0.54	−0.36
Permit requirement	0.31	0.92	−0.61
Delay	0.49	1.00	−0.51
NoDelay	0.51	0.00	0.51
Overall suicide rate (per 100k)	13.23	10.18	3.05
Handgun-related suicide rate (per 100k)	5.40	3.22	2.18
Long-gun suicide rate (per 100k)	1.74	0.74	1.00
% of white population	0.82	0.74	0.08
% of rural households	0.31	0.18	0.13
% with only high school degree	0.31	0.28	0.03
Median income (1000s)	43.27	46.68	−3.41
Firearm prevalence	0.41	0.25	0.16

Table 3: Descriptive statistics (RQ1)

individuals with only a high-school degree (difference by 8 percentage points and 3 percentage points, respectively). With respect to earnings, median income was lower in the *NoDelay* states compared to the *Delay* states (by 3400\$). Finally, data from the BRFSS telephone survey also point in the direction that there are disparities in pre-existing firearm prevalence rates (41 percent of people in the *NoDelay* states reported living in a household with a firearm compared to 25 percent in the *Delay* states). However, as noted in Section 3.3, the data from the BRFSS telephone survey should be interpreted with caution. In summary, there are minor differences between the *NoDelay* and *Delay* states with respect to the observable characteristics.

Similarly, descriptive statistics of the state characteristics for Research Question 2 are provided in Table 4. As can be seen, a similar pattern of results is observable as in Table 3. Again, the *NoDelay* states have a lower population than the *Delay* states on average. However, in this sample, the population differences are much more prominent, and the *NoDelay* states have a smaller population size by about 4 million people. Overall suicide rates are higher in the *NoDelay* states for all observed forms of suicide (14 per 100,000 people compared to 9.43 per 100,000 people). The effect is even more pronounced in this sample, and this could be due to the significant population differences mentioned above. Handgun-related suicide rates are also higher in the *NoDelay* states, more than double the average rate observed in the *Delay* states (6.77 per 100,000 compared to 3.07 per 100,000). With respect to demographic characteristics, all the differences observed in the previous table are also present in this sample. In other words, the *NoDelay* states have on average a higher percentage of people living in rural households, a higher percentage of white population, a higher percentage of citizens with only a high school degree, lower median income, and greater self-reported firearm prevalence rates. Finally, the average unemployment rate is similar in the *NoDelay* and *Delay* states throughout the entire duration of the panel. However, the average unemployment rates differed between the *NoDelay* and *Delay* states during the 2008 recession. The *NoDelay* states had an average unemployment rate of 7.77 percent compared to an average unemployment rate of 8.27 percent in the *Delay* states. Compared to Table 3, the differences in the observable characteristics are greater between the *NoDelay* and *Delay* states. This could be considered as a possible limiting factor of the analysis if these observed differences contributed to a differential reaction to the 2008 recession between the *NoDelay* and *Delay* states.

	<i>NoDelay</i>	<i>Delay</i>	<i>Diff.</i>
Number of states	31	15	16
Population (1000s)	4500.28	8520.21	−4020
Waiting period	0.00	0.53	−0.53
Permit requirement	0.00	0.87	−0.87
Delay	0.00	1.00	−1.00
NoDelay	1.00	0.00	1.00
Overall suicide rate (per 100k)	14.02	9.43	4.59
Handgun-related suicide rate (per 100k)	6.77	3.07	3.70
Long-gun suicide rate (per 100k)	1.50	0.90	0.60
% of white population	0.82	0.75	0.07
% of rural households	0.34	0.20	0.14
% with only high school degree	0.30	0.29	0.01
Median income (1000s)	38.61	46.35	−7.74
Firearm prevalence	0.45	0.27	0.18
Unemployment rate	5.38	5.49	−0.11

Table 4: Descriptive statistics (RQ2)

## 4 Empirical Strategy

This section presents the empirical approach used to identify the causal effect of firearm purchase delay laws on handgun-related suicides. Both Research Questions 1 and 2 use a difference-in-differences (DiD) research design. Explicitly, Research Question 1 focuses on a DiD model in which there is variation in treatment timing. On the other hand, Research Question 2 relies on a DiD model in which pre-existing differences in delay laws are interacted with time-series variation that comes by way of the 2008 economic recession. The following subsections explain in more detail how these approaches differ and the key identifying assumptions needed to establish causality.

### 4.1 Research question 1

Two different estimation methods are used to identify the causal effect of repealing delay laws on handgun-related suicides in Research Question 1. The first estimation method used is the two-way fixed effects difference-in-difference estimator (TWFE). The TWFE estimator simply controls for group and time fixed effects and provides a before versus after estimate of the difference in outcomes between treatment and control groups (Huntington-Klein, 2021). However, recent advances in the econometrics literature have demonstrated that the TWFE estimator can be biased when there is variation in treatment timing and when treatment effects are heterogeneous across different treatment groups (Borusyak and Jaravel, 2017; De Chaisemartin and d’Haultfoeuille, 2020; Goodman-Bacon, 2021). Since variation in treatment timing is present in Research Question 1, the estimation method developed by Callaway and Sant’Anna (2021) is used concurrently because it provides a solution to the above-mentioned problems with TWFE and differential treatment timing. Concerning the main regression equation, it reads:

$$\ln(\text{HandgunSuicides}_{st}) = \alpha + \beta_1 \text{NoDelay}_{st} + \gamma_t X_s + \lambda_t + \mu_s + \varepsilon_{st} \quad (1)$$

where the outcome variable is the natural log of handgun-related suicides per 100,000 people. The effect of repealing delay laws is captured by  $\text{NoDelay}_{st}$ , which is a binary variable for states  $s$  that repealed delay laws in period  $t$ . Therefore, the coefficient of interest  $\beta_1$  captures the percentage difference in handgun-related suicide rates between the  $\text{NoDelay}$  and  $\text{Delay}$  states after the repeal of the delay laws. In terms of fixed effects,  $\lambda_t$  captures year-fixed effects, and  $\mu_s$  captures state-fixed effects. Furthermore,  $\gamma_t X_s$  represents the interaction between year-fixed effects and a vector of pre-treatment control variables. The error term is represented by  $\varepsilon_{st}$ . Regarding inference, since the data are



at the state-year level, the sample does not have enough clusters<sup>10</sup> such that standard errors can be corrected using clustered standard errors. To counteract this problem and get more precise standard errors, the author uses the wild-cluster bootstrapping method (Cameron et al., 2008) in the TWFE model<sup>11</sup>. The wild bootstrap is commonly used when there are few clusters (Roodman et al., 2019). The *boottest* package developed by Roodman et al. (2019) was used to implement this method<sup>12</sup>. On the other hand, when using the Callaway and Sant’Anna estimator, inference is executed using a multiplier-type bootstrap procedure suggested by them in their paper, which can be directly implemented with their *did* package in R. As a final note, due to the disparities in population between the *NoDelay* and *Delay* states, all regressions are weighted by state population.

After estimating Equation 1, it is of equal importance to check for the presence of pre-treatment trends and to understand the dynamics of the treatment effects. Therefore, Equation 1 was transformed into an event-study specification in the following way:

$$\ln(\text{HandgunSuicides}_{st}) = \alpha + \sum_{\tau=-10}^{-2} \beta_{\tau} \text{NoDelay}_{s\tau} + \sum_{\tau=0}^{10} \beta_{\tau} \text{NoDelay}_{s\tau} + \gamma_t X_s + \lambda_t + \mu_s + \varepsilon_{st} \quad (2)$$

where event-time  $\tau$  ranges from  $-10$  to  $10$ , and  $\tau = -1$  is excluded since it is the last period before treatment. All other terms are the same as in Equation 1. The coefficient  $\beta_{\tau}$  captures the percentage difference in handgun-related suicide rates between the *NoDelay* and *Delay* states in period  $\tau$  relative to the last period before delay laws were repealed.

Finally, it is also important to show how handgun sales responded to the repeal of restrictive delay laws. This analysis will provide additional validity to the results of Equation 1 and the findings will be used when estimating the elasticity of handgun-related suicides with respect to handgun sales. The following equation captures the effect of delay law repeals on handgun purchases:

$$\ln(\text{HandgunBGCs}_{st}) = \alpha + \beta_1 \text{NoDelay}_{st} + \gamma_t X_s + \lambda_t + \mu_s + \varepsilon_{st} \quad (3)$$

where the outcome variable is the natural log of handgun background checks per 100,000 people. All other terms are the same as in Equation 1. The coefficient  $\beta_1$  captures the percentage difference in handgun sales<sup>13</sup> between the *NoDelay* and *Delay* states, after the repeal of the respective delay laws. Again, inference is performed by means of wild-cluster bootstrapping.

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<sup>10</sup>Baseline sample has 17 states in total.

<sup>11</sup>Clustered on the state level.

<sup>12</sup>Since all of the empirical analysis was done in R, this was implemented in the *fwildclusterboot* package.

<sup>13</sup>Handgun background checks are a proxy variable for handgun sales.

#### 4.1.1 Callaway and Sant’Anna (2021)

This subsection will start by presenting an overview of the drawbacks that can occur when using a TWFE estimator in DiD setups with staggered timing. Second, the author will continue by briefly presenting the approach developed by Callaway and Sant’Anna (2021). The focus will be on how it solves the presented drawbacks and how the key identifying assumptions relate to this analysis.

TWFE estimators have been used in staggered setups for years. However, as was discovered recently, the computed parameter of interest can be highly biased in the presence of heterogeneous treatment effects (Borusyak & Jaravel, 2017; Goodman-Bacon, 2021). This is due to the underlying weighting process of TWFE regressions which computes the treatment effect as a weighted average of all the possible “canonical” 2x2 DiD estimators (Goodman-Bacon, 2021). Therefore, in a staggered setup, the parameter of interest is a weighted average of comparisons between earlier versus later treated groups, later versus earlier treated groups, and treated versus untreated groups (Goodman-Bacon, 2021). The problem arises when earlier treated units are compared with later treated units (Roth et al., 2022). In the context of this analysis, this means that Virginia is used as a control group for all other *NoDelay* states after already repealing a delay law in 2004<sup>14</sup>. These problematic comparisons can heavily bias estimates if treatment effects vary over time due to the negative weighting problem, as explained in Borusyak and Jaravel (2017) and Goodman-Bacon (2021). Goodman-Bacon (2021) provides a way to decompose the treatment effect parameter according to the weights derived from each of the above-mentioned comparisons. This makes it possible for researchers to observe how the estimate is derived and to understand the extent of bias introduced by the problematic comparison. The treatment parameter of interest in Research Question 1,  $\beta_1$  from Equation 1, is decomposed in Table 12 of Appendix A.1. We can see that the problematic comparison gets a weight of “only” around 4.3 percent, and, indeed, it computes a negative estimate of  $-0.025$ <sup>15</sup>. The relatively small weightage from this comparison group is most likely due to the somewhat larger “never-treated” control group in this sample. Given this information, the overall treatment effect parameter from Equation 1 can be trusted when using the TWFE estimator. However, there are additional problems associated with the TWFE

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<sup>14</sup>Similarly, Missouri would be used as a control group in comparisons with Michigan and Wisconsin after repealing the delay law in 2007. Finally, Michigan would be used as a control group in comparisons with Wisconsin even after repealing the delay law in 2013.

<sup>15</sup>The estimate from this comparison suggests a decrease in handgun-related suicide rates after delay laws were repealed.

estimator in staggered setups. When using the TWFE estimator in the “dynamic” specification as specified in Equation 2, pre-treatment trends can arise solely from the presence of heterogeneous treatment effects (Sun & Abraham, 2021). Therefore, results can be misleading when estimating Equation 2 using the TWFE estimator.

To counteract these issues, Callaway and Sant’Anna (2021) proposed a new estimator which focuses on group-time average treatment effects for each cohort that was treated in a certain period. After estimating all group-time average treatment effects (for different cohorts and time periods), Callaway and Sant’Anna (2021) propose multiple aggregation schemes that allow the construction of different treatment effect parameters. For example, the parameters can be aggregated into parameters showing the treatment effects over event-times  $\tau$ , by specific cohorts  $g$ , or by calendar time  $t$ . Furthermore, these parameters can also be aggregated into a single parameter, showing the overall average effect of the treatment. Their approach also extends the parallel trends assumption, allowing it to hold possibly only after conditioning on pre-treatment covariates. Finally, they also propose three different estimation methods: the outcome regression, inverse probability weighting, or the doubly-robust one. In this paper, the author uses the outcome regression estimation method. However, the specific differences between the estimation methods are beyond the scope of this paper and are further explained in Callaway and Sant’Anna (2021). Importantly, this is automated through their simple *did* package in R. However, in order to identify the average treatment effect by way of the CS estimator, there are two important identifying assumptions that should hold: *Limited Treatment Anticipation* and *Conditional Parallel Trends Based on a “Never-Treated” Group*. In this paper, the author assumes that there are no anticipation effects. This is because the policy that was repealed was restrictive, hence, it is unclear how behavior would change in anticipation of receiving the treatment. Anticipation effects would more likely be a valid consideration in anticipation of a more restrictive policy rather than its repeal. On the other hand, the conditional parallel trends assumption is just an extension of the original parallel trends assumption, allowing it to hold possibly only after conditioning on a set of pre-treatment covariates  $X$  (Callaway & Sant’Anna, 2021). Section 4.3 will provide evidence with respect to the validity of this assumption. Overall, the estimation approach developed by Callaway and Sant’Anna (2021) avoids all of the above mentioned problems with the TWFE estimator in both the static (Equation 1) and dynamic (Equation 2) specification, even when treatment effects are heterogeneous (Roth et al., 2022).

## 4.2 Research question 2

To identify the causal effect of firearm purchase delay laws on handgun-related suicides during the economic recession of 2008, the author uses a similar specification as in Equation 1. However, in this research question there is no variation in the timing of treatment since this research question only analyzes pre-existing differences in delay laws. Therefore, only a TWFE model is estimated. The regression equation for this research question reads as follows:

$$\ln(\text{HandgunSuicides}_{st}) = \alpha + \beta_1(\text{NoDelay}_s \times \text{Post}_t) + \gamma_t X_s + \lambda_t + \mu_s + \varepsilon_{st} \quad (4)$$

where the only difference compared to Equation 1 is that  $\text{NoDelay}_s$  is now interacted with  $\text{Post}_t$ . This is because, in this specification, there are no states that “switch” from *Delay* states to *NoDelay* states, as noted above.  $\text{NoDelay}_s$  is a dummy variable for states  $s$  without delay laws and  $\text{Post}_t$  is a dummy variable for the years  $t$  of the economic recession (2008 and 2009). All fixed effects are the same as in Equation 1 including an interaction term between year-fixed effects and a vector of pre-treatment control variables. Therefore, the coefficient of interest  $\beta_1$  will capture the percentage difference in handgun-related suicide rates between the *NoDelay* and *Delay* states during the 2008 economic recession. In terms of inference, this sample has 46 states and the use of clustered standard errors at the state level is justified. However, for the sake of correctness, the main specification using WCSE is additionally presented in the Appendix. The disparities in population between the *NoDelay* and *Delay* states persist in this sample, and, as in the previous research question, each regression is weighted by state population.

## 4.3 Validity of identifying assumptions

Since both research questions use a DiD research design, several assumptions must be satisfied so that the results can be considered causal. This section focuses on the two most important assumptions that need to hold when using a DiD research design:

1. The parallel trends assumption
2. The absence of other policies/shocks which could have affected the outcome of interest

The parallel trends assumption states that the outcome for the treatment and control group would follow similar trends over time in the absence of treatment. However, the problem with this assumption is that we do not actually observe the trend of the treatment group

in the absence of treatment. As the counterfactual is not observable and since the delay laws are not exogenously set, to prove this we must show that the trends of the outcome variable had a similar trajectory before the start of treatment. On the other hand, we need to demonstrate the absence of other shocks that could have affected our outcome of interest by means of an argument. In other words, we must provide a compelling argument that our findings are the result of the treatment that we analyze. Since Research Question 1 and 2 differ with respect to the proof necessary, the following paragraphs will analyze these two assumptions in greater detail for each research question in turn.

Figure 6a provides graphical evidence of the estimation of Equation 2 which was used to test for the presence of pre-treatment trends in Research Question 1. Observing the pre-treatment event-time estimates, we can see that all except  $\tau = -2$  are negative. Furthermore, there seems to be an overall upward trend in the pre-treatment years. On the other hand, all of the pre-treatment estimates are statistically insignificant with the exception of  $\tau = -9$  and  $\tau = -10$ . These coefficients are estimated with only observations from two states, Wisconsin and Michigan. Hence, they rely on limited data and these statistically significant estimates are observed 10 years before the delay laws were repealed, making it improbable that they have any relevance with respect to the post-treatment changes in handgun-related suicide rates. However, a major limitation of Figure 6a is that the event study plot is estimated by way of the TWFE estimator and pre-treatment trends can arise solely from the presence of heterogeneous treatment effects (Sun & Abraham, 2021). To counteract this problem, the CS estimator allows for the testing of pre-treatment trends. Figures 7a and 7b show event-study plots based on the unconditional (Figure 7a) and conditional<sup>16</sup> (Figure 7b) parallel trends assumption. The aggregate event-time average treatment effects are limited from periods  $\tau = -10$  to  $\tau = 10$ , with the red dots representing periods before the restrictive delay laws were repealed. In these figures there is no evidence of pre-treatment trends. The somewhat increasing trend present in Figure 6a has completely disappeared and not all event-time estimates are negative. Furthermore, in both figures, the previously significant event-time estimates  $\tau = -9$  and  $\tau = -10$  are now statistically insignificant. Finally, Figures 10a and 10b (Appendix A.1) show the group-time average treatment effects of each *NoDelay* state with respect to calendar time. The red dots represent pre-treatment group-time estimates and we can see that Group 2015 (Wisconsin) shows the most consistent signs of pre-treatment trends. Under the conditional parallel trends assumption, the evidence for the presence of pre-treatment

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<sup>16</sup>Conditional on pre-treatment covariates % rural, % only HS degree, % white population, and median income.

trends in Wisconsin is smaller. However, it is important to note that the estimates in these graphs have one observation per calendar year for each *NoDelay* state meaning that they should be interpreted with caution. As a final check, a placebo test was implemented by removing all post-treatment years for the *NoDelay* states and consequently imputing a fake treatment period in the middle of the remaining pre-treatment years. As can be seen in Column 6 of Table 8, the estimate suggests a 12.5 percent increase<sup>17</sup> in handgun-related suicide rates during the fake treatment period in the *NoDelay* states. Figure 8 provides graphical evidence of the placebo test using an event study specification and the results are indicative of an increase in handgun-related suicides rates immediately after the start of the fake treatment period. Overall, the possibility of pre-treatment trends in the *NoDelay* states cannot fully be dismissed. However, it is important to note that more weight should be given to the results which were estimated using the CS estimator. These results show limited evidence of pre-treatment trends. On the other hand, proving the absence of other policies or correlated shocks that could have influenced the outcome of interest is a difficult task. One specific limitation is that only state-year level data were available. Given this limitation, the sample focused on an extended period of time such that the analysis would have enough statistical power to identify an effect. With respect to other gun laws, the author found no evidence of other major changes in legislation which could have affected the outcome of interest<sup>18</sup>. Additionally, the *NoDelay* states had unfavorable rates of handgun-related suicides compared to the *Delay* states, meaning that the repeal of restrictive delay laws was unrelated to the level of this variable. On the other hand, there is a multitude of different correlated shocks that could have been present in the *NoDelay* and *Delay* states throughout the duration of the panel (1999-2020) which could have potentially affected the overall rate of suicides, and concurrently the rate of handgun-related suicides. Still, the *NoDelay* states repealed delay laws in different calendar years but an increase in handgun-related suicide rates is observed for each state individually as can be seen in Table 7 and Figures 10a and 10b. Considering this information, it is unlikely that several different correlated shocks occurred for each state in different points of time influencing all of the state-specific post-treatment estimates. Nevertheless, the long time period of the study should be considered as a minor limitation of the identification strategy.

For Research Question 2, testing the prior parallel trend assumption is more straightforward since there was no differential treatment timing. The mean of the outcome variable

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<sup>17</sup>The results are statistically significant at the 5 percent level.

<sup>18</sup>Covered in more detail in Appendix C.

was plotted for the *NoDelay* and *Delay* states with respect to the years of the sample (2000 - 2009), as is shown in Figure 9. Looking at Figure 9, we see a converging trend in the rate of handgun-related suicides between 2001 and 2002. However, the outcome variable is stable, apart from this slight deviation in 2001. There is no compelling evidence of converging prior parallel trends. With respect to the absence of other policies and correlated shocks which could have affected our outcome of interest, the limiting factors are similar as for the previous research question. Since only yearly data were available, there could have been different correlated shocks during the time period of the panel. This sample took into account a larger number of states relative to the previous research question, which in turn makes it harder to assess all of the possible confounding events that could have occurred from 2000 until 2009. Furthermore, the larger number of states from different regions also opens up questions about geographic heterogeneity between the *NoDelay* and *Delay* states. Additionally, there were more significant differences in pre-treatment characteristics between the *NoDelay* and *Delay* states than in the previous research question. Finally, the sample only includes states that did not change delay laws, making it more difficult to claim that the *NoDelay* and *Delay* states are similar, knowing that there are underlying reasons why these states had different delay laws in place. Overall, the impact of the 2008 recession on suicide rates between states is complex and multifaceted. Therefore, it is likely that differential impacts of the recession on state-level economies and industries, combined with differences in cultural attitudes and other unobservable characteristics, could have contributed to differing suicide rates between states.

## 5 Results

### 5.1 Research question 1

The preliminary analysis shows how the repeal of restrictive delay laws affected handgun sales. Since one of the main assumptions of this paper is that the ease of obtaining a handgun greatly increases the likelihood that someone will kill themselves, a logical necessary precondition for that assumption to hold is that we should observe an increase in handgun sales once restrictive delay laws are repealed. Figure 4 shows this effect for each of the *NoDelay* states individually. The red line represents handgun background check (BGC) rates before delay laws were repealed. On the other hand, the light blue line represents handgun BGC rates after delay laws were repealed. As can be seen from the figure, all of the *NoDelay* states had increased rates of handgun BGCs after delay laws were repealed, except Michigan. Michigan registered a decrease in the first year after the repeal, but rebounded with a large increase in the subsequent years. This could be due to an unobserved shock or a general limitation of the data set. However, a more involved analysis is necessary to determine whether or not this increasing trend in the *NoDelay* states constituted an overall increase in handgun sales compared to handgun sales in the *Delay* states.

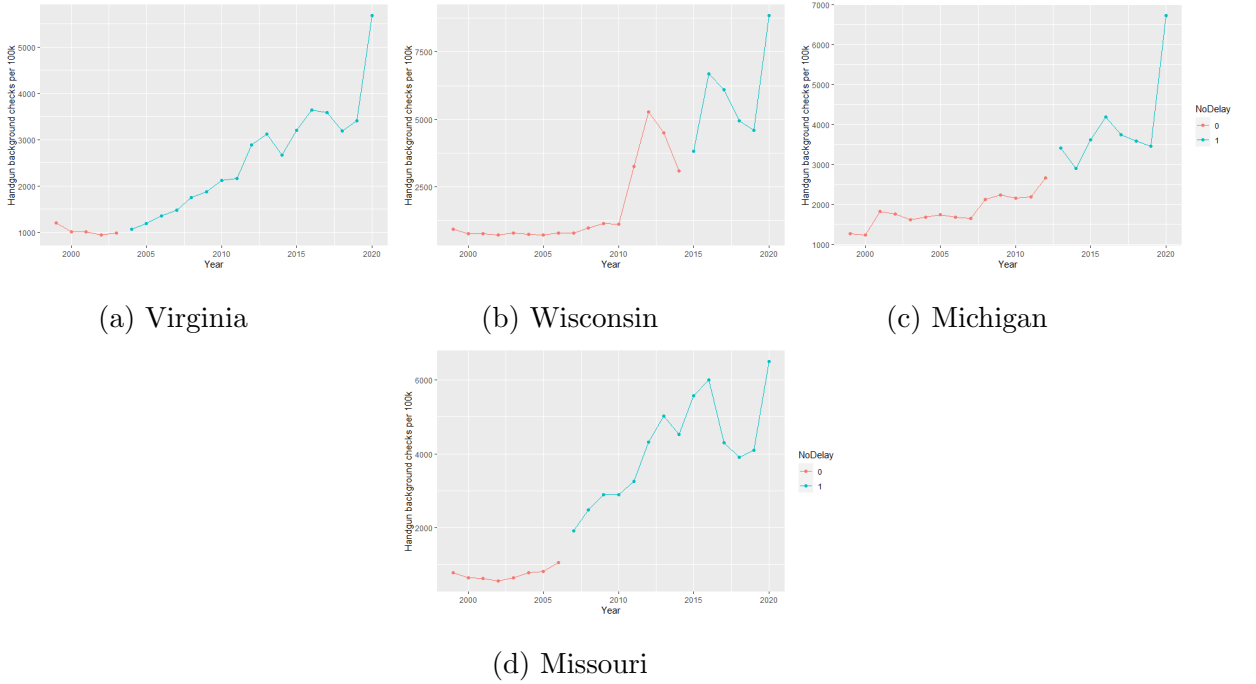


Figure 4: Handgun BGC rates in the *NoDelay* states (RQ1)



	Outcome variable: Log of handgun BGCs per 100,000 people				
	(1)	(2)	(3)	(4)	(5)
NoDelay	0.464	0.443	0.399	0.341	0.333
	[−0.072, 0.939]	[−0.130, 0.928]	[−0.075, 0.972]	[−0.161, 0.936]	[−0.183, 0.962]
	p = 0.166	p = 0.173	p = 0.161	p = 0.234	p = 0.276
Inference	WCSE	WCSE	WCSE	WCSE	WCSE
Num.Obs.	374	374	374	374	374
States	17	17	17	17	17
Std.Errors	by: State	by: State	State	by: State	by: State
FE: State	✓	✓	✓	✓	✓
FE: Year	✓	✓	✓	✓	✓
Controls	×	✓	×	✓	✓
State Pop. Weights	×	×	✓	✓	✓

**Notes:** Design of table inspired by Koenig and Schindler (2018). Confidence intervals are presented in square brackets. Additionally, p-values are presented for each estimate. The sample period is from 1999 until 2020. In Columns (2) and (4) included control variables are % rural and % only HS degree. In Column (5) included control variables are % rural, % only HS degree, % white population, and median income. All control variables are as of 2000 and interacted with Year FE. Levels of stat. significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ .

Table 5: Handgun BGCs (RQ1)

Table 5 captures this, and the regression output is from the estimation of Equation 3. The coefficient of interest is large and positive throughout all specifications, and the estimates range from a 39 to 59<sup>19</sup> percent increase in handgun BGC rates after delay laws were repealed. However, as can be seen from the p-values, the estimates are not statistically significant at any conventional level of significance. Therefore, the overall effect of repealing restrictive delay laws on handgun BGC rates ranges from a 16.7 percent decrease to a 164 percent increase, depending on the exact specification used. One of the likely reasons confidence intervals are wide is that the dependent variable is very noisy. Throughout the duration of the panel, multiple external shocks could have rapidly increased or decreased rates of handgun BGCs, which in turn decreased the precision of the estimates. As a final observation, these results could be partially driven by large increases in rates of handgun BGCs that are observed in each of the *NoDelay* states in 2020. Furthermore, pre-existing increasing trends in handgun BGC rates could have been present in the *NoDelay* states before delay laws were repealed. To capture this, Figure 5

<sup>19</sup>Throughout the analysis, overall effects were calculated using the following formula  $(e^a - 1) * 100$ , where  $a$  is the coefficient of the estimate of interest.

plots the effect of repealing delay laws on handgun BGC rates relative to the last period before the delay laws were repealed (event-time = -1). Observing Figure 5, there is limited

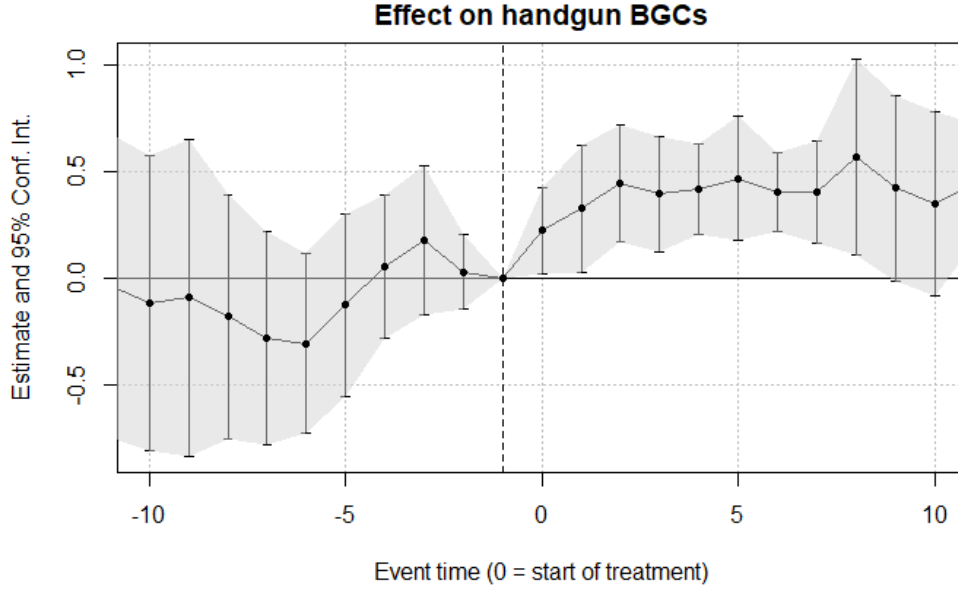


Figure 5: Event study plot - Handgun BGCs (RQ1)

evidence of pre-existing trends that could be driving the positive effect, since all of the event-time coefficients are extremely wide and there is no observable trend. Furthermore, it can be seen that there is a substantial increase in handgun BGC rates immediately after the delay laws were repealed since all of the early post-treatment event-time estimates are positive and statistically different from 0. Taking this information into account, the conclusion is that there is evidence of an increase in handgun sales in the *NoDelay* states after the delay laws were repealed, even when taking into account the marginally insignificant estimates observed in Table 5.

The continuation of this section focuses on the main results of Research Question 1 that were obtained estimating Equation 1. With respect to the estimation of Equation 1, this paper uses two estimation methods, TWFE and CS. The results will focus on each of these estimation methods in turn. Table 6 shows the results using the TWFE estimator. The regression output shows that the repeal of the delay laws had a large effect on handgun-related suicide rates. Column 1 shows that the repeal of delay laws in *NoDelay* states lead to an increase in handgun-related suicide rates by 25.6 percent. The result is statistically significant at the 5 percent level ( $p\text{-value} = 0.032$ ), with the overall effect ranging from a 1.5 to a 61 percent increase in handgun-related suicide rates. Similarly, with the addition of controls in Column 2 and Column 3, the estimates remain large and

	Outcome variable: Log of ...-related suicides per 100,000 people				
	Handgun		All	Non-handgun	
	(1)	(2)	(3)	(4)	(5)
NoDelay	0.228**	0.133*	0.134**	0.036	-0.067**
	[0.015, 0.475]	[-0.012, 0.328]	[0.010, 0.295]	[-0.046, 0.116]	[-0.156, -0.011]
	p = 0.032	p = 0.066	p = 0.041	p = 0.251	p = 0.017
Inference	WCSE	WCSE	WCSE	WCSE	WCSE
Num.Obs.	374	374	374	374	374
States	17	17	17	17	17
Std.Errors	by: State	by: State	by: State	by: State	by: State
FE: State	✓	✓	✓	✓	✓
FE: Year	✓	✓	✓	✓	✓
Controls	×	✓	✓	×	×

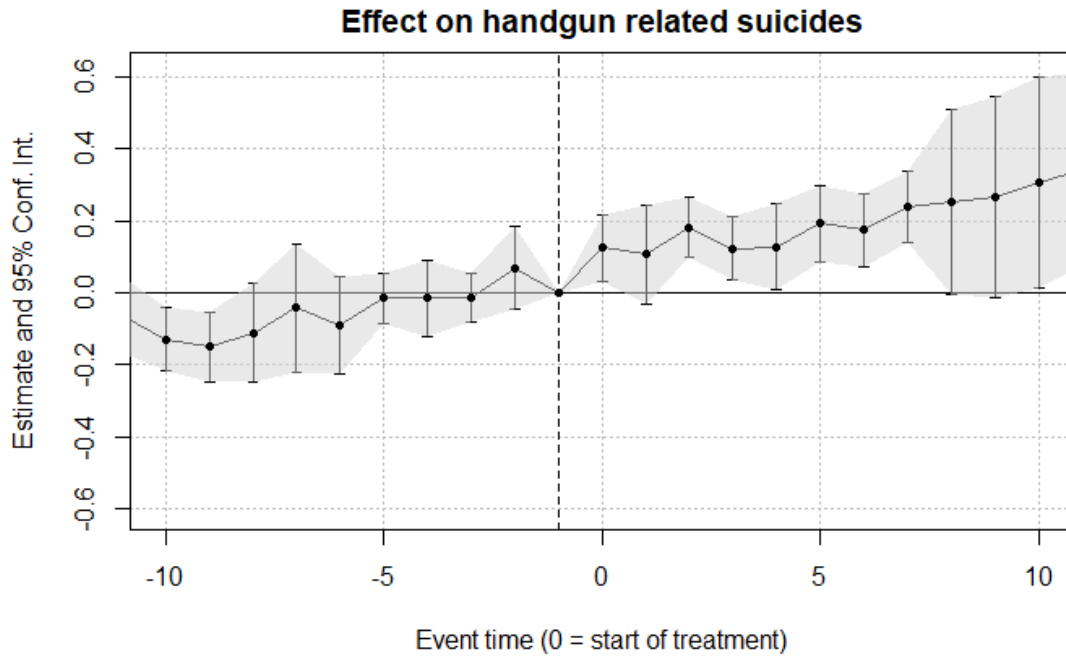
**Notes:** Design of table inspired by Koenig and Schindler (2018). Confidence intervals are presented in square brackets. Additionally, p-values are presented for each estimate. The sample period is from 1999 until 2020. In Column (2) included control variables are % rural and % only HS degree. In Column (3) included control variables are % rural, % only HS degree, % white population, and median income. All control variables are as of 2000 and interacted with Year FE. All regressions are weighted by state population. Levels of stat. significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ .

Table 6: Baseline results (TWFE,RQ1)

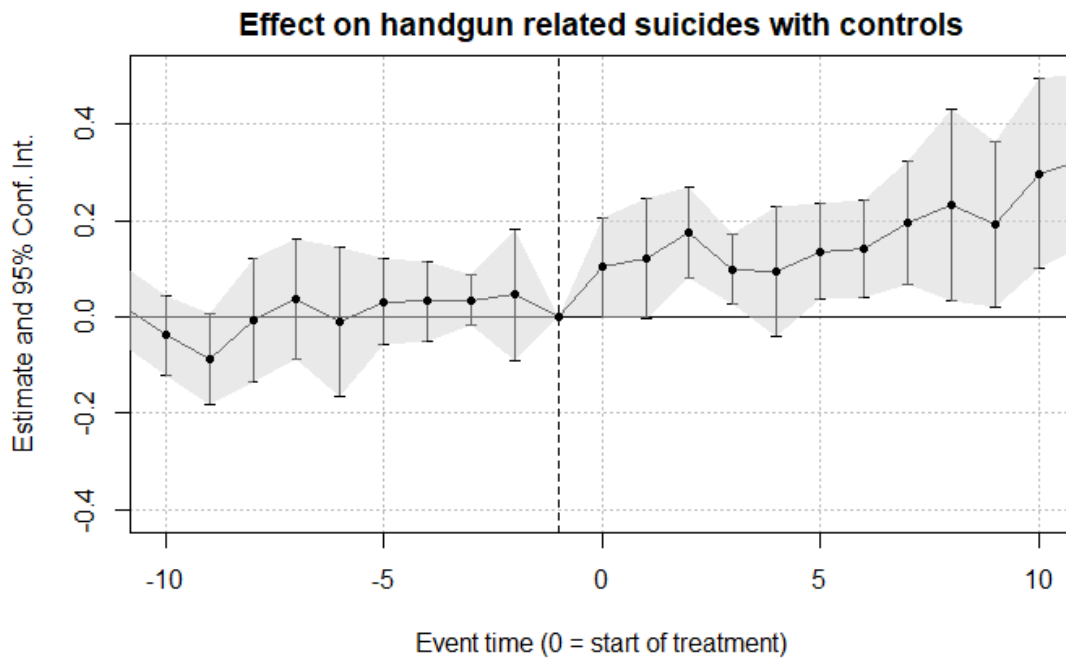
statistically significant, suggesting a 14.3 percent increase in handgun-related suicide rates. To investigate the net effect on suicide rates and the possibility of substitution between handguns and other means of suicide, Columns 4 and 5 show the results of Equation 1 with two different dependent variables: the log of the overall suicide rate per 100,000 people and the log of the non-handgun-related suicide rate per 100,000 people. As can be seen from Column 4, the positive estimate suggests that there was an increase in overall suicide rates in the *NoDelay* states by around 3.6 percent. However, the estimate is statistically insignificant and the effect ranges from a 4.5 percent decrease to a 12.2 percent increase in overall suicide rates. On the other hand, Column 5 shows that there was a 6.5 percent decrease in non-handgun-related suicide rates in the *NoDelay* states. The result is statistically significant at the 5 percent level with the overall effect ranging from a decrease of 1.1 to 14.4 percent. These findings suggest that substitution is occurring in which suicidal individuals shift towards using handguns instead of less lethal means, once handguns are more easily available. This in turn drives an increase in the overall suicide

rate, most likely due to the higher level of lethality associated with handguns. However, the estimate of interest with respect to overall suicide rates is imprecise and no definitive conclusion can be reached regarding the overall net effect.

Before focusing on the results estimated by the CS estimator, the analysis will check how the effect varied in the immediate years after the delay laws were repealed. The focus will be on interpreting the effect in the immediate post-treatment years since the identifying assumptions and the pre-treatment estimates were discussed in great detail in Section 4.3. Figures 6a and 6b use the event-study specification as stipulated in Equation 2. We can see that all post-treatment event-time coefficients have positive estimates, meaning that there was an increase in handgun-related suicide rates immediately after the delay laws were repealed. With respect to statistical significance, the majority of post-treatment confidence intervals do not include 0 and, as such, are statistically significant. However, a limiting factor in this analysis is that these estimates use clustered standard errors that most likely underestimate the standard errors, hence, the confidence interval bounds are smaller. With respect to this, a more precise look at each of the post-treatment event-study coefficients will be covered by means of the CS estimator in the following pages. To conclude, findings from the TWFE estimator provide evidence that the repeal of restrictive delay laws led to an increase in handgun-related suicide rates in the *NoDelay* states. Given the results from Tables 5 and 6, a back of the envelope elasticity calculation suggests that a 1 percent increase in handgun sales led to a 0.52 percent increase in handgun-related suicides. Furthermore, the preliminary findings are robust to the inclusion of control variables and all of the post-treatment event-time coefficients have positive estimates. This provides further evidence of an increase in handgun-related suicide rates after delay laws were repealed. The results of Table 6 also suggest that this increase in handgun-related suicide rates is attributable to the substitution effect, meaning that individuals tend to use more lethal means if they are more easily available. However, no evidence was found of a subsequent increase of overall suicide rates in the *NoDelay* states, meaning the net-effect of repealing delay laws remains uncertain.

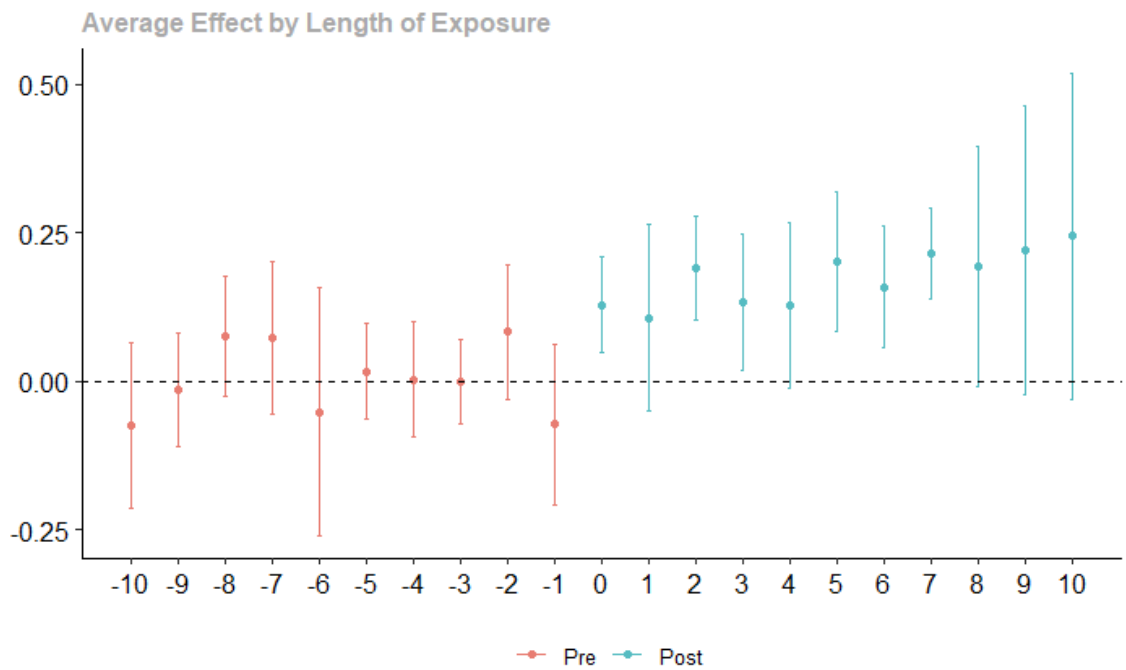


(a) Main specification

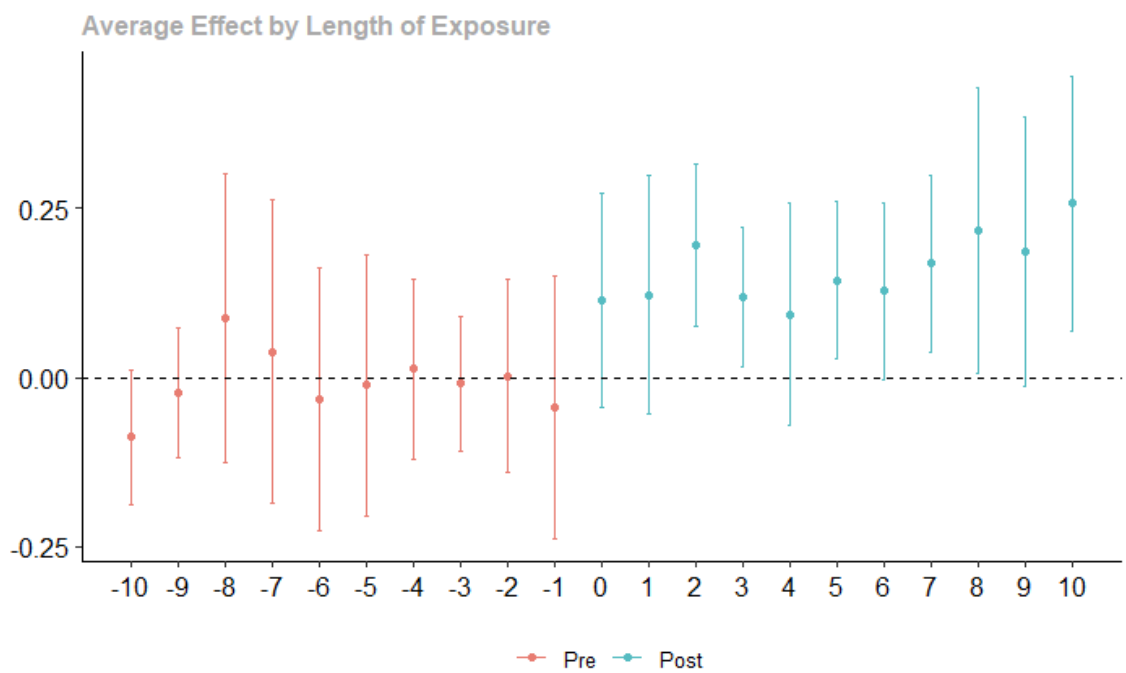


(b) Main specification with controls

Figure 6: Baseline event study plots (TWFE, RQ1)



(a) Unconditional parallel trends



(b) Conditional parallel trends

Figure 7: Baseline event study plots (CS, RQ1)

Continuing, the following table and figures provide results that have come by way of the new CS estimator. Figures 7a and 7b show the aggregate event-time average treatment effects, with the blue dots representing how these effects evolved over event-times  $\tau$ . The post-treatment results are similar to the TWFE event-study plots as in Figures 6a and 6b. All of the post-treatment estimates are large and positive. Therefore, it is clear that there is an immediate and persistent increase in handgun-related suicide rates after delay laws were repealed. Furthermore, Table 7 shows the aggregated treatment effects, with Panel (a) showing the results based on the unconditional parallel trends assumption and Panel (b) showing the results based on the conditional parallel trends assumption, i.e., after conditioning on observed pre-treatment characteristics. The results from both panels are split between group-specific and event-study average treatment effects. Group-specific treatment effects in this study are the treatment effects for each group depending on the timing of treatment. Since each state changed treatment status in a distinct year, these can be viewed as treatment effects for each state individually. Furthermore, the event-study treatment effects show the average treatment effect relative to the start of treatment. Interpreting the results from Table 7, the first row of Panel (a) shows the results of the aggregated group-time average treatment effects. As can be seen from the regression output, all the *NoDelay* states show a large and statistically significant increase in handgun-related suicide rates. To be exact, the estimates show an increase in handgun-related suicide rates by 13.25 percent in Virginia, 26 percent in Missouri, 18.1 percent in Michigan, and 38.6 percent in Wisconsin. Furthermore, the overall aggregated average treatment effect estimate also shows a large and statistically significant effect, suggesting a 22 percent increase in handgun-related suicide rates. The second row of Panel (a) shows the first four aggregated event-time estimates. All four reported event-time estimates are large and positive ranging from a 11.2 to a 21 percent increase in handgun-related suicide rates. Importantly, the first period relative to the repeal of restrictive delay laws (event-time = 0) suggests an increase in the rate of handgun-related suicides of 13.7 percent in *NoDelay* states. In terms of statistical significance, only the estimate for event-time 1 is not statistically significant. However, this is most likely due to the limited power of each event-time estimate, since there are only four *NoDelay* states. To counteract this problem, Table 7 also provides an overall aggregate estimate of the average event-time treatment effects from periods  $\tau = 0$  to  $\tau = 10$ . The aggregate estimate suggests a 19 percent increase in handgun-related suicide rates in the *NoDelay* states after the delay laws were repealed.

Outcome variable: Log of handgun-related suicides per 100,000 people					
(a) Unconditional parallel trends					
	Partially aggregated				Single parameters
Group-specific effects	<u>Virginia</u>	<u>Missouri</u>	<u>Michigan</u>	<u>Wisconsin</u>	
	0.1245*	0.2311*	0.1671*	0.3266*	0.1998*
	(0.0407)	(0.0317)	(0.0308)	(0.0166)	(0.0246)
Event study	<u>e=0</u>	<u>e=1</u>	<u>e=2</u>	<u>e=3</u>	
	0.1285*	0.1062	0.1906*	0.1338*	0.1742*
	(0.0475)	(0.0611)	(0.0393)	(0.0464)	(0.0481)
(b) Conditional parallel trends					
	Partially aggregated				Single parameters
Group-specific effects	<u>Virginia</u>	<u>Missouri</u>	<u>Michigan</u>	<u>Wisconsin</u>	
	0.1886*	0.1777	0.1014*	0.2779*	0.1725*
	(0.0473)	(0.1011)	(0.0467)	(0.0462)	(0.0461)
Event study	<u>e=0</u>	<u>e=1</u>	<u>e=2</u>	<u>e=3</u>	
	0.1140	0.1228	0.1959*	0.1200*	0.1589*
	(0.0587)	(0.0745)	(0.0476)	(0.0439)	(0.0458)

**Notes:** Design of table inspired by Callaway and Sant’Anna (2021). The sample period is from 1999 until 2020, with a total of 374 observations from 17 states. Standard errors computed using the multiplier bootstrap. Provided in parenthesis & clustered at the state level. Results of Panel (a) are based on the unconditional parallel trends assumption. Results of Panel (b) are based on the conditional parallel trends assumption. Conditional on the following variables: % rural, % only HS degree, % white population, and median income. All variables are as of 2000. Levels of stat. significance: \*  $p < 0.05$ . All regressions are weighted by state population. Panel (a) and Panel (b) estimates are generated using the outcome regression estimator. Comparison group: Only “Never-Treated” units.

Table 7: Baseline results (CS, RQ1)

As a final addition, Panel (b) of Table 7 also provides group-specific and event-study treatment effects after conditioning on the following pre-treatment covariates: percentage of population with only a high-school degree, percentage of rural households, median income, and percentage of white population. Group-specific treatment effects are presented in the first row of Panel (b). We can see that the large effect persists for all of the *NoDelay* states suggesting a large increase in handgun-related suicide rates. To be more specific, the estimate for Virginia shows a somewhat larger effect than in Panel (a), suggesting a 20.7 percent increase in the rate of handgun-related suicides. On the other hand, the estimates for Missouri, Michigan, and Wisconsin are somewhat more conservative than in Panel (a). The more conservative estimates also drive down the overall aggregate group-specific treatment effect, which now provides evidence of a 18.8 percent increase in handgun-related suicide rates. All group-specific average treatment effects are statistically



significant at the 5 percent level with the exception of Missouri. Similarly, the first four event-time average treatment effects are presented in the second row of Panel (b). As can be seen, there are only slight differences between the results of Panel (a) and Panel (b). The most distinguishable difference is that event-time 0 is now marginally statistically insignificant at the 5 percent level. Additionally, the overall aggregate treatment effect is also smaller, suggesting a 17.2 percent increase in handgun-related suicide rates. To conclude, the general results of this section provide evidence of an increase in handgun-related suicide rates after the *NoDelay* states repealed restrictive delay laws by between 17.2 and 25.6 percent.

### 5.1.1 Robustness checks

This subsection focuses on robustness checks with respect to the findings of the first research question. Columns 1 and 2 of Table 8 provide estimates of the main specification according to Equation 1, but now without state population weighting. As can be seen from the regression output, without controls, the estimate in Column 1 is still large and statistically significant at the 10 percent level, suggesting an increase in handgun-related suicide rates by 20.5 percent. Furthermore, when including the full set of control variables in Column 2, the estimate remains large, suggesting an increase in handgun-related suicide rates by 12.4 percent. Additionally, the results are now statistically significant at the 5 percent level. Resuming, one possible mechanism that could be driving the treatment effect are greater pre-existing firearm prevalence rates in the *NoDelay* states. To try to control for this problem, Columns 3 and 4 show the estimation of the main specification according to Equation 1 with the addition of the interaction between the before-mentioned firearm prevalence control variable and year-fixed effects. The only difference between Columns 3 and 4 is that the latter also includes the full set of control variables<sup>20</sup>. As can be seen, the estimates of interest in both columns still suggest large positive increases in handgun-related suicide rates of 15.6 and 11.8 percent. Furthermore, the estimate in Column 3 is statistically significant at the 10 percent level (p-value = 0.082). On the other hand, with the additional inclusion of the full set of control variables, the estimate in Column 4 becomes statistically insignificant (p-value = 0.173). This provides evidence that the effect persists even when trying to control for pre-existing firearm prevalence rates. Nevertheless, it is important to remember the limitations of the data on firearm prevalence, as mentioned in Section 3.3. To try to address possible concerns of geographic heterogeneity between the

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<sup>20</sup>Interacted with Year FE.

	Outcome variable: Log of ...-related suicides per 100,000 people						
	Handgun						Long-gun
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NoDelay	0.187*	0.117**	0.145*	0.112	0.123	0.125**	-0.049
	[-0.017, 0.357]	[0.013, 0.241]	[-0.022, 0.360]	[-0.051, 0.292]	[-0.103, 0.428]	[0.010, 0.373]	[-0.292, 0.126]
	p = 0.073	p = 0.032	p = 0.082	p = 0.173	p = 0.230	p = 0.046	p = 0.603
Inference	WCSE	WCSE	WCSE	WCSE	WCSE	WCSE	WCSE
Num.Obs.	374	374	374	374	154	324	374
States	17	17	17	17	7	16	17
Std.Errors	by: State	by: State	by: State	by: State	by: State	by: State	by: State
FE: State	✓	✓	✓	✓	✓	✓	✓
FE: Year	✓	✓	✓	✓	✓	✓	✓
Controls	×	✓	✓	✓	×	×	×
State Pop. Weights	×	×	✓	✓	✓	✓	✓

**Notes:** Design of table inspired by Koenig and Schindler (2018). Confidence intervals are presented in square brackets. Additionally, p-values are presented for each estimate. The sample period is from 1999 until 2020. In Column (2) included control variables are % rural, % only HS degree, % white population, and median income. In Column (3) included control variable is % of respondents who reported living in a household with a firearm. In Column (4) included control variables are % of respondents living in a household with a firearm, % rural, % only HS degree, % white population, and median income. All control variables are as of 2000 (with the exception of % of respondents living in a household with a firearm, which is of 2004) and interacted with Year FE. Column (5) looks at a limited sample of only states from the Midwest region. Column (6) presents the results of a placebo test in which the post-treatment years of *NoDelay* states were excluded and a fake treatment was inputted in the middle of the remaining pre-treatment years. Levels of stat. significance: \* p < 0.1, \*\* p < 0.05.

Table 8: Robustness checks (RQ1)

*NoDelay* and *Delay* states in the original sample, Column 5 shows the results of Equation 1 after limiting the sample to only states from the Midwest region<sup>21</sup>. The results suggest a 13 percent increase in handgun-related suicide rates after delay laws are repealed in the *NoDelay* states. However, the result is not statistically significant at any conventional level of significance, and the confidence interval is wide, with the effect ranging from an 8.7 decrease to a 40 percent increase in handgun-related suicide rates. Since this sample is more limited in the number of observations, the statistical power is much lower, which could explain why the results are not statistically significant. Column 6 provides the estimation results of a placebo test which was implemented by removing post-treatment years for all of the *NoDelay* states and consequently imputing a fake treatment period in the middle of the remaining pre-treatment years. In this robustness test, Virginia was dropped from the sample since treatment started in 2004 and there were not enough pre-

<sup>21</sup>The original sample takes into account states from the following 4 regions: Midwest, Northeast, South, and West. However, 3 (Michigan, Missouri, Wisconsin) out of the 4 *NoDelay* states are in the Midwest region. Valid concerns could arise that the geographic heterogeneity between *NoDelay* and *Delay* states could be driving some of the results, even after using state-fixed effects in every specification. The limited sample that was used in Column 5 includes the following states from the Midwest region: Illinois, Iowa, Michigan, Minnesota, Missouri, Nebraska, and Wisconsin.

treatment years to conduct this test. This test was carried out as an additional evaluation of the untestable parallel trends assumption and to assess whether pre-treatment trends could be driving the results observed in Table 7. The intuition is that there should not be a positive and statistically significant effect in this fake treatment period. As can be seen in Column 6, the estimate suggests a 13.3 percent increase in handgun-related suicide rates, which is statistically significant at the 5 percent level. These findings are suggestive of pre-treatment trends, which could be driving part of the effect observed in the main specifications above. To further analyze this finding, Figure 8 provides graphical evidence of this placebo test using an event study specification. The figure suggests a positive effect immediately after the start of the fake

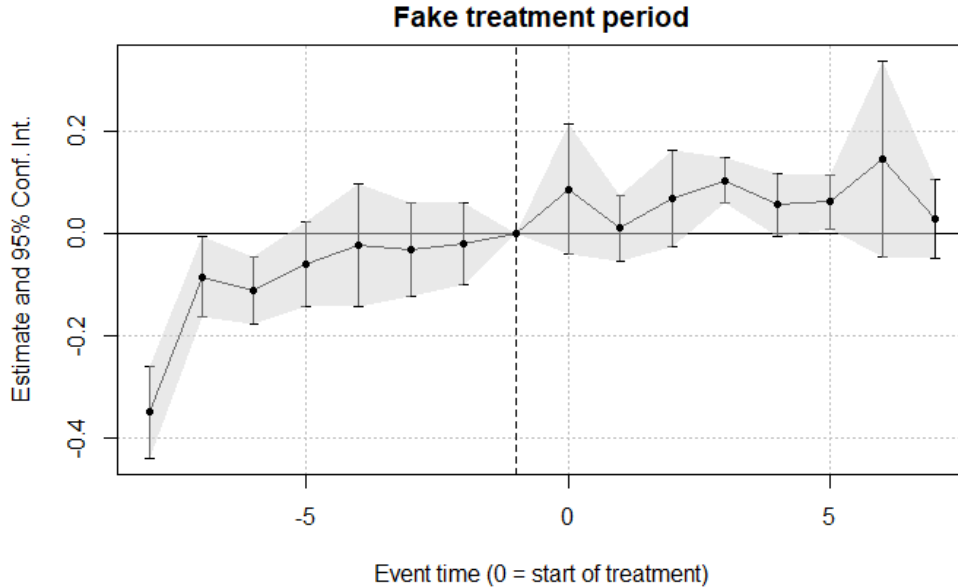


Figure 8: Placebo test (RQ1)

treatment period. This provides further evidence that pre-treatment trends could be driving some of the observed results. Finally, since the variation in delay laws focused specifically on handgun-related legislation, it would be counter-intuitive if there was a large increase in long-gun-related suicide rates in the *NoDelay* states. Following this logic, Column 7 provides evidence on the effect of repealing handgun delay laws on long-gun-related suicide rates. The estimate of interest is negative and statistically insignificant at any relevant level of significance ( $p\text{-value} = 0.603$ ).

Regarding all robustness checks performed, the general conclusion is that the results of Research Question 1 are robust with respect to the control variables used, the exclusion of state population weighting, and to limited geographic samples. However, findings from the

above-mentioned placebo test can be considered as a limiting factor of the identification strategy. Even though there was a large increase in handgun-related suicide rates in the *NoDelay* states once the restrictive delay laws were repealed, it is unclear how much of this effect was driven by pre-treatment trends. Therefore, the results should be interpreted with this limitation in mind. Moreover, Table 11 and Figures 11a and 11b in Appendix A.1 present the results from the CS estimator without state population weights. All of the overall aggregate estimates still suggest a large increase in handgun-related suicide rates in the *NoDelay* states after delay laws were repealed by between 14.5 to 19 percent.

## 5.2 Research question 2

This section focuses on the main results of Research Question 2, which were obtained by estimating Equation 4. Figure 9 shows how the mean of the outcome variable evolved over time. Again, the pre-treatment evolution was covered in detail in Section 4.3, hence, the discussion in this section will focus on the *Post* years and how handgun-related suicide rates evolved in the years of the 2008 economic recession. As can be seen, both the *NoDelay* and *Delay* states registered an increase in handgun-related suicide rates immediately after 2007, which is the last year before the start of the recession.

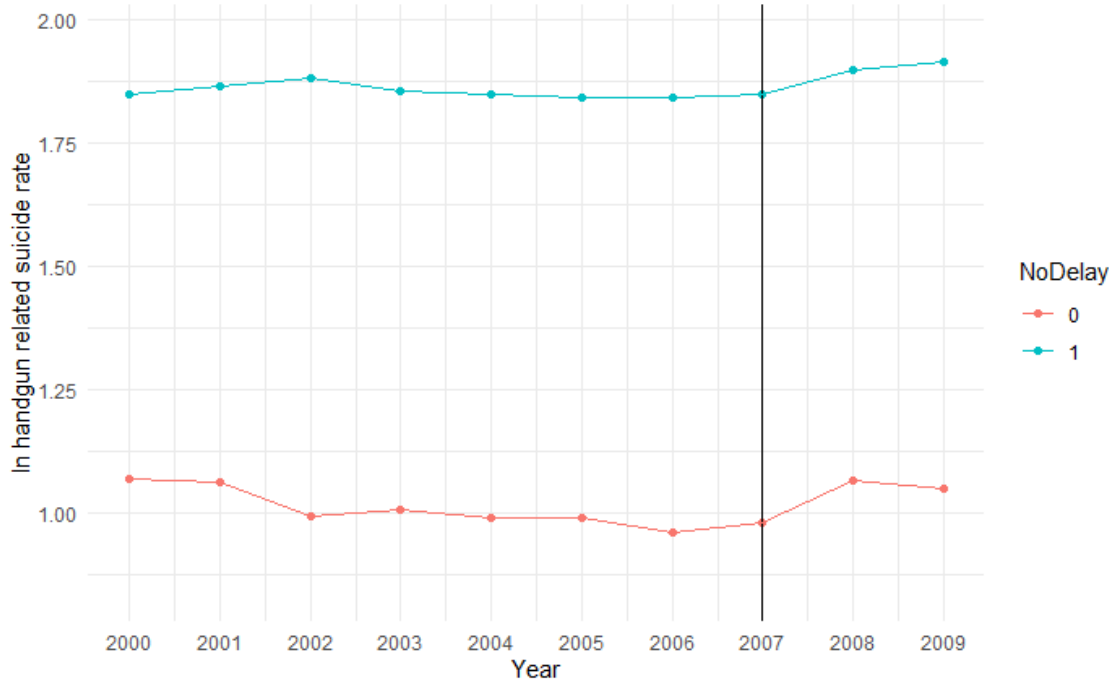


Figure 9: Evolution of outcome variable (RQ2)

Table 9 shows the results of the estimation of Equation 4. Column 1 shows that handgun-related suicide rates increased by 3.8 percent in the *NoDelay* states during the 2008 recession. The estimate is significant at the 5 percent level (p-value = 0.032), and the overall effect ranges from a 0.3 to 7.5 percent increase in handgun-related suicide rates. The results from Columns 2 and 3 show the results with the inclusion of control variables. The estimates are still positive, and the inclusion of the full set of control variables in Column 3 suggests a 5 percent increase in handgun-related suicide rates in the *NoDelay* states during the 2008 recession. However, the estimates lose precision, with p-values of 0.12 and 0.062, respectively. Since this research question zooms in on the effect of the 2008 recession, one possible factor that could have affected handgun-related suicide rates was differing unemployment rates between the *NoDelay* and *Delay* states. During the 2008 recession, the *NoDelay* states had an average unemployment rate of 7.77 percent compared to 8.27 percent in the *Delay* states. The pre-recession unemployment rates were on average 4.8 percent in both the *NoDelay* and *Delay* states. To control for this, Column 4 explores what happens to the estimate of interest when the yearly state-level unemployment rate is added as a control variable to Equation 4. Importantly, this was done for purely exploratory purposes, given that the author is aware of the already mentioned “bad control” problem that can occur when adding time-varying controls. From Column 4, it can be seen that the estimate of interest increases marginally, suggesting a 4.3 percent increase in handgun-related suicide rates in the *NoDelay* states during the 2008 recession. Furthermore, the estimate gains precision and is statistically significant at the 5 percent level, with the overall effect ranging from a 0.7 to 8.2 percent increase. The coefficient<sup>22</sup> of the yearly unemployment rate has been suppressed from Table 9.

As in the previous research question, it is important to look at the net effect on suicide rates and how non-handgun-related suicide rates evolved between the *NoDelay* and *Delay* states during the 2008 recession. This was estimated using a modified version of Equation 4 where the dependent variables used were the log of the overall suicide rate per 100,000 people (Column 4) and the log of the non-handgun-related suicide rate per 100,000 people (Column 5). As we can see in Column 4, during the 2008 recession overall suicide rates decreased by 3.5 percent in the *NoDelay* states compared to the *Delay* states. Similarly, non-handgun-related suicide rates also decreased by 5 percent. Both coefficients are statistically significant at the 5 percent level with p-values of 0.033 and 0.046, respectively. The results are suggestive of individuals in the *NoDelay* states choosing handguns

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<sup>22</sup>The coefficient of the yearly unemployment rate was marginally positive with a value of 0.008.

	Outcome variable: Log of ...-related suicides per 100,000 people					
	Handgun				All	Non-handgun
	(1)	(2)	(3)	(4)	(5)	(6)
NoDelay x Post	0.038**	0.033	0.050*	0.043**	-0.032**	-0.047**
	[0.003, 0.073]	[-0.009, 0.074]	[-0.003, 0.103]	[0.007, 0.079]	[-0.061, -0.003]	[-0.093, -0.001]
	p = 0.032	p = 0.120	p = 0.062	p = 0.021	p = 0.033	p = 0.046
Inference	Clustered SE	Clustered SE	Clustered SE	Clustered SE	Clustered SE	Clustered SE
Num.Obs.	460	460	460	460	460	460
States	46	46	46	46	46	46
Std.Errors	by: State	by: State	by: State	by: State	by: State	by: State
FE: State	✓	✓	✓	✓	✓	✓
FE: Year	✓	✓	✓	✓	✓	✓
Controls	×	✓	✓	✓	×	×

**Notes:** Design of table inspired by Koenig and Schindler (2018). Confidence intervals are presented in square brackets. Additionally, p-values are presented for each estimate. The sample period is from 2000 until 2009. In Column (2) included control variables are % rural and % only HS degree. In Column (3) included control variables are % rural, % only HS degree, % white population, and median income. All control variables are as of 2000 and interacted with Year FE. In Column (4), the only included control variable is the time-varying state level unemployment rate which is compiled for the years from 2000 until 2009. All regressions are weighted by state population. Levels of stat. significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ .

Table 9: Baseline results (RQ2)

as their preferred means of suicide since they were more readily available. Comparably, in the absence of handguns, citizens of the *Delay* states highly favored non-handgun-related means of suicide. Furthermore, the overall rate of suicide decreased in the *NoDelay* states compared to the *Delay* states.

To conclude, there is evidence that handgun-related suicide rates increased in the *NoDelay* states compared to the *Delay* states during the 2008 recession. However, compared to the *Delay* states, there was an overall decrease in both non-handgun-related and overall suicide rates. The observed decrease in overall suicide rates is suggestive of a potentially differential reaction to the 2008 recession between the *NoDelay* and *Delay* states. The recession could have affected different states in the United States to varying degrees since the impact on individual states was largely influenced by the state of their housing markets, the strength of their local economies, the diversity of their industries, and the policy responses at the state level. Knowing that the *NoDelay* states had on average lower unemployment rates during the recession and that more people lived in rural areas, it is possible that the 2008 recession did not affect the *NoDelay* and *Delay* states with the same intensity. Therefore, the results in this section should be interpreted with caution.

### 5.2.1 Robustness checks

This subsection focuses on the robustness checks conducted with respect to the findings of Research Question 2. The first robustness check excludes population weighting when estimating Equation 4. As can be seen in Columns 1 and 2 of Table 10, population weighting is necessary to capture the effects observed in Table 9, since both estimates are around 0 and statistically insignificant at any relevant level of significance. This is most likely due to the large differences in population between the *NoDelay* and *Delay* states in this sample. Again, as in the previous research question, Columns 3 and 4 show the estimation of the main specification according to Equation 4 with the addition of the interaction between the firearm prevalence control variable and year-fixed effects. The only difference between Columns 3 and 4 is that the latter also includes the full set of control variables. Both estimates are positive, but with different levels of statistical significance. As can be seen in Column 4, with the addition of the full set of control variables and the proxy variable for firearm prevalence, the estimate suggests an increase in handgun-related suicide rates by around 6 percent. The result is marginally statistically insignificant at the 5 percent level ( $p\text{-value} = 0.051$ ). Furthermore, a placebo test was performed to test whether the observed increase in handgun-related suicide rates in the *NoDelay* states was due to the impact of the 2008 recession or due to an already increasing trend in handgun-related suicide rates. The author used data from 1999 to 2007, in which a fake “Post” period was inputted for the years 2006 and 2007. The sample was otherwise the same, except for the exclusion of one additional state, Alabama. This was because Alabama changed treatment status in 1999. The results of this placebo test are presented in Column 5. The estimate is positive and suggests a 3.2 percent increase in handgun-related suicide rates during the fake “Post” period. Although the estimate is statistically insignificant, the upper and lower confidence interval bounds are similar to those found in the main specification, with the effect ranging from a 1.1 decrease to a 7.8 percent increase in handgun-related suicide rates. These findings question the validity of the identification strategy used, since there is no intuitive reason for a fake post-period to have the same impact on handgun-related suicide rates as the 2008 recession. However, the estimate is statistically insignificant, so no definitive conclusion can be made. Extending the analysis, Column 6 shows the results of Equation 4 without the exclusion of Alaska, Missouri, and Virginia<sup>23</sup>. As can be seen, the results are statistically significant at the 5 percent level and

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<sup>23</sup>Missouri and Virginia were excluded because they changed treatment status during the sample period. Alaska was excluded due to large variations in the dependent variable. Pennsylvania remains excluded due to the differing classifications of waiting periods between the two main firearm law databases.

	Outcome variable: Log of handgun-related suicides per 100,000 people						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	NoDelay x Post	0.002	-0.009	0.031	0.058*	0.032	0.034**
		[-0.058, 0.062]	[-0.106, 0.089]	[-0.017, 0.078]	[0.000, 0.117]	[-0.011, 0.076]	[0.000, 0.069]
		p = 0.948	p = 0.860	p = 0.200	p = 0.051	p = 0.140	p = 0.050
Inference	Clustered SE	Clustered SE	Clustered SE	Clustered SE	Clustered SE	Clustered SE	WCSE
Num.Obs.	460	460	460	460	405	490	190
States	46	46	46	46	45	49	19
Std.Errors	by: State	by: State	by: State	by: State	by: State	by: State	by: State
FE: State	✓	✓	✓	✓	✓	✓	✓
FE: Year	✓	✓	✓	✓	✓	✓	✓
Controls	×	✓	✓	✓	×	×	×
State Pop. Weights	×	×	✓	✓	✓	✓	✓

**Notes:** Design of table inspired by Koenig and Schindler (2018). Confidence intervals are presented in square brackets. Additionally, p-values are presented for each estimate. The sample period is from 2000 until 2009 in Columns (1)-(4) & (6)-(7), and from 1999 until 2007 in Column (5). In Column (2) included control variables are % rural, % only HS degree, % white population, and median income. In Column (3) included control variable is % of respondents who reported living in a household with a firearm. In Column (4) included control variables are % of respondents living in a household with a firearm, % rural, % only HS degree, % white population, and median income. All control variables are as of 2000 (with the exception of % of respondents living in a household with a firearm which is of 2004) and interacted with Year FE. Column (5) looks at fake post period which was inputted for years 2006 and 2007. Column (6) looks at the full sample without the exclusion of Alaska, Missouri and Virginia. Column (7) looks at a limited sample of states from the Midwest and Northeast regions. Levels of stat. significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ .

Table 10: Robustness checks (RQ2)

suggest a 3.4 percent increase in handgun-related suicide rates. Finally, given that this research question encompasses a large number of states from different regions, concerns of geographic heterogeneity between the *NoDelay* and *Delay* states could be reasonable. To deal with this, Column 7 shows the results of Equation 4 after limiting the sample to only states from the Midwest and Northeast region. The reason why these regions were selected is that most of the *Delay* states are from these two regions. The positive estimate suggests a 3.4 percent increase in handgun-related suicide rates in the *NoDelay* states during the 2008 recession. However, the results are statistically insignificant at any conventional level of significance, most likely because of the small sample size. Additionally, in this specification, the author used WCSE given the smaller number of states. Finally, Table 13 in Appendix A.2 provides the baseline results as in Table 9, but with WCSE instead of clustered standard errors. This was done as an additional check on the validity of the results, and it can be seen that there are only marginal changes to the levels of statistical significance.

With respect to the robustness checks performed in this section, the findings of Research Question 2 are dependent on the regressions being weighed by the state population. Furthermore, the estimates remain positive with different samples and control variables,



but vary with respect to the levels of statistical significance observed. On the other hand, a placebo test suggested a minor positive effect in the fake “Post” period. However, the estimate was statistically insignificant and no definite conclusion can be made with respect to the results from the placebo test performed. Overall, the results suggest an increase in handgun-related suicide rates in the *NoDelay* states during the 2008 recession. However, as mentioned above, the results should be interpreted with caution given the possibly differential reaction to the 2008 recession between the *NoDelay* and *Delay* states.

## 6 Conclusion and Discussion

The high rate of handgun-related suicides is an area of major concern in the United States, and understanding the underlying mechanisms driving this phenomenon becomes ever more urgent. The availability and accessibility of handguns, particularly among people who may be at risk of suicide, is a major contributing factor to the high rates of handgun-related suicides. To curb this problem, policymakers have several tools at their disposal, one of which is imposing more restrictive firearm purchase delay laws. However, the overall evidence of whether having more restrictive gun control legislation leads to better outcomes with respect to suicides is still inconclusive. The main objective of this paper was to provide further evidence on this topic through the analysis of two research questions.

The first and main research question focused on exploiting the variation created when four states repealed restrictive firearm purchase delay laws. The results of the first research question provide evidence of a large increase in handgun-related suicide rates after the restrictive delay laws were repealed. These results were obtained using two different estimation methods, the two-way fixed effects estimator and the Callaway and Sant’Anna (2021) estimator. Both estimation methods showed substantial increases in handgun-related suicide rates after the repeal of the respective delay laws. Besides increasing rates of handgun-related suicides, a statistically significant decrease in non-handgun related suicide rates was observed, suggesting that substitution occurs, where suicidal persons turn to handguns as their preferred method of suicide once they are made easier to obtain. However, there is limited evidence that the increase in handgun-related suicide rates and subsequent substitution contributed to an overall increase in the rate of suicides within the *NoDelay* states. Although the results are robust to multiple different robustness checks, there is some indication that the results could be driven by an already increasing trend of handgun-related suicide rates in the *NoDelay* states which occurred in the years prior to the repeal of delay laws.

The second research question zoomed in on a particularly vulnerable time for citizens of the United States, the 2008 recession. It tried to answer whether there were any differences between the *NoDelay* and *Delay* states with respect to handgun-related suicide rates in this period. Evidence of an increase in handgun-related suicide rates was found in the *NoDelay* states during the 2008 recession. However, the results are smaller in magnitude and are not as robust as in the previous research question, depending on the exact specification used. Furthermore, even with an increase in handgun-related suicide

rates, overall suicide and non-handgun-related suicide rates decreased in the *NoDelay* states compared to the *Delay* states during the 2008 recession. This implies that the 2008 recession likely had a differential impact between the *NoDelay* and *Delay* states which were different in both observable and unobservable characteristics. Given this information, the increase in handgun-related suicide rates in the *NoDelay* states during the 2008 recession cannot be competently interpreted in terms of the mechanisms which caused this change.

The general conclusion of this paper is that the absence of restrictive delay laws leads to an increase in handgun-related suicide rates. Furthermore, in times of crisis, people will turn to more lethal means if they are more readily available. However, an important limitation when considering the policy implications of these findings is that no increase in overall suicide rates was observed in either of the research questions. Given this information, the net effect of having restrictive delay laws is unclear. Since no conclusion was reached regarding the net effect of these laws, additional research in this field is required before definitive policy recommendations can be provided. Finally, the large increase in handgun-related suicide rates observed in Research Question 1 offers additional avenues for research that could be further examined by researchers that will not be constrained by the same limitations faced in this analysis.

## 6.1 Limitations

There are several key limitations in this study. Firstly, only year-state level data were available for handgun-related suicide rates, since monthly or county level data were often suppressed by the CDC. Given this, a large sample of pre- and post-treatment years and states was used, which could have possibly affected the overall results. Secondly, there are only a small number of states that have either waiting periods or permitting requirements. Given this limitation, the independent variable of interest had to be compiled combining these two policies. Even though the mechanisms by which these policies could affect handgun-related suicides are similar, it is hard to disentangle the difference between the two with respect to the findings of this paper. Finally, the *NoDelay* and *Delay* states are different in observable and unobservable characteristics. This in turn means that the overall impact of having restrictive delay laws could differ between states, making general conclusions about the effectiveness of delay laws challenging.

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## A Additional analyses

### A.1 Research question 1

#### Group-time average treatment effects

Figures 10a and 10b present the group-time average<sup>24</sup> treatment effects estimated by way of Callaway and Sant’Anna (2021). Since these plots were generated through the corresponding “did” package, it is important to note that *Group 2004* refers to Virginia, *Group 2007* refers to Missouri, *Group 2012* refers to Michigan, and *Group 2015* refers to Wisconsin. Figure 10a is based on the unconditional parallel trends assumption. On the other hand, Figure 10b is based on the conditional parallel trends assumption. The pre-treatment covariates used are as follows: % rural, % only HS degree, % white population and median income. As can be seen from the figures, the prior parallel trends assumption is more likely to hold after conditioning on the above-mentioned pre-treatment covariates. Still, there is moderate evidence suggesting the presence of pre-treatment trends, especially in Wisconsin. In terms of group-time treatment effects, it can be seen that most post-treatment estimates are positive, which provides evidence that repealing delay laws led to an increase in handgun-related suicide rates in each of the *NoDelay* states. Since these estimates are based on state-specific treatment effects, it helps prove that aggregated event-time estimates were not driven by one specific *NoDelay* state.

#### CS estimator without state population weights

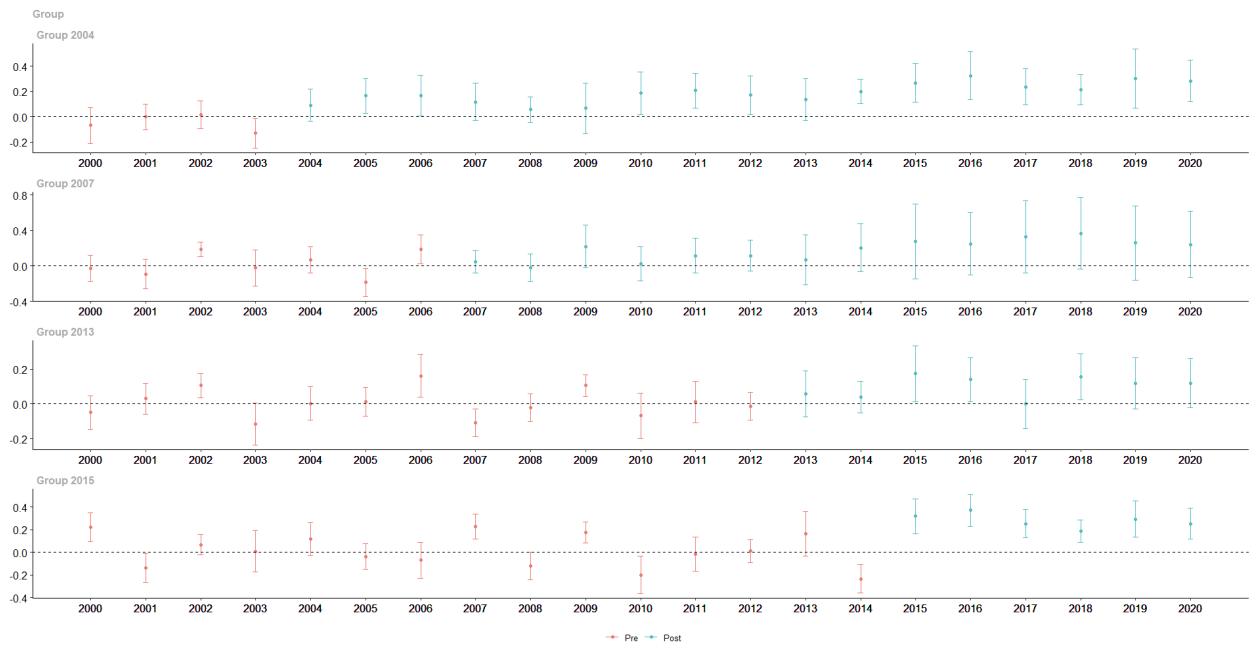
Table 11 and Figures 11a and 11b are estimated by the CS estimator without state population weights. Even without state population weighting, neither figure shows any observable pre-treatment trend. All of the pre-treatment event-time estimates are statistically insignificant. Both Panel (a) and Panel (b) of Table 11 also show large and statistically significant increases in handgun-related suicide rates after delay laws were repealed in *NoDelay* states. Specifically, in Panel (b), the aggregate group-specific treatment effect suggests a 20.6 percent increase in handgun-related suicides. On the other hand, the aggregate event-time treatment effect suggests a 22.5 percent increase in handgun-related suicide rates. Importantly, the estimate from the first period after delay laws were repealed suggests a 17.2 percent increase in handgun-related suicides in the *NoDelay* states. These results reinforce the robustness of the baseline findings.

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<sup>24</sup>The term “average” will be dropped since there is only one group per period. Therefore, these are group-time treatment effects which show how the treatment effect evolved over calendar time.



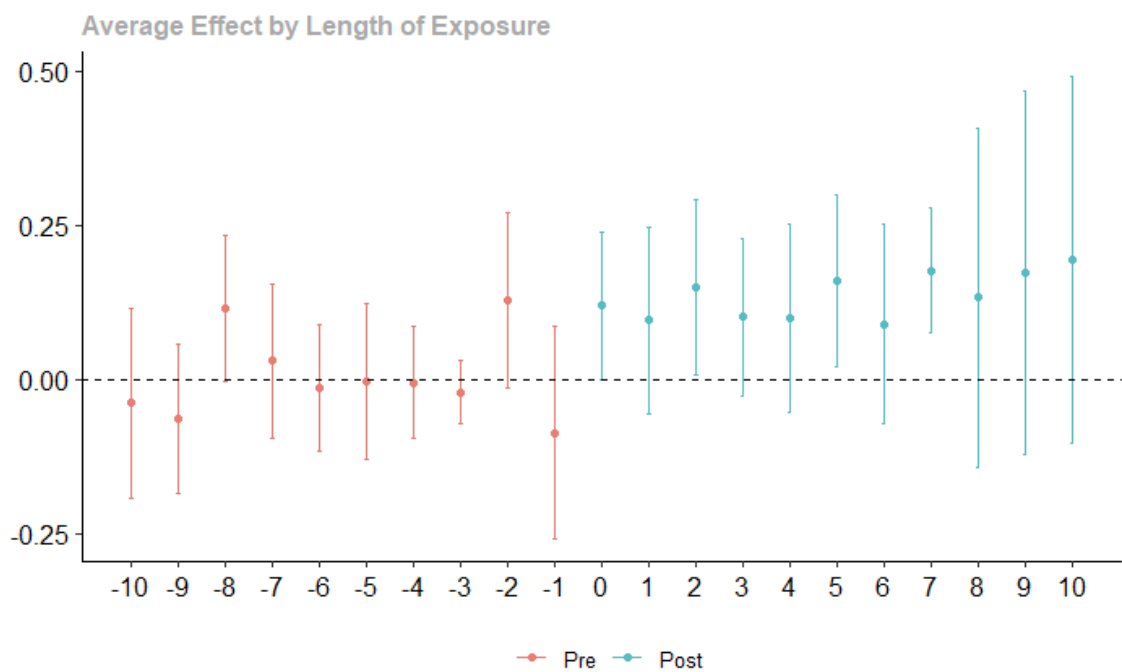
(a) Unconditional parallel trends



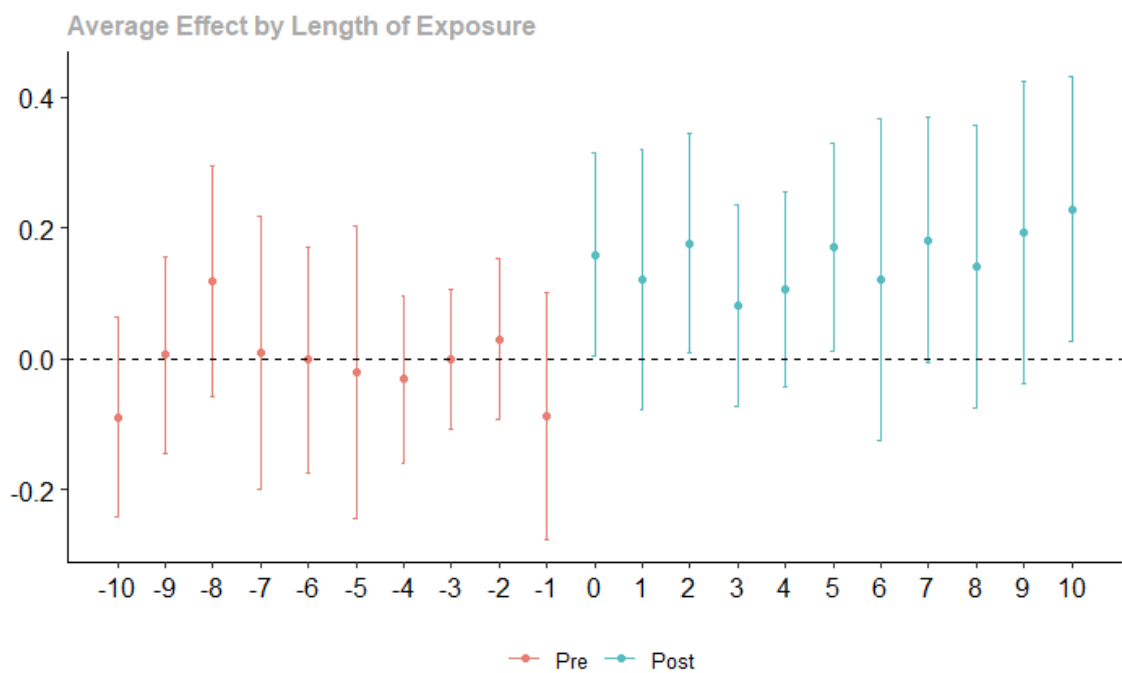
(b) Conditional parallel trends

Figure 10: Group-time average treatment effects (CS, RQ1)





(a) Unconditional parallel trends



(b) Conditional parallel trends

Figure 11: Event study plots - without state population weights (CS, RQ1)

Outcome variable: Log of handgun-related suicide rate per 100,000 people					
(a) Unconditional parallel trends					
	Partially aggregated				Single parameters
Group-specific effects	<u>Virginia</u>	<u>Missouri</u>	<u>Michigan</u>	<u>Wisconsin</u>	
	0.0417	0.1915*	0.1446*	0.2767*	0.1637*
	(0.0732)	(0.0482)	(0.0490)	(0.0469)	(0.0336)
Event study	<u>e=0</u>	<u>e=1</u>	<u>e=2</u>	<u>e=3</u>	
	0.1190	0.0962	0.1501*	0.1007	0.1354*
	(0.0541)	(0.0787)	(0.0612)	(0.0604)	(0.0611)
(b) Conditional parallel trends					
	Partially aggregated				Single parameters
Group-specific effects	<u>Virginia</u>	<u>Missouri</u>	<u>Michigan</u>	<u>Wisconsin</u>	
	0.2492*	0.1293	0.0709	0.2390*	0.1721*
	(0.1020)	(0.1023)	(0.0419)	(0.0630)	(0.0585)
Event study	<u>e=0</u>	<u>e=1</u>	<u>e=2</u>	<u>e=3</u>	
	0.1590*	0.1216	0.1761*	0.0808	0.1526*
	(0.0650)	(0.0906)	(0.0699)	(0.0644)	(0.0671)

**Notes:** Design of table inspired by Callaway and Sant’Anna (2021). The sample period is from 1999 until 2020, with a total of 374 observations from 17 states. Standard errors computed using the multiplier bootstrap. Provided in parenthesis & clustered at the state level. Results of Panel (a) are based on the unconditional parallel trends assumption. Results of Panel (b) are based on the conditional parallel trends assumption. Conditional on the following variables: % rural, % only HS degree, % white population, and median income. All variables are as of 2000. Levels of stat. significance: \*  $p < 0.05$ . Panel (a) and Panel (b) estimates are generated using the outcome regression estimator. Comparison group: Only “Never-Treated” units.

Table 11: Baseline results - without state population weights (CS, RQ1)

## Bacon Decomposition

This subsection shows how the TWFE weights are applied in this analysis using Bacon decomposition (Goodman-Bacon, 2021). As can be seen from Table 12, most of the weight comes from the “good” treated versus untreated comparison (90.7%), and the average estimate when looking at this comparison is 0.20. This is most likely due to a large “never-treated” control group in this sample. Furthermore, the other “good” comparison has a weight of around 5%, also showing a positive estimate of 0.083. On the other hand, the “problematic” comparison shows a negative estimate of  $-0.025$ , but, it only has a weight of 4.3%. Finally, the weighted sum of the decomposition is 0.1868<sup>25</sup>. Figure 12 decomposes this with more detail allowing us to graphically observe all of the different comparisons with the subsequent weights and estimates that are derived. As can be seen from the

<sup>25</sup>The results differ from the main specification in Table 7 because Bacon decomposition does not allow for state population weighting.

figure, all treated versus untreated comparisons are large and have a large proportion of the overall weight, with the exception of one treated versus untreated comparison (this is most likely Virginia). Given these results, the treatment effect parameter from Equation 1,  $\beta_1$ , can be trusted when using the TWFE estimator in this analysis. However, testing for pre-treatment trends could still be problematic when using the TWFE estimator to estimate the “dynamic” specification as in Equation 3. As a final note, this analysis was performed using the *bacondecomp* package in R.

Comparison Type	Weight	Average Estimate
Earlier vs Later Treated	0.04390	-0.02555
Later vs Earlier Treated	0.04959	0.08371
Treated vs Untreated	0.90651	0.20277
Weighted sum of decomposition	1	0.1868

Table 12: Bacon Decomposition (RQ1)

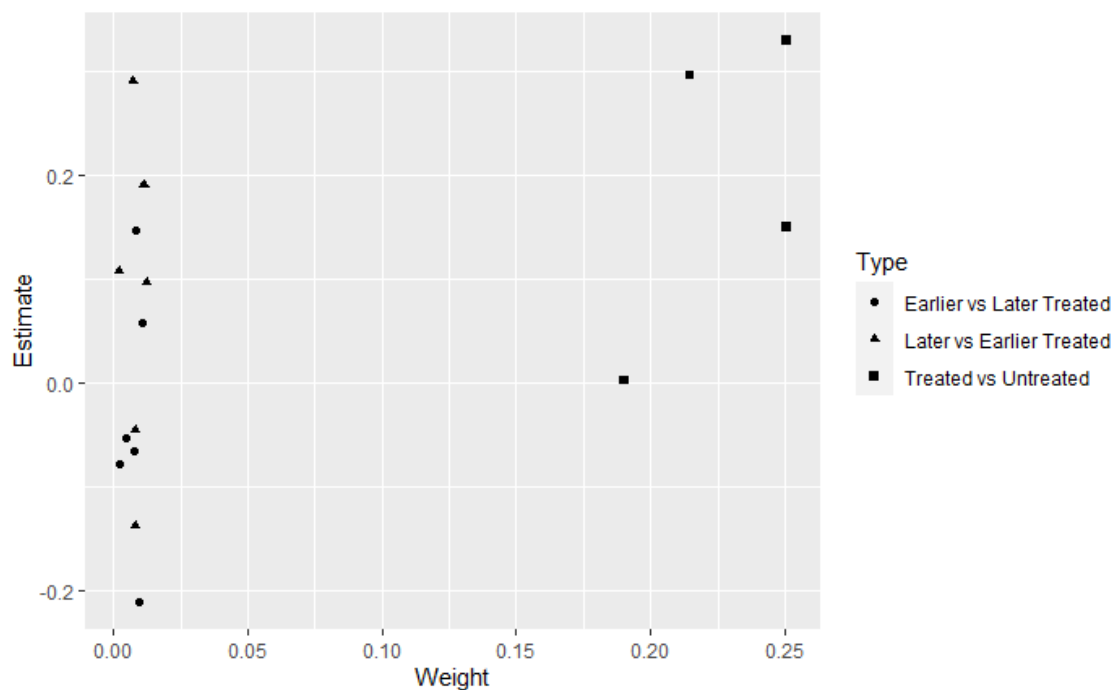


Figure 12: Bacon Decomposition Plot

## A.2 Research question 2

### Main specification with WCSE

As a further robustness check, Table 13 provides the baseline results as in Table 9 with WCSE instead of clustered standard errors. This was done as an additional robustness check of the baseline results. It can be seen that there are only marginal changes to the levels of statistical significance when comparing the p-values between the two tables. For example, the estimate from Column 1 of Table 13 has a p-value of 0.052 compared to the p-value of 0.032 in Column 1 of Table 9 which used clustered standard errors. Therefore, there are only marginal changes to the observed levels of statistical significance between tables, suggesting that the use of clustered standard errors is reasonable.

	Outcome variable: Log of ...-related suicide rate per 100,000 people					
	Handgun				All	Non-handgun
	(1)	(2)	(3)	(4)	(5)	(6)
NoDelay x Post	0.038*	0.033	0.050*	0.043*	-0.032*	-0.047
	[-0.001, 0.075]	[-0.019, 0.076]	[-0.010, 0.107]	[-0.002, 0.079]	[-0.060, 0.006]	[-0.092, 0.015]
	p = 0.052	p = 0.183	p = 0.094	p = 0.057	p = 0.087	p = 0.115
Inference	WCSE	WCSE	WCSE	WCSE	WCSE	WCSE
Num.Obs.	460	460	460	460	460	460
States	46	46	46	46	46	46
Std.Errors	by: State	by: State	by: State	by: State	by: State	by: State
FE: State	✓	✓	✓	✓	✓	✓
FE: Year	✓	✓	✓	✓	✓	✓
Controls	×	✓	✓	✓	×	×

**Notes:** Design of table inspired by Koenig and Schindler (2018). Confidence intervals are presented in square brackets. Additionally, p-values are presented for each estimate. The sample period is from 2000 until 2009. In Column (2) included control variables are % rural and % only HS degree. In Column (3) included control variables are % rural, % only HS degree, % white population, and median income. All control variables in Columns (2) and (3) are as of 2000 and interacted with Year FE. In Column (4), the only included control variable is the time-varying state level unemployment rate which is compiled for the years from 2000 until 2009. Levels of stat. significance: \*  $p < 0.1$ , \*\*  $p < 0.05$ .

Table 13: Baseline results with WCSE (RQ2)

## B Data cleaning

The relevant data sets were merged and cleaned using Python through Jupyter Notebook. This process was done in a reproducible manner so that anyone can see how the final data sets were formed. Regarding the results, all the econometric work was done in R. Again, the work was done in R Markdown so that anyone can access and reproduce the results. For further transparency, all the data sets and scripts mentioned above are stored in the following GitHub folder: <https://github.com/lparisi94/Master-Thesis>. Since there is an extensive list of scripts in this GitHub repository, this section will cover only the main cleaning procedures undertaken and will give further advice on how to navigate through the different folders in the repository mentioned above.

### Handgun-related suicides

There were no exclusions in Research Question 1 due to values of the outcome variable. On the other hand, there was one exclusion in Research Question 2. Alaska was excluded due to highly varying handgun-related suicide rates. As can be seen in Figure 13, the handgun-related suicide rate varied from 14 per 100,000 people in 2002, to 6 per 100,000 people in 2009. It is unclear why this variable had such large disparities between the years. Therefore, it was excluded from the analysis. However, the results do not change with the inclusion of this state, as shown in Table 10.

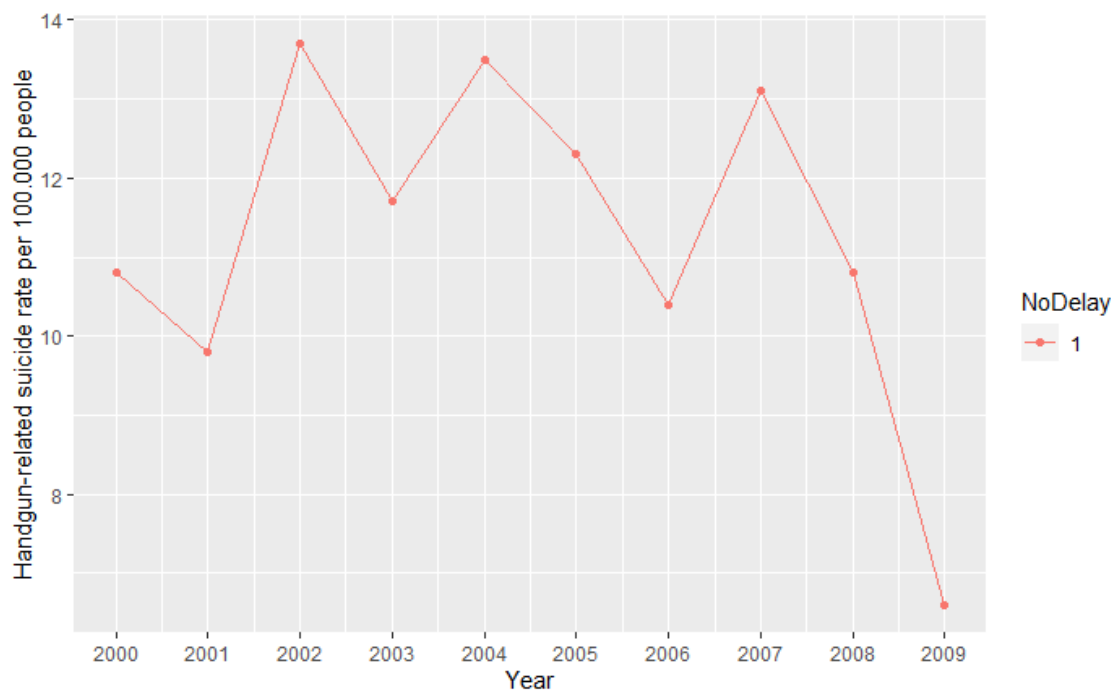


Figure 13: Evolution of handgun-related suicide rate in Alaska (2000-2009)

### Handgun-purchase delay laws

The only major change with respect to delay laws was that Michigan was manually re-coded. As noted, Michigan repealed its permitting requirement in December 2012 and the first period of treatment was set to 2013 instead of 2012. On the other hand, Pennsylvania was excluded from the analysis because of the identified discrepancies between Siegel et al. (2017) and Cherney et al. (2022).

### GitHub replication files

As mentioned above, the following repository has all of the scripts necessary to replicate the results: <https://github.com/lparisi94/Master-Thesis>. Table 14 provides a brief summary of the folders within this repository.

Folder Name	Contents
Data	Original .csv data files & .txt file with sources from which data was downloaded.
Cleaning	Jupyter notebooks for both research question.
Final	Final .csv data sets.
Analysis	R Markdown files which show the complete analysis.
Figures	Main figures.

Table 14: GitHub replication folders

## C Comprehensive list of delay laws

This section provides a comprehensive list of delay laws in all states for both research questions. Table 15 lists all handgun purchase delay laws present in every state of the sample for Research Question 1. The table was compiled for the year 2020, which is the last year in the sample.

### *NoDelay states*

State	Waiting Period (WP)	Permit Requirement (PR)	Additional Notes
Michigan	×	×	Repealed PR in 2012.
Missouri	×	×	Repealed PR in 2007.
Virginia	×	×	Repealed PR in 2004.
Wisconsin	×	×	Repealed 48 hour WP in 2015.

### *Delay states*

State	Waiting Period	Permit Requirement	Additional Notes
California	✓	✓	10 day WP & PR
Connecticut	×	✓	Only PR
Hawaii	✓	✓	14 day WP & PR
Illinois	✓	✓	72 hour WP & PR
Iowa	×	✓	Only PR
Maryland	✓	✓	7 day WP & PR
Massachusetts	×	✓	Only PR
Minnesota	✓	×	7 day WP
Nebraska	×	✓	Only PR
New Jersey	✓	✓	7 day WP & PR
New York	×	✓	Only PR
North Carolina	×	✓	Only PR
Rhode Island	✓	✓	7 day WP & PR

**Notes:** Cherney et al. (2022) and Siegel et al. (2017) used as main sources when compiling all relevant handgun-purchase delay laws. Michigan was re-coded in the data cleaning process and its first period of treatment was set to 2013 since it repealed its PR in December 2012.

Table 15: List of handgun delay laws as of 2020 (RQ1)

Table 16 lists all handgun purchase delay laws present in every state of the sample for Research Question 2. The table was compiled for 2009, which is the last year in the sample. Importantly, in this sample, no states changed legislation throughout the whole duration of the panel (2000-2009).

*NoDelay states*

State	Waiting Period (WP)	Permit Requirement (PR)	Additional Notes
Alabama	×	×	No delay law for handguns
Arizona	×	×	No delay law for handguns
Arkansas	×	×	No delay law for handguns
Colorado	×	×	No delay law for handguns
Delaware	×	×	No delay law for handguns
Florida	×	×	No delay law for handguns
Georgia	×	×	No delay law for handguns
Idaho	×	×	No delay law for handguns
Indiana	×	×	No delay law for handguns
Kansas	×	×	No delay law for handguns
Kentucky	×	×	No delay law for handguns
Louisiana	×	×	No delay law for handguns
Maine	×	×	No delay law for handguns
Mississippi	×	×	No delay law for handguns
Montana	×	×	No delay law for handguns
Nevada	×	×	No delay law for handguns
New Hampshire	×	×	No delay law for handguns
New Mexico	×	×	No delay law for handguns
North Dakota	×	×	No delay law for handguns
Ohio	×	×	No delay law for handguns
Oklahoma	×	×	No delay law for handguns
Oregon	×	×	No delay law for handguns
South Carolina	×	×	No delay law for handguns
South Dakota	×	×	No delay law for handguns
Tennessee	×	×	No delay law for handguns
Texas	×	×	No delay law for handguns
Utah	×	×	No delay law for handguns
Vermont	×	×	No delay law for handguns
Washington	×	×	No delay law for handguns
West Virginia	×	×	No delay law for handguns
Wyoming	×	×	No delay law for handguns

*Delay states*

State	Waiting Period	Permit Requirement	Additional Notes
California	✓	✓	10 day WP & PR
Connecticut	×	✓	Only PR
Hawaii	✓	✓	14 day WP & PR
Illinois	✓	✓	72 hour WP & PR
Iowa	×	✓	Only PR
Maryland	✓	✓	7 day WP & PR
Massachusetts	×	✓	Only PR
Michigan	×	✓	Only PR
Minnesota	✓	×	7 day WP
Nebraska	×	✓	Only PR
New Jersey	✓	✓	7 day WP & PR
New York	×	✓	Only PR
North Carolina	×	✓	Only PR
Rhode Island	✓	✓	7 day WP & PR
Wisconsin	✓	×	48 hour WP

**Notes:** Cherney et al. (2022) and Siegel et al. (2017) used as main sources when compiling all relevant handgun-purchase delay laws.

Table 16: List of handgun delay laws as of 2009 (RQ2)