Looking Down the Barrel of a Loaded Gun:
The Effect of Mandatory Handgun Purchase Delays on Homicide and Suicide

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Abstract

The effects of policies aimed to restrict firearm ownership and usage is a heavily debated topic in modern social science research. While much of the debate has focused on right-to-carry laws, less research has focused on other policies that affect firearm ownership and use, in particular statutory delays between the purchase and delivery of a firearm. In addition to the 1994 Brady Handgun Violence Prevention Act, which placed a mandatory five-day wait period between the purchase and delivery of a handgun, many states enacted similar policies before and after Brady's effective years. We exploit within-state variation across time in both the existence of a purchase delay and length of the delay to examine the effect of purchase delays on firearm-related homicides and suicides. We find that the existence of a purchase delay reduces firearm related suicides by between 2 to 5 percent with no statistical evidence of a substitution towards non-firearm suicides. Purchase delays are not associated with statistically significant changes in homicide rates.

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Introduction

The frequency of mass shootings in recent years has energized the long-standing and hotly debated topic of gun control in the United States. While these tragedies tend to make the costs of firearm availability salient to policy makers and the general public, the day-to-day reality of gun violence, including homicides and suicides, is unquestionably a much larger source of social cost.[1] On average, 36 firearm-related homicides occur every day, and an additional 50 individuals per day die from firearm-related suicides.[2] To put this in perspective, self-inflicted gun shots kill as many Americans every day as the worst mass shooting in the country's history. Moreover, firearm-related homicides result in more deaths each day than America's second worst mass shooting.[3] Thus, evaluating ways to reduce these more common and costly sources of firearm-related deaths remains an important task for researchers.

Although a large body of research examines the impact of various gun control polices on gun-related violence,[4] mandated delays between the purchase and delivery of a handgun have received much less attention from researchers despite the potential to deter gun-related violence at minimal cost to gun owners. Purchase delays, often characterized as waiting periods, are notable in that the policy is one of only a small number of gun control policies ever implemented at the Federal level. The Brady Handgun Violence Prevention Act (Brady) implemented a temporary five-day waiting period on handgun purchases for federally licensed firearm dealers in 1994 and required that FFL dealers contact local authorities to perform a background check on all handgun purchasers before completing the sale. The waiting period provision of Brady

[1] Ludwig and Cook (2000) estimate the social cost of gun violence to be more than \$1.7 million per injury (2013 dollars).
[2] Estimates of the average daily number of homicides and suicides are computed using data from the CDC's Fatal Injury Reports.
[3] The Orlando, FL shooting in 2016 claimed 50 lives, and the Virginia Tech shooting in 2007 claimed 32 lives.
[4] *See* Manski and Pepper (2015) for an overview of the right to carry literature and challenges inherent to the gun control literature.

expired in 1998 when the FBI launched the National Instant Criminal Background Check System (NICS).[5] However, in addition to Brady, many states have passed legislation imposing delays on the delivery of firearms including explicit waiting periods or implicit waiting periods through licensing or permit requirements.

We exploit the variation in purchase delays due to both Brady and to changes in state laws across time to examine the effect of these policies on the rates of homicide and suicide. We compile a database of state-level gun purchase restriction legislation between 1990 and 2013 which includes the existence and type of gun purchase restriction legislation, and we examine multiple cause of death data from the Centres for Disease Control and Prevention and homicide data from the Uniform Crime Reports. Using a difference-in-differences approach, we find that any mandatory purchase delay reduces firearm-related suicides by between 2 to 5 percent, and we find no statistically significant substitution toward non-firearm suicides. Additionally, mandatory purchase delays are not statistically significantly related to homicides. Our results are robust to various measures of gun restrictions, and we find little evidence of policy endogeneity.

Our paper is the first paper to explicitly examine the effect of mandatory handgun purchase delays on violent deaths using a 50 state panel. While Ludwig and Cook (2000) examine the effect of Brady, they are unable to isolate the effect of wait periods or other purchase delays since Brady included both wait periods and background checks and the significant amount of variation in purchase delay policies within the treatment and control groups makes it impossible for Ludwig and Cook to isolate the effects of purchase delays. Additionally, Ludwig and Cook's data end in 1999, but we utilize variation in purchase delay laws from both federal and state statutes from

[5] The waiting period aspect of Brady was originally written as a temporary measure which expired in 1998. President Clinton was unable to get support from the Republican majority in congress to extent the wait period provision.

1990 through 2013. This allows us to exploit more variation by examining the laws leading into and coming out of Brady. While Ludwig and Cook find only sporadic effects of Brady on suicides and homicides, we consistently find that purchase delays deter firearm related suicides without an increase in non-firearm related suicides.

In terms of policy implications, purchase delays represent an effective but noninvasive policy that balances the rights of gun owners and the externalities associated with owning a gun.

Background

The Relationship Between Guns and Violence

A required delay in purchasing a firearm might reduce fatalities by either directly interrupting a homicidal or suicidal plan (cooling off effect) or indirectly by discouraging handgun purchases and consequently reducing the stock of handguns (fewer guns effect). The cooling off effect would only affect purchases made from a regulated firearms dealer, whereas the fewer guns effect has the potential of affecting all channels of firearm acquisition.

With respect to homicides, there is reason to doubt that a cooling off effect would decrease violent deaths because the majority of criminals report obtaining firearms through a number of non-traditional channels including theft, family members or friends, or private sales on the secondary market (Cook, Ludwig, & Samaha, 2009; Ross, Yakovlev, & Carson, 2012). Concern about unregulated private sales has led many policy makers to work at closing the "gun show loophole." However, Duggan, Hjalmarsson, and Jacob (2010) show that gun shows have no detectable effect on homicides or suicides, and tighter regulation of gun shows does not appear to reduce firearm-related death. Taken together, the evidence seems to suggest that a large portion of those who commit homicides obtain firearms through theft or private connections, and thus homicides are unlikely to be significantly affected by purchase delays.

It is still possible that firearm purchase delays could still affect homicides indirectly by affecting the prevalence of guns in a jurisdiction. The availability of firearms may affect the homicide rate through a combination of changing the violent crime rate and changing the fatality rate of criminal activity. There has been considerable debate about the effect of greater gun availability on crime (e.g. (Ayres and Iii (2003); H. Naci Mocan and Erdal Tekin (2006); Lang (2016); Lott (1998); Mark Duggan (2001); Moody & Marvell, 2005; Siegel, Ross, and King (2013))). Cook et al. (2009) note that while there is little compelling evidence that gun prevalence increases violent crime, there is strong evidence to suggest that a greater availability of firearms increases the probability that a gun will be used in a crime and the likelihood that a crime will result in a fatality. Nonetheless, since purchase delay policies are not likely a strong deterrent to gun sales *per se*, there is reason to be sceptical that delay policies have a significant effect on homicides.

Unlike homicides, the mechanism by which a firearm purchase delay may discourage suicides is straightforward. In addition to the body of research that shows an association between gun prevalence and suicides (Anglemyer, Horvath, & Rutherford, 2014; Briggs & Tabarrok, 2014; Lang, 2013; Matthew Miller, Barber, White, & Azrael, 2013; Phillips, 2013), there are three stylized facts that have emerged from the firearm related suicide literature that establish the mechanism through which purchase delays may affect suicides.

First, research suggests that many firearms used in fatal suicides were recent purchases (Kellermann et al., 1992; Lewiecki & Miller, 2012; Vriniotis, Barber, Frank, Demicco, & the New Hampshire Firearm Safety, 2015). Second, the decision to attempt suicide is, for at least some victims, often made within a few hours of suicide ideation (M. Miller, Azrael, & Barber, 2012; Peterson, Peterson, O'Shanick, & Swann, 1985). Third, for many potential victims of suicide, suicidal thoughts are impulses that can be diverted and discouraged (Clarke & Mayhew, 1988; M. Miller et al., 2012). This third point is evidenced by observing that suicides by jumping can be prevented without substitution with the installation of physical barriers preventing access and in some cases signs discouraging suicide (Cox et al., 2013). Additionally, suicide prevention hotlines or contact with mental health providers have been associated with a decrease in suicides (Cebrià et al., 2013; Hughes & Asarnow, 2013). Research has also shown that the majority of those who survive near-lethal suicide attempts go on to die from causes other than suicide (Owens, Horrocks, & House, 2002)

The first two facts together suggest that it is possible for some victims of suicide to experience ideation, a firearm purchase, and an attempt all within a short period of time. Coupled with the third fact that many suicides are easily discouraged, a purchase delay could create just enough of a break in the ideation-purchase-attempt flow to effectively discourage some would-be firearm suicides without substitution to other types of suicide. It is also important to note that while it may factor into the overall effect of purchase delays on suicides, the mechanism is not necessarily dependent on a decrease in the prevalence of firearms (the fewer guns effect). It is entirely possible that an individual may have purchased (and now own) a gun with the intent to commit suicide, but being subject to a purchase delay provided ample time for the suicide ideation to pass, and as previous research suggests, even though that individual now owns a gun, and has had suicidal thoughts, the individual will probably eventually die for reasons other than suicide.

Though our story does not necessarily hinge on a "fewer guns" effect, there is some research from studies outside the United States on the efficacy of reducing the stock of guns in a population (Leigh & Neill, 2010; Lenis, Ronconi, & Schargrodsky, 2010). In the United States however, this is complicated greatly by the lack of an accurate measure of gun availability. There is no public registry of new gun purchases. Additionally, there is a large stock of guns in the United States, and with minimal maintenance, guns can function for many years. This problem is further complicated in studying suicides since one of the most trusted measures of gun prevalence in the literature is the percent of suicides committed with a firearm which would place the outcome variable in the numerator or denominator (depending on what suicide outcome is being studied) of the regressor of interest (Lang, 2013). Other studies attempt to proxy for changes in gun availability by examining the number of federal background checks (Lang, 2013), constructing an index of gun-related items (Briggs & Tabarrok, 2014), examining subscriptions to gun-related magazines (Mark Duggan, 2001), examining the local effect of gun shows (Duggan et al., 2010), and exploiting the surge in purchases around the 2008 presidential election (Depetris-Chauvin).

Similar to Lang (2013) we are unable to use the firearm suicide ratio since suicides is an outcome of interest.[6] We are, unfortunately, further restricted than Lang (2013) in that background check records did not begin until 1998 and much of our identifying variation occurs between 1990 and 1998. In place of the firearm suicide ratio or background check data, we proxy for the supply of firearms in the US by controlling for the accidental firearm death rate which is correlated to background check data in a similar matter to the firearm suicide ratio. In

[6] We are, however, able to control for the firearm suicide ratio in the homicide regressions and find virtually no difference in our estimates when controlling for the rates of accidental firearm deaths and firearm related suicide deaths.

overlapping years in our data set (1998-2013), the correlation between FBI handgun background checks and accidental firearm deaths is 0.45, compared to a correlation coefficient of 0.11 between FBI handgun background checks and the firearm suicide ratio. [7]

Previous research examining the effect of statutory purchase delays on violent deaths has largely found a negative relationship between handgun purchase delays and homicides and suicides. However, these studies have generally examined only a single site, ((Rudolph, Stuart, Vernick, and Webster (2015); D. Webster, Crifasi, and Vernick (2014))), employed a short panel (Kleck and Patterson (1993), or did not include information on wait period legislation (Anestis et al., 2015; Cook et al., 2009).

We explicitly measure the effect of statutory purchase delays using over two decades of data that include not only the start of the federally mandated waiting period from Brady, but also what happened before and after the five-day waiting period that expired in 1998. Moreover, we are able to examine all other statutory sources of delay in purchasing a gun at the state level. As noted, some states have their own waiting periods, and others require permits for gun purchase which creates an unofficial delay in the time it takes to obtain the permit. This additional policy variation gives us additional statistical power to identify effects of a wider-array of gun regulations on homicides and suicides. Lastly, our state-level panel dataset allows us to account for the potential endogeneity of gun laws.

Statutory Sources of Delay

[7] The seemingly low correlation between background checks and the firearm suicide ratio is likely due to the huge upswing in background checks that happened after 2008 due to the Obama presidency—which is not captured in Lang (2013) as his dataset ends in 2008. Additionally, restricting our data set to years between 1998 and 2013—when both accidental firearm deaths and background check data are both available—we find similar results when interchanging accidental firearm deaths and background checks.

Statutory delay of a handgun purchase falls into three broad categories. First, many states impose no delay. As of 2013, 38 states impose no delay, and an individual could walk into a gun shop and walk out with a handgun. Second, many states require an express waiting period prior to obtaining the handgun.[8] The waiting periods, described in Table 1, range from 48 hours in Wisconsin[9] to 14 days in Hawaii.[10] Lastly, some states require license, permit, or certificate requirements. The delay in these "permit"[11] states comes from the time required to process the necessary paperwork that accompanies the permit. While some states with permit-related delays have enacted caps on the time the state can take to issue the permit, it is difficult to know with certainty the realized delay create by a permit.[12] Given this, we include as a control in each regression a dummy variable for permit states but make no further assumption about the length of delay from a permit state when calculating a gradient effect of purchase delays. The variation we exploit in this paper can be seen visually in Figures 1 and 2 and explicitly in Table 1. As is explained in the Methods section, we exploit moves from no delay to a delay or from a delay to no delay within a state over time. Table 1 shows the years in which states change either from a delay to no delay or from no delay to a delay, and Figure 1 shows the number of states with different categories of purchase delays by year. We map the states with different categories of purchase delays in Figure 2 to display the spatial variation of laws, and there is no apparent spatial correlation. We more formally test for political endogeneity in our Results and Appendix sections.

[8] In at least some states, gun dealers will complete the sale of the firearm then leave the responsibility on the purchaser to return to pick up the gun after the requisite wait.
[9] Wis. Stat. Ann. § 175.35 (West 2014)
[10] Haw. Rev. Stat. § 134-2 (West 2014)
[11] For sake of ease, we refer to all sort of document based delay (permits, licenses, certificates, etc.) as permits, recognizing the differences in each.
[12] These caps ranged from 3 days in Nebraska (Neb. Rev. Stat. § 69-2405 (West 2015)) to 6 months in New York (N.Y. McKinney's Penal Law § 400.00) (2013)).

The landscape of handgun delays changed significantly with the passage of Brady in 1994. The primary focus of Brady was a national background check system, but the law also imposed on many, but not all, states a temporary five-day wait period to purchase a handgun. The law was named for James Brady, who was shot by John Hinckley, Jr. during Hinkley's attempted assassination of President Reagan in 1981. In 1998, the interim five-day wait period expired by the terms of the Brady Act. States responded to the end of the Brady wait period in different ways. Some states eliminated their wait, others reverted back to the original wait period, and some increased their wait period.[13] Having a mandated wait and a permit requirement are not mutually exclusive policies in our dataset.[14] While rifles and shotguns are more quickly available in most jurisdictions, only a few states, California, Connecticut, D.C., Hawaii, Illinois, Maryland, Massachusetts, Minnesota, New Jersey, and Rhode Island, impose a delay for long guns. Unfortunately, these long gun policies do not provide enough identifying variation for estimation.[15]

The role Brady has had on violent outcomes has been well researched.[16] In a leading study, Ludwig and Cook (2000) examine the role Brady played on homicide, firearm homicide, suicide, and firearm suicide rates with a panel from 1985 to 1997. They find that the firearm suicide rate among people aged 55 years or older declined after Brady by a statistically significant amount, but the Brady law implementation had no other statistically significant effects on the outcome measures. What we attempt to answer in this project differs from Ludwig

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[13] *See* Table 1 for a full list of each state's statutory history. For example, after Brady Alabama and South Dakota reduced their wait to 2 days then eventually removed it all together while D.C. increased its wait in 2010.

[14] *See* Conn. Gen. Stat. § 29-28, -37a; D.C. Code §§ 22-4508 & 7-2502; Md. Code, Public Safety, §§ 5-123, -124, -117, -117.1; Mass. Gen. L. 140 §§ 131E, 129B, 131, 131A; Mich. Comp. Laws Ann. §§ 28.422, 28.425b; Vernon's Ann. Mo. Stat. § 571.080; Neb. Rev. Stat. §§ 69-2403, -2404, -2405; N.J. Stat. §§ 2C:58-2, -3; N.Y. Penal Law § 400.00; N.C. Gen. Stat. §§ 14-402, -404; and R.I. Gen. L. §§ 11-47-35, -35.2

[15] Of the few states that do implement purchase delays for long guns, any changes in these policies perfectly coincide with changes in purchase delays for handguns.

[16] *See* Lenis et al. (2010) for a summary of the Brady literature.

and Cook (2000) in that we are interested in the effect of purchase delays specifically, and we are able to exploit more statutory variation by following the panel in and out of Brady. Furthermore, as stated in the Introduction, within group variation in presence and length of purchase delays in both their treatment and control states makes it impossible for Ludwig and Cook (2000) to isolate the effects of purchase delays on violent deaths. Of those states classified as treatment states by Ludwig and Cook, 8 states had a mandatory wait period and another 2 had permit requirements prior to Brady, with the remaining 22 states had no purchase delay prior to Brady. Likewise, of the states classified as control states 13 had a mandatory wait period, 3 had permit requirements, and 3 had no purchase delay.[18] Thus, Ludwig and Cook's analysis primarily estimates the effects of criminal background check requirements in which purchase delays are a confounding factor.[19] On the contrary, our analysis specifically examines the effect of purchase delays on violent deaths while controlling for the presence of background checks.

Data

Our dependent variables of interest are logged suicide and homicide rates in each state and year. Using multiple cause of death data from the National Center for Health Statistics,[23] we

[18] Nevada, which Ludwig and Cook classify as a control state, actually had the 5 day Brady wait period imposed because they lost their exempt status.
[19] Ludwig and Cook (2000) do not control for the presence of purchase delays prior to or during the Brady Act.
[23] While we consider the NCHS data the most complete source of homicide data, we test the same models using the Uniform Crime Report (UCR) data and find virtually the same results. Those results are available in the Appendix.

collect the number of firearm and non-firearm related homicides and suicides between 1990 and 2013.[24]

Table 2 shows the summary statistics of the relevant variables. Our variables of interest measure delays between the purchase and delivery of a handgun. We code the laws as "treated" from the first full year of enactment.[25] The second and third columns of Table 2 show the summary statistics of state-years separated according to whether there was a mandatory purchase delay in effect or not. On average the suicide rate is approximately 24 percent lower (14.6 versus 11.12 per 100,000) in state-years with a mandatory purchase delay. Likewise, the firearm-related suicide rate is about 35 percent lower (8.91 versus 5.79 per 100,000) on average when a purchase delay is in effect. The non-firearm-related suicide rate is also lower in state-years with purchase delays, but the magnitude of the difference is much smaller (about a 0.36 per 100,000). In contrast, homicide rates (total, firearm, and non-firearm) are all higher in state-years with a purchase delay. While there are certainly more factors causing these differences than the purchase delay policies alone, these raw numbers support the notion that purchase delays may prevent firearm suicide and motivate further investigation.

To account for the role background check laws may have on homicides and suicides, we include a dummy variable that takes the value of one for all post-1994 years when presumably all states had a background check system, and also takes the value of one for all pre-1994 state/years that exempted out of Brady by already having a background check system in place.[26]

[24] The most reliable statutory histories come from this time frame.
[25] The results presented here are generally insensitive to alternative coding of the laws.
[26] This Brady background check variable assumes states that exempted out of Brady had a background check for the entire period between 1990 and 1994. As an additional check, we ran our main results restricting the dataset to the "universal background check" years of 1994-2013 and found the point estimates to be insensitive to this window of time.

As discussed previously, a good measure of the stock, flow, or usage of firearms in the United States is difficult to obtain. We include the rate of accidental firearm deaths per state and year from the NCHS multiple cause of death files to proxy for the stock, flow and usage of firearms. Cook et al. (2009) note that measurement of accidental gun deaths is affected by local coroners' standards for what constitutes a homicide or suicide as opposed to an accidental death. To the extent that these different judgment calls are not systematically related to changes in statutory purchase delays, this problem is resolved by state and year fixed effects.

Additionally, we control for other factors that may be driving homicides and suicides. These include demographic variables from the US Census such as the percent of the population that is adult male, is African American, and lives in urban areas, as well as the infant mortality rate, the unemployment rate and real per capita income.[27] We include per capita ethanol consumption from the National Institute on Alcohol Abuse and Alcoholism (NIAAA) to capture the role alcohol plays in homicides and suicides, and the proportion of each state's house and senate that is democrat to capture the role politics may play in the passage of these laws.

There is an extensive literature that maps the relationship of mental health to both crime and suicide (Edwards, 2013, 2014). To capture this in our regressions we include the real per capita state mental health expenditures from Ross et al (2012). There is also reason to believe that both homicides and suicides in the time period in which we examine were affected by the rollout of new psycho-pharmaceuticals and antidepressants. Marcotte and Markowitz (2011) exploit the state to state variation in drug roll out to measure the effect of prescription drugs on crime. We attempt to capture these within state trends of prescription drug usage by include the accidental poisoning death rate collected from the WISQARS database. While the accidental

[27] The last two come from the Bureaus of Labour Statistics and Economic Analysis, respectively, and all are interpolated when needed.

poisoning death rate may be correlated with the roll out of new prescription drugs, it is potentially also capturing the prevalence of prescription opiate drug abuse. Although this is an important factor in homicides and suicides, it is not directly to the point of Marcotte and Markowitz (2011). To more directly capture this and other within state trends that are hard to observe, we include in each regression state specific time trends. Additionally, mapping each state's raw trend in homicides and suicides suggest the need for state specific trends. Those graphs are available in the Appendix. Based on the raw trends, we include linear time trends for each homicide regression and quadratic time trends for the suicide regressions, though each result is completely robust to using linear or quadratic time trends.

There are a host of other policy variables, mentioned previously, that may potentially influence gun related outcomes and have been the subject of much research such as right to carry laws, efforts to regulate private sales through closing the gun show loophole, and background check requirements for private sales. Given the lack of consensus in the research about right to carry laws and gun show loopholes, we omit these variables from our equations. While we do not have data on the states that require a background check for private sales, we do address background checks generally.

Methods

We utilize a quasi-natural experiment design, connecting policy changes within states over time, to changes in homicides and suicides. We first estimate a model to determine whether the existence of any policy that creates a delay affects homicide and suicide rates,

$$\ln{(s_{it})} = \alpha + \beta W_{it} + \theta \mathrm{X}_{it} + \gamma_t + \tau_i + \tau_i * t + \varepsilon_{it} \qquad (1)$$

where $\ln(s_{it})$ is the natural log of the homicide or suicide rate, X is a vector of controls outlined in the Data section, γ is a set of year fixed effects, τ is a set of state fixed effects and $\tau_i * t$ are state specific time trends. We choose a log-linear model because we believe the impact of the policy will be proportional to the base rate of homicide or suicide in each state-year. That is, it is likely that the policy would have a larger effect in an area or time when the rate of suicides or homicides is high—not a constant marginal effect in all areas and time periods, like using rate dependent variable would assume.[28] Each regression is weighted by state population,[29] and standard errors are clustered at the state level.

The specific variation we exploit in purchase delays is measured by W_{it}—an indicator variable that takes on the value of 1 when state *i* in time *t* has a non-zero wait time. The years each state switches on and off purchase delays can be seen in Table 1. Additionally, W_{it} takes on the value 1 when a state has a delay created by a permit requirement.

It is possible that length of purchase delay, not just the presence of a delay, also influences violent deaths. Wait periods during this time frame only take on seven values (2, 3, 5, 7, 10, 14, and 15 days), and many values are unique to one or two states. We therefore measure the gradient effect using a more discrete approach, rather than treat the length of delay as a continuous variable. We expand our specification in Equation (1) to measure whether the length of the statutory delay affects the policy effectiveness with the following specification:

$$\ln(s_{it}) = \alpha + \beta W_{it} + \delta L_{it} + \theta \mathrm{X}_{it} + \gamma_t + \tau_i + \tau_i * t + \varepsilon_{it} \qquad (2)$$

where L_{it} is a dummy variable that takes the value of 1 if the state has a wait period a week or longer and zero otherwise, and all other variables are the same as in Equation (1) including a

[28] Additionally, the Box-Cox test of functional form for firearm suicides, our main outcome, yields a test statistic (θ=0.35) that suggests the log model as the most appropriate.
[29] The magnitudes of the results are generally insensitive to the inclusion/exclusion of population weights, and are completely insensitive in our main findings reported in Table 4.

control for permit related delays. The inclusion of L_{it} captures any gradient effect in the variation of length of delay. Note that these two variables are not exclusive to W_{it}. The interpretation of δ can be thought of as the additional effect of a long wait period.[30] We prefer the specifications above because alternative specifications that seek to more finely measure the gradient effect of the length of the wait suggest no statistical difference between 1 to 3, 5, and 7+ day waits and comes at the cost of weaker identifying variation.

We also use an event-study specification to estimate the dynamic effects of purchase delay policies. This framework allows for an implicit test of whether pre-existing trends are driving our difference-in-difference results. To correctly capture all the available variation in purchase delays, including the repeal of some purchase delays, we estimate the following modification of equation (1):

$$\ln(s_{it}) = \sum_{k=-3}^{5} a_k W_{i(t-T^*)} + \sum_{z=0}^{4} c_z Post_{i(t+E^*)} + \theta \mathrm{X}_{it} + \gamma_t + \tau_i + \tau_i * t + \varepsilon_{it}, \quad (3)$$

where T^* is the year the policy is enacted and E^* is the year the policy is repealed. What results are three dummy variables, $W_{i(-3)}$, $W_{i(-2)}$, $W_{i(0)}$, that capture the years leading into the policy change (and provide a test of parallel pre-existing trends), and five dummy variables that serve to measure the effect of the policy by year since enactment. To account for the fact that every state has a different policy timeline, $W_{i(-3)}$ actually represents years less than and equal to -3, and $W_{i(5)}$ represents years greater than equal to 5 years. $Post_{i(0)}$ measures the any effect in the year the policy is repealed, $Post_{i(1)}$ the first full year after repeal, and so on. Similar to others, $W_{i(-1)}$, or the last full year prior to policy enactment, is dropped as the comparison group (Colman, Dee, & Joyce, 2013). All other notion remains the same from the previous equations.

[30] The classification of W_{it} assumes uniformity of effect between pre-Brady and Brady states. While there are distinct differences between Brady states and all else, comparing the empirical distributions of observables between Brady and non-Brady states reveals no systematic difference between the two groups—especially among political covariates. Those results are available upon request.

To provide some insight to the validity of the model—specifically with regard to policy endogeneity—we run multiple tests. If policy makers were responding to changes in gun related suicide or homicides rates by enacting these laws, we would expect to see suicide or homicide rates as significant predictors of the uptake of a law. These results and other model validity tests are available in the Appendix, and we find no evidence of this sort of policy endogeneity. Additionally, tests of randomly generated placebo laws also confirm the exogeneity of these laws. Lastly, the pre-existing parallel trends assumption tested in the event study suggests, similar to the other tests, that these laws were not endogenously created.

Results

Our main results are reported for homicides in Table 3 and suicides in Table 4 and Table 5. Tables 3 and 4 are organized such that each column represents a unique regression. The estimation of Equation (1) for each outcome is reported in columns (1), (2), (5), (6), (9) and (10), and the estimation of Equation (2) in the remaining columns. As is evident in Table 3, there appears to be no consistent statistically significant relationship between handgun delay policies and homicides. While handgun delay policies may alter the purchase of legitimate firearms, around 80% of criminal offenders report obtaining firearms through secondary or illegal markets (Ross et al., 2012). This suggests that a policy designed to interrupt the legitimate sale of firearms will not have any bite in secondary or illegal markets, which may explain why we see no statistically discernible difference in homicides after a change in purchase delay policies. This null finding holds when using firearm homicide data from the Uniform Crime Report, and when restricting the universe of firearm homicides to those committed by an "intimate partner".[31]

[31] Available in the Appendix

In contrast to the results in Table 3, we report in Table 4 that handgun delay policies do have a consistently negative and statistically significant effect on firearm related suicides. Specifically, we find that any policy that requires waiting to purchase a handgun decreases firearm-related suicides by about 2 percent. One concern may be a substitution effect between firearm suicides and non-firearm suicides (Briggs & Tabarrok, 2014). That is, discouraging firearm suicides may actually just encourage suicides by other means. Columns (9) through (12) in Table 4 examine non-firearm related suicides, and we find no statistically significant relationship between purchase delay policies and non-firearm related suicides. In fact, we find negative point estimates in most of the specifications. We also find, as reported in Table 4, that there is no additional effect from an especially long wait period, consistent with studies mentioned previously which find that the decision to attempt suicide can be, for many potential victims, discouraged by small interruptions.

In Table 5, and graphically displayed in Figure 3, we analyse a dynamic event study model of purchase delays as described in Equation (3). First, we see no evidence that pre-existing trends play a role in the determination of gun related suicides, as all of the lead-in point estimates are near to and statistically indistinguishable from zero.[32] The event study suggests that purchase delays reduce gun related suicides by approximately 5 percent and that this marginal effect is relatively stable while the policy remains in effect. Interestingly though, this effect of the policy seems to persist, at least initially, after the law is repealed. This may be the result of imperfect information about the changes of the policies on the part of would be gun owners and/or gun sellers. It could also be the case that the effects of purchase delay policies are not symmetrically reversible for other, unobserved reasons.

[32] A joint F-test of the policy lead in variables also fails to reject the null hypothesis that the policy lead-in variables are equal to zero.

Next, we estimate an additional set of results comparing youth firearm suicides to adult firearm suicides. The idea in these models is that youth firearm suicides shouldn't be affected by purchase delay laws, as the laws only affect legal purchases. However, we hesitate placing too much stock in this idea for two reasons. First, federal and state laws dictating legal purchasing age for a firearm are complicated and vary significantly (D. W. Webster, Vernick, Zeoli, & Manganello, 2004). Second, while it may seem imprudent to some, parents purchasing firearms for their minor children is a very common practice in many parts of the US.[33] Nonetheless, we do find that purchase delay laws have no detectable effect on youth firearm suicides and results comparable to our main results for adult suicides. These age-based results, as well as other demographic breakdowns in suicides by race and gender are available in the Appendix.

Lastly, we create a proxy for low, medium low, medium high and high gun prevalence by using per capita background check data and interact them with the main policy variable in equations (1) and (2). Graphically displayed in Figure 4, we find that that effect seems to be largest in states with relatively few firearms and that the effect dissipates as firearm prevalence increases. In areas where firearms are more common a potential victim of suicide has increased access to alternative ways of acquiring a firearm that do not involve a background check, such as making a purchase on the secondary market or borrowing from a friend or family member. This kind of situation attenuates the effect of a policy that works through interrupting retail purchases, like a mandatory purchase delay. A similar story is true when interacting the policy variable with quartiles of per capita real mental health spending—displayed in Figure 5. A further discussion of these results is available in the Appendix.

[33] See https://wpo.st/NR7d2 and https://n.pr/106H9Rq detailing the intricacies of purchasing firearms for minors and how the firearm industry targets, at least to a certain degree, marketing firearms to minors.

Discussion

In this paper, we compile a database of state-level gun restrictions between 1990 and 2013 to estimate the effects of handgun mandatory purchase delays on firearm related homicides and suicides. We find little to no evidence of a relationship between purchase delay policies and homicides. This may be due, as mentioned previously, to the avenues through which potential criminals obtain firearms. We do find, however, that any purchase delay policy reduces firearm-related suicides by about 2 percent on average and around 5 percent after enacted.[39] This result is both statistically and substantively significant. It suggests that if all 33 states currently without a mandatory purchase delay were to adopt one, then it would save more than 600 lives per year.[40] Our results add to a growing literature examining the relationship between firearms and suicide, and are congruent with the findings of previous seminal studies. For example, Ludwig and Cook (2000) find that the Brady Act impacted firearm suicides and these results were strongest among older individuals. Similarly, Lang (2013) finds a significant relationship between the stock of firearms and suicide, using background checks as a proxy for changes in the stock of firearms.

Relative to direct suicide intervention methods, a 2 to 5 percent decrease in suicides may seem small. Cox et al. (2013) surveyed a host of suicide prevention studies and found that the average decrease in suicides from direct intervention was around 60 percent. It is important to remember, though, that this paper surveyed single site studies where the intervention was highly localized and specific to the method of suicide being studied. While these studies report, in percentage terms, a large reduction in suicides, the total sum of suicides diverted in all of the

[39] The 2 percent estimate from Table 4 is a weighted average of the conditional difference in the suicide rate after enacting a purchase delay—which is the estimated effect from the event study in Table 5—and the difference in the suicide rate after purchase delay is repealed, which is initially small.
[40] This assumes a 5 percent decline from the combined 13,318 firearm related suicides in these 33 states in 2013.

studies surveyed likely totals less than 100.[41] Furthermore, we are only able to estimate the effect of handgun purchase delays as a suicide invention method at the margin of suicidal individuals that are affected by the policy. Because a purchase delay on handguns is unlikely to affect the behaviour of a suicidal individual who already has access to a firearm, our results are similar to an intent-to-treat effect. However, we can use a back of the envelope calculation to make our results comparable to those studied by Cox et al. (2013). A New Hampshire study found that about 8 percent of firearms used in suicides were purchased or rented within a week of death (Vriniotis et al., 2015). If we assume this to be generally true for the entire US population, then mandatory purchase delay law may prevent about 62 percent of all suicides committed by those individuals affected by the policy.[43] Under these assumptions, purchase delays have a similar efficacy to other suicide intervention methods.

Research into suicide also supports the perhaps surprising result that the actual number of days required to wait between purchase and delivery is not related to statistically significant declines in firearm related suicides. The shortest wait period in our data is 48 hours, and the time between a decision to commit suicide and an attempt is usually less than a day (Peterson et al., 1985). Furthermore, as mentioned previously, one study found that 70% of survivors of near-lethal suicide attempts deliberated less than one hour (M. Miller et al., 2012).

One might speculate that the mechanism by which purchase delays may deter firearm related suicides merely postpones, rather than discourages, suicides. However, the research on suicide suggests the contrary. Surviving the suicidal moment usually avoids suicide altogether,

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[41] As an example, one study might find a change from 8 suicides a year to 4 after subway employees received better suicide prevention training. While this is a "large" change in terms of percentages, the total number of suicides diverted is 4.

[43] If we think of our 5 percent estimate as the intent-to-treat effect and 8 percent to be the proportion treated by the policy, then the average treatment effect on the treated is 0.05/0.08=0.625

and the chance of survival goes up dramatically if there is no readily available firearm. Firearm suicide attempts succeed in about 85% of cases, as compared with an overall fatality rate for all methods of only 9% (M. Miller et al., 2012), and the vast majority of people who attempt suicide and survive die at a later date from a cause other than suicide (Owens et al., 2002). Moreover, our dynamic event study results show a stable, negative effect of purchase delays on suicides and our main results show no significant evidence of substitution effects into non-firearm suicide, providing more evidence that suicides are prevented rather than just delayed.

Firearms are a contentious and polarizing topic in American culture involving deeply rooted moral, social, and political beliefs. As a public policy however, purchase delays may offer a political middle ground. One public opinion poll showed that 74% of non-gun owners approved of a five-day wait period as did half of NRA members (Sides, 2012). Presumably, support would be even higher if wait periods were voluntary rather than mandatory (Vars, 2015).

A key element of depolarizing the normative debate about gun control and gun violence is establishing a foundation of facts about gun control policies and gun violence. From an economic perspective, firearms impart utility to gun owners through recreational use and as a method of self-defence. However, the availability of firearms also creates a negative externality for society by increasing the probability that a firearm will be misused as an instrument of violence. As such, policies that aim to strike a balance between the costs associated with restricting gun ownership and the negative externalities associated with improper use of firearms are likely welfare improving and also the most likely to be legislatively successful. What we find is that any delay policy associated with the purchase of a handgun can help to mitigate some of the negative externalities of gun ownership, specifically suicide. Furthermore, our results cast doubt on the benefits, if any, of a lengthy wait period. Thus the costs of purchase delays to responsible individuals could be minimized by not imposing excessively long delays.

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Tables

Table 1: State Handgun Delays Year of Change

<table>
<tr><th>State</th><th>Pre-1990</th><th>1991</th><th>1992</th><th>1993</th><th>1994</th><th>1995</th><th>1996</th><th>1997</th><th>1998</th><th>1999</th><th>2000</th><th>2001</th><th>2002-2009</th><th>2010-2013</th><th>Permit Year</th></tr>
<tr><td>Alabama</td><td colspan="4">2</td><td colspan="5">5</td><td colspan="2">2</td><td colspan="3">0</td><td></td></tr>
<tr><td>Alaska</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Arizona</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Arkansas</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>California</td><td colspan="9">15</td><td colspan="5">10</td><td></td></tr>
<tr><td>Colorado</td><td colspan="4">0</td><td colspan="5">exempt</td><td colspan="5">0</td><td></td></tr>
<tr><td>Connecticut</td><td colspan="10">14</td><td colspan="4">0</td><td>pre-1990</td></tr>
<tr><td>Delaware</td><td colspan="4">0</td><td colspan="5">exempt</td><td colspan="5">0</td><td></td></tr>
<tr><td>D.C.</td><td colspan="13">2</td><td>10</td><td>pre-1990</td></tr>
<tr><td>Florida</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Georgia</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Hawaii</td><td colspan="3">10</td><td colspan="11">14</td><td></td></tr>
<tr><td>Idaho</td><td colspan="4">0</td><td colspan="5">exempt</td><td colspan="5">0</td><td></td></tr>
<tr><td>Illinois</td><td colspan="14">3</td><td></td></tr>
<tr><td>Indiana</td><td colspan="9">7</td><td colspan="5">0</td><td></td></tr>
<tr><td>Iowa</td><td colspan="14">3</td><td></td></tr>
<tr><td>Kansas</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Kentucky</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Louisiana</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Maine</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Maryland</td><td colspan="14">7</td><td>1996</td></tr>
<tr><td>Massachusetts</td><td colspan="14">permit</td><td>pre-1990</td></tr>
<tr><td>Michigan</td><td colspan="14">permit</td><td>pre-1990</td></tr>
<tr><td>Minnesota</td><td colspan="14">7</td><td></td></tr>
<tr><td>Mississippi</td><td colspan="4">0</td><td colspan="5">5</td><td colspan="5">0</td><td></td></tr>
<tr><td>Missouri</td><td colspan="14">permit</td><td>pre-1990</td></tr>
</table>

Montana	0	5	0	
Nebraska	0	permit		1991
Nevada	0	5	0	
New Hampshire	5	exempt	0	
New Jersey	7			pre-1990
New Mexico	0	5	0	
New York	permit			pre-1990
North Carolina	permit			pre-1990
North Dakota	0	5	0	
Ohio	0	5	0	
Oklahoma	0	5	0	
Oregon	15			
Pennsylvania	2	5	0	
Rhode Island	3	7		pre-1990
South Carolina	0	5	0	
South Dakota	2	5	2 / 0	
Tennessee	15		0	
Texas	0	5	0	
Utah	0	exempt	0	
Vermont	0	5	0	
Virginia	0	exempt	0	
Washington	5			
West Virginia	0	5	0	
Wisconsin	2	5	2	
Wyoming	0	5	0	

Notes: States marked as "exempt" were exempted from Brady for having preexisting background check systems. States labeled "permit" are states with a permit requirement and thus impose a de facto delay through the permitting process. Light shading represents states that contribute to the identifying variation of the main policy variable, W_{it}. Dark shading identifies the states that contribute to the identifying variation of the week long wait variable, L_{it}.

Table 2: Summary Statistics

Variable	Full Sample	Purchase Delays	No Purchase Delay
Suicide Rate	12.92	11.12	14.60
	(3.58)	(2.98)	(3.26)
Firearm-Related Suicide Rate	7.40	5.79	8.91
	(3.06)	(2.86)	(2.40)
Non Firearm-Related Suicide Rate	5.52	5.33	5.69
	(1.56)	(1.34)	(1.72)
Homicide Rate	6.49	7.20	5.83
	(6.12)	(8.03)	(3.36)
Firearm-Related Homicide Rate	4.35	4.93	3.81
	(4.95)	(6.56)	(2.58)
Non Firearm-Related Homicide Rate	2.19	2.36	2.04
	(1.48)	(1.84)	(1.00)
Any Delay Policy	0.48	1.00	0.00
	(0.50)	(0.00)	(0.00)
Long Gun Wait Period	0.15	0.30	0.00
	(0.35)	(0.46)	(0.00)
License Requirement	0.20	0.42	0.00
	(0.40)	(0.49)	(0.00)
Brady Background Check	0.92	0.97	0.87
	(0.28)	(0.18)	(0.34)
Accidental Poisoning Rate	6.42	5.01	7.73
	(4.87)	(3.86)	(5.32)
Real Per Capita Mental Health Expenditures	77.59	82.53	72.95
	(52.31)	(56.03)	(48.15)
Fraction Male 45 to 64	29.33	28.52	30.08
	(3.72)	(3.38)	(3.86)
Fraction Black	0.09	0.10	0.09
	(0.12)	(0.13)	(0.11)
Accidental Firearm Death Rate	0.41	0.33	0.48
	(0.48)	(0.35)	(0.57)
Unemployment Rate	5.73	5.64	5.82
	(1.90)	(1.92)	(1.88)
Real Per Capita Income	39.32	41.26	37.50
	(7.50)	(8.55)	(5.79)
Per Capital Ethanol Consumption	2.36	2.34	2.38
	(0.53)	(0.42)	(0.61)
Urbanization Rate	0.73	0.78	0.68
	(0.16)	(0.15)	(0.15)
Infant Mortality Rate	756.82	762.65	751.37
	(457.63)	(281.97)	(575.65)
Proportion State House Democrat	0.52	0.57	0.48
	(0.17)	(0.16)	(0.16)
Proportion State Senate Democrat	0.52	0.56	0.49

Table 3: Purchase Delays on Homicides

	1.Total Homicides				2.Firearm Homicides				3.Non-Firearm Homicides			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Any Purchase Delay	-0.037	-0.022	-0.040	-0.025	-0.066	-0.051	-0.070	-0.061^	-0.001	0.014	0.000	0.019
	(0.044)	(0.035)	(0.046)	(0.037)	(0.047)	(0.033)	(0.049)	(0.034)	(0.039)	(0.038)	(0.040)	(0.040)
Long Wait (7+ Days)	.	.	0.025	0.054	.	.	0.046	0.123	.	.	-0.038	-0.050
	.	.	(0.055)	(0.046)	.	.	(0.095)	(0.081)	.	.	(0.080)	(0.077)
Controls		X		X		X		X		X		X
Sample Size	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224
R Squared	0.937	0.942	0.937	0.942	0.947	0.955	0.947	0.956	0.829	0.835	0.830	0.835

Notes: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the various homicide rates and the standard errors are clustered at the state level. All specifications include state and year fixed effects and state specific time trends. The controls included in even numbered columns are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, income, ethanol consumption, urbanizations, and infant mortality. ^ p<0.10 † p<0.05 ‡ p<0.01

Table 4: Purchase Delays on Suicides

	1. Total Suicides				2. Firearm Suicides				3. Non Firearm Suicides			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Any Purchase Delay	-0.019	-0.016	-0.016	-0.014	-0.031†	-0.022†	-0.029†	-0.020^	-0.004	-0.017	0.000	-0.014
	(0.013)	(0.010)	(0.013)	(0.011)	(0.013)	(0.011)	(0.013)	(0.010)	(0.018)	(0.016)	(0.018)	(0.016)
Long Wait (7+ Days)	.	.	-0.049	-0.045	.	.	-0.043	-0.041	.	.	-0.054	-0.050
	.	.	(0.030)	(0.027)	.	.	(0.027)	(0.030)	.	.	(0.045)	(0.039)
Controls		X		X		X		X		X		X
Sample Size	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224	1224
R Squared	0.958	0.965	0.958	0.965	0.982	0.983	0.982	0.983	0.897	0.914	0.897	0.914

Notes: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the various suicide rates and the standard errors are clustered at the state level. All specifications include state and year fixed effects and state specific time trends. The controls included in even numbered columns are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, income, ethanol consumption, urbanizations, and infant mortality. ^ $p<0.10$ † $p<0.05$ ‡ $p<0.01$

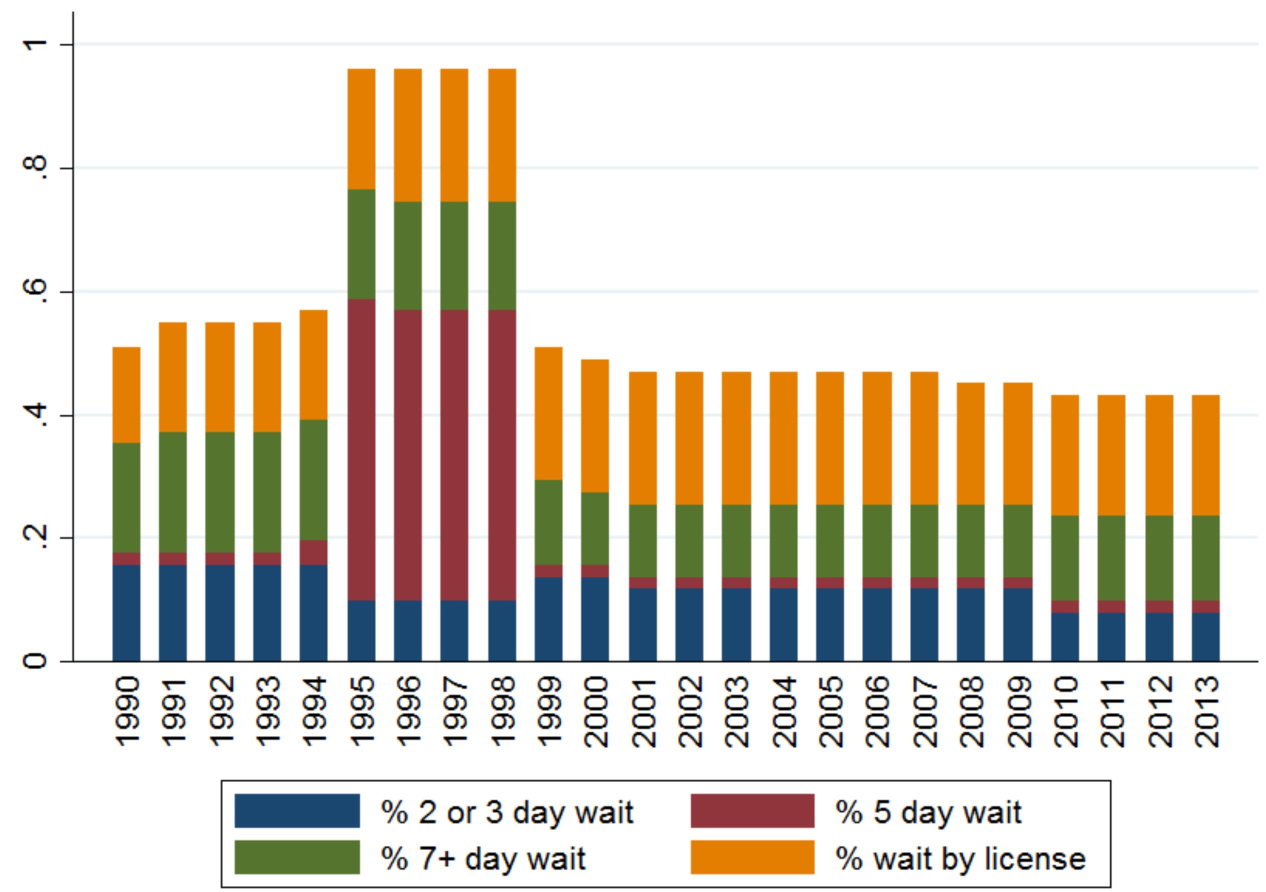
Table 5: Purchase Delay Event Study

	coefficient	se
Policy lead in		
3+ years prior	-0.001	(0.03)
2 years prior	0.007	(0.02)
1 year prior	*(omitted)*	
year passed (partial year)	-0.021	(0.03)
Policy years		
1st year	-0.047^	(0.03)
2nd year	-0.051^	(0.03)
3rd year	-0.050†	(0.02)
4th year	-0.056‡	(0.02)
5+ years	-0.041†	(0.02)
Years after policy repeal		
year annulled (partial year)	-0.063†	(0.03)
1st year	-0.040^	(0.02)
2nd year	-0.027	(0.02)
3rd year	-0.002	(0.02)
4+ years	-0.018	(0.02)
P value of lead ins=0	{0.84}	
P value of policy years=0	{0.01}	
Sample Size	1224	
R Squared	0.98	

Notes: Each observation is at the state-year level. The dependent variable is the natural log of the firearm related suicide rate and the standard errors are clustered at the state level. This specification include state and year fixed effects and state specific time trends. The controls included are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, income, ethanol consumption, urbanizations, and infant mortality. ^ $p<0.10$ † $p<0.05$ ‡ $p<0.01$

Figures

Figure 1: Timeline of Purchase Delays



Notes: The vertical distance for each group represents the fraction of the country with the associated law though the summation of the vertical lines by year slightly overestimates the true proportion of the country covered by purchase delays by year as many have both a purchase delay and a license requirement.

Figure 2: Geographical Variation in Purchase Delay Laws

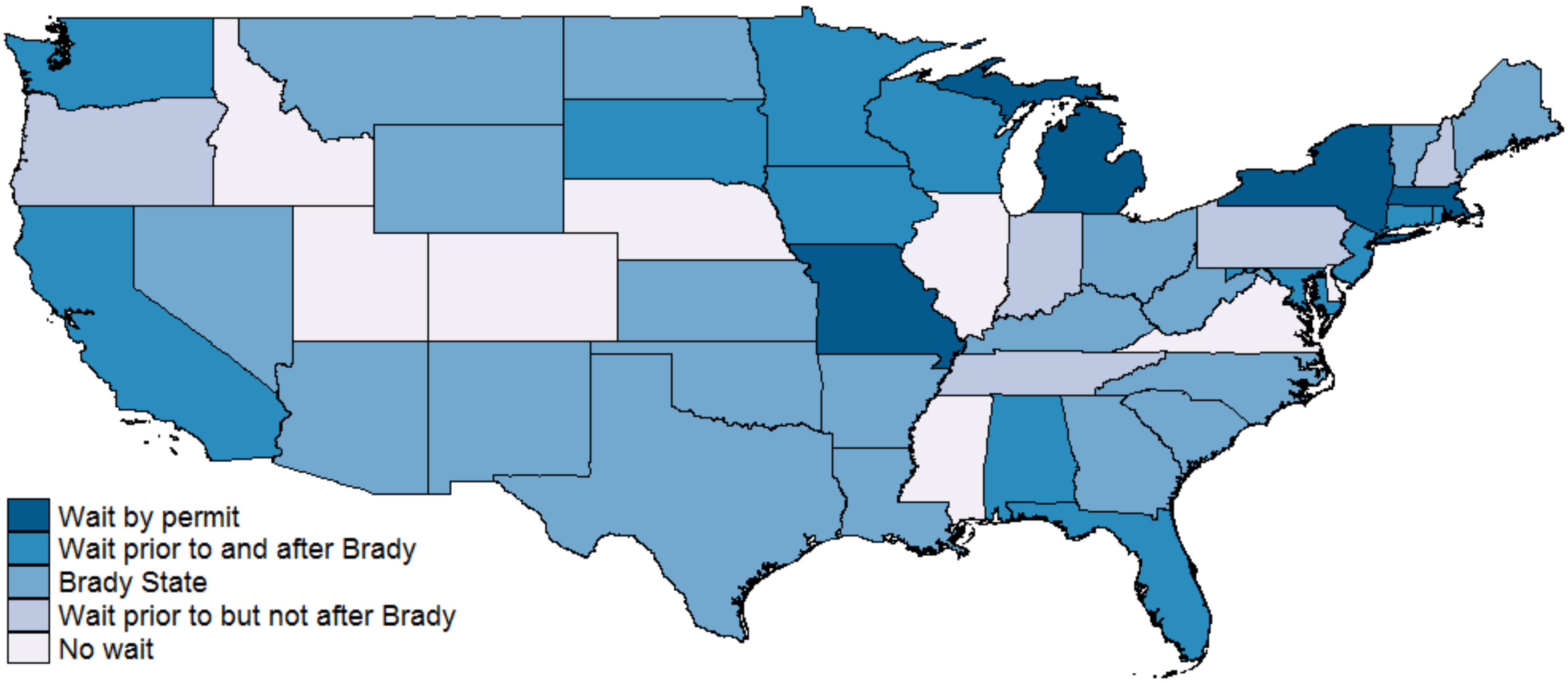
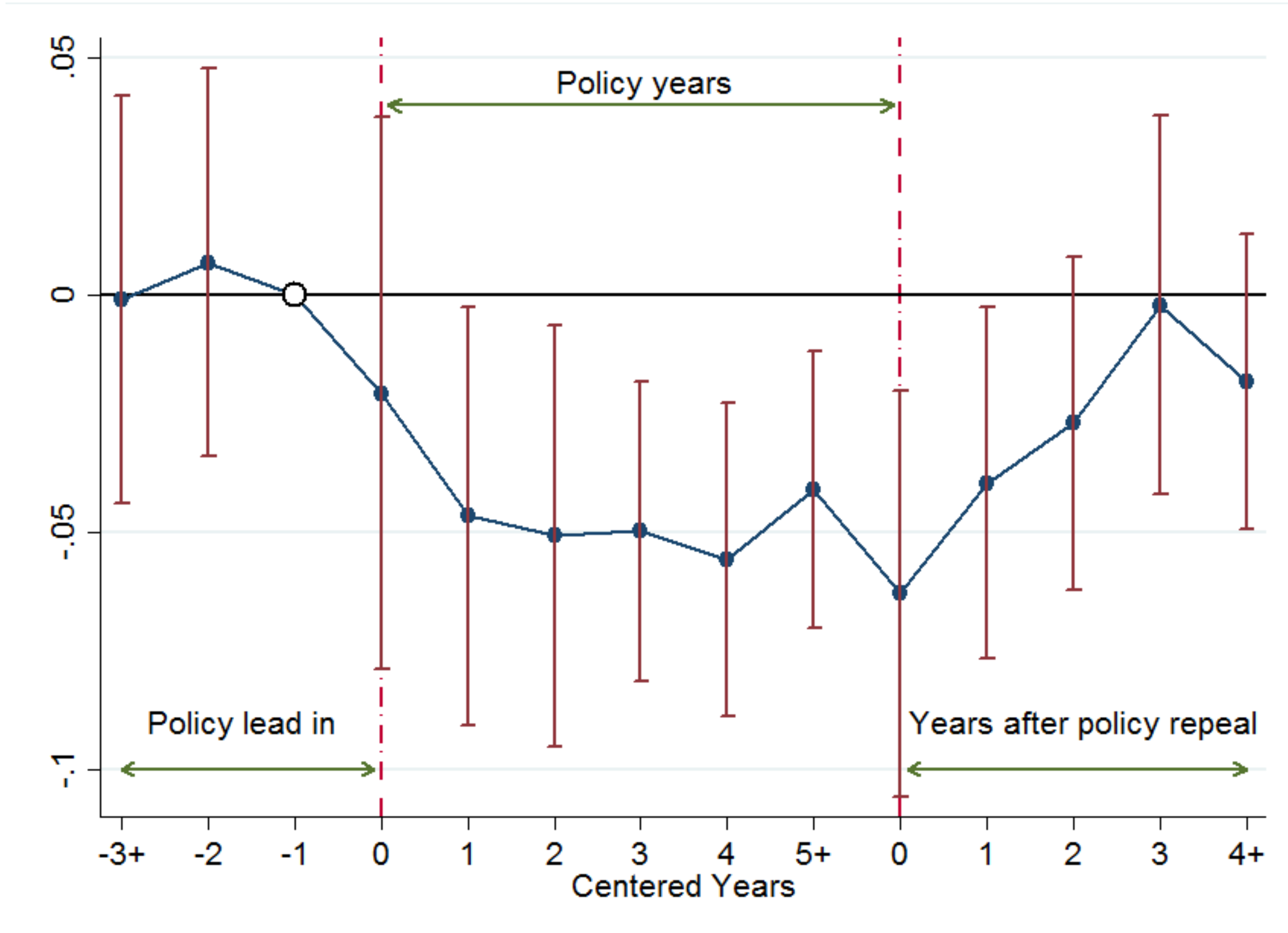
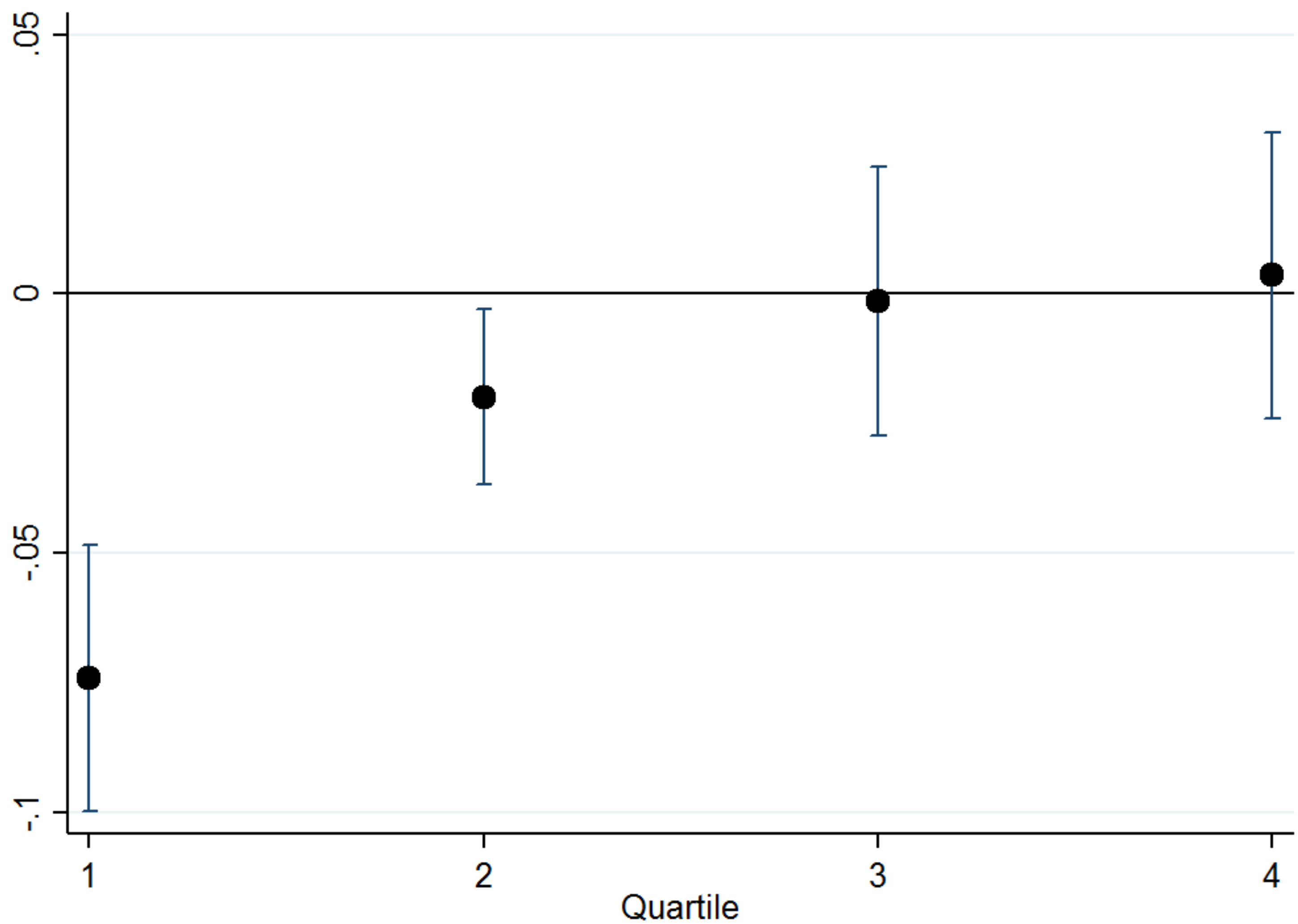


Figure 3: Event Study of Firearm Suicides



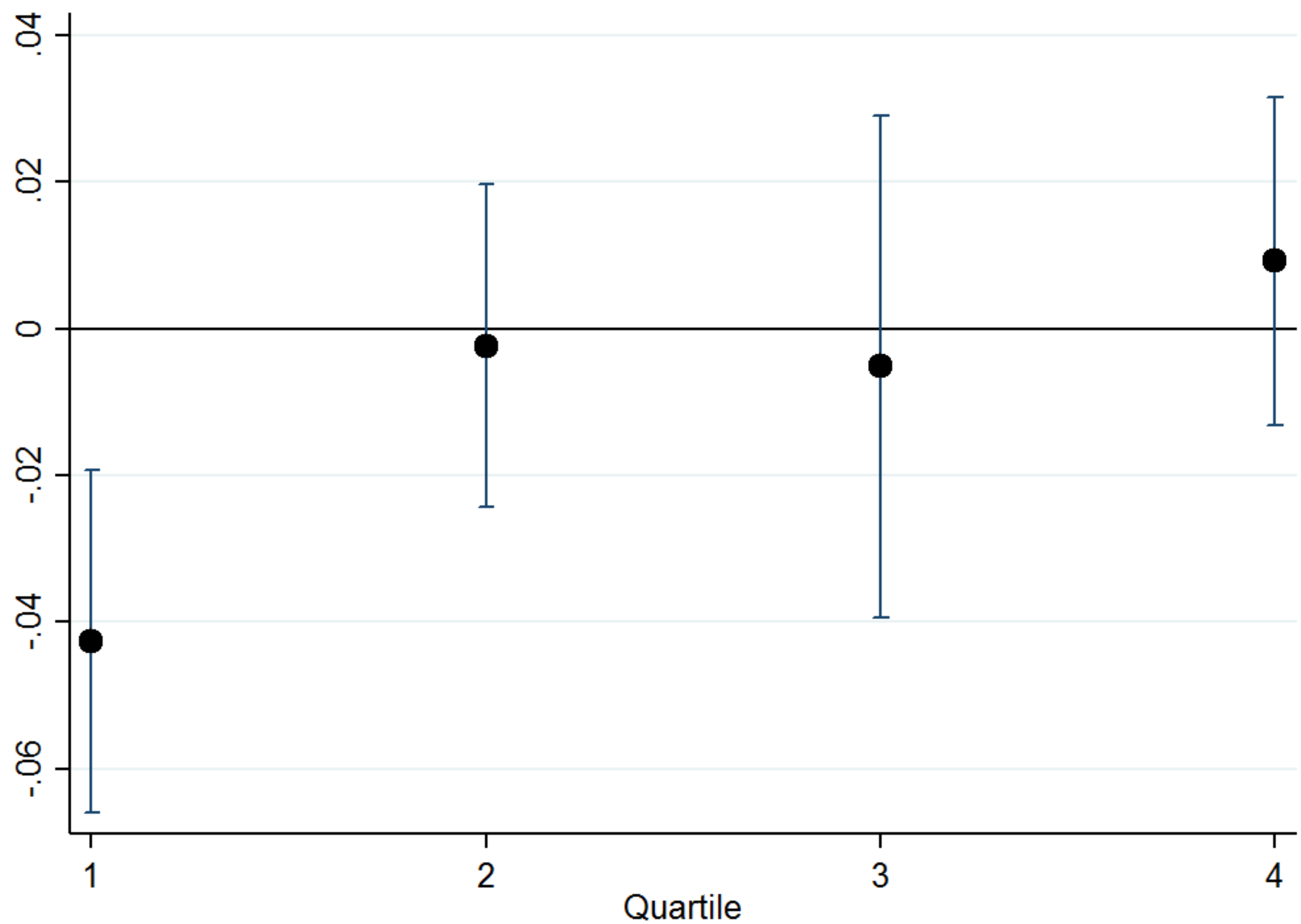
Notes: This figure show results from Table 5. The dots represent coefficients and the bands represent the 90% confidence interval.

Figure 4: Heterogeneous Purchase Delay Effect by Quartile of Background Checks



Notes: Bands represent 90% confidence intervals. Associated tables are available upon request.

Figure 5: Heterogeneous Purchase Delay Effect by Quartile of Mental Health Expenditures



Notes: Bands represent 90% confidence intervals. Associated tables are available upon request.