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Fredrick E. Vars, Joshua J. Robinson, Griffin S. Edwards & Erik Nesson, *Looking Down the Barrel of a Loaded Gun: The Effect of Mandatory Handgun Purchase Delays on Homicide and Suicide*, (2015). Available at: https://scholarship.law.ua.edu/fac_working_papers/167

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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 which 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 around 3 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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This paper benefited from helpful comments by Kathie Barnes, participants at the 2014 Southern Economic Association Annual Meetings, 2015 Midwestern Economic Association Meetings, 2015 Conference on Empirical Legal Studies and the 2016 meeting of the Tennessee Empirical Applied Micro Festival. Hope Henson, a student at the University of Alabama School of Law, provided countless hours of valuable research assistance. We don't blame our mistakes on others. The title references a song by singer/songwriter Tony Sly.
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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, 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 (FFL) 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

[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.

Brady expired in 1998 when the FBI launched the National Instant Criminal Background Check System (NICS).[4] 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 Centers 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 about 3%, 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. Prior to Brady, some states had wait periods or other purchase delays not accounted for in Ludwig and Cook. Additionally, Ludwig and Cook's data end in 1999, but we utilize variation in purchase delay laws from both federal and state statutes from 1991 through 2013. This allows us to exploit more variation by examining the laws leading

[4] 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.

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.

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; Jacobs & Potter, 1995). 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 Donohue (2003); H. Naci Mocan and Erdal Tekin (2006); Lott (1998); Mark Duggan (2001); 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 skeptical that delay policies have a significant effect on homicides.

Unlike homicides, the mechanism by which a delay may discourage suicides is more 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 (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 couple of days. 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 “few guns” effect, the research on the effect of gun prevalence on violent deaths (both homicides and suicides) 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.[5] 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 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. [6]

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

[5] 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.

[6] 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.

(Kleck and Patterson (1993), or didn't 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

Statutory delay of a handgun purchase falls into three broad categories. First, many states impose no delay. As of 2013, 33 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.[7] The waiting periods, described in Table 1, range from 48 hours in Wisconsin[8] to 14 days in Hawaii[9]. Lastly, some states require license, permit, or certificate requirements. The delay in these "permit"[10] states comes from the time required to process the necessary paperwork that accompanies the permit. Some states with permit-related delays have

[7] 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.
[8] Wis. Stat. Ann. § 175.35 (West 2014)
[9] Haw. Rev. Stat. § 134-2 (West 2014)
[10] For sake of ease, we refer to all sort of document based delay (permits, licenses, certificates, etc.) as permits, recognizing the differences in each.

enacted caps on the time the state can take to issue the permit.[11] The geographical variation in purchase delays can be seen in Figure 1.

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 his attempted assassination of President Reagan in 1981. The role Brady has had on violent outcomes has been well researched.[12] In a leading study, Ludwig and Cook (2000) examined the role Brady played on homicide, firearm homicide, suicide, and firearm suicide rates with a panel from 1985 to 1997. They found that the firearm suicide rate among people aged 55 years or older declined with Brady by a statistically significant amount, but the Brady law implementation had no other statistically significant effects on the outcome measures.[13]

Many states however already required background checks and thus were exempted from Brady—including the five-day wait period.[14] In 1998, the interim five-day wait period expired by the terms of the statute. 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.[15] Having a mandated wait and a permit requirement are not mutually exclusive policies in our dataset.[16] While rifles and shotguns are more quickly available

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[11] 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)).
[12] *See* D. W. Webster and Vernick (2013) for a summary of the Brady literature.
[13] What we attempt to answer in this project differs from Ludwig 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.
[14] *See* Ludwig and Cook (2000)
[15] *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 it's wait in 2010.
[16] *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

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.

Data

Our dependent variables of interest are logged suicide and homicide rates in each state and year. Using the Centers for Disease Control and Preventions' WISQARS database,[17] we collect the number of firearm and non-firearm related homicides and suicides between 1990 and 2013.[18]

Our independent variables of interest measure delays between the purchase and delivery of a handgun and are described in Table 2.[19] To account for the role background check laws may have had 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.[20]

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

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

[17] While we consider the CDC 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.

[18] The most reliable statutory histories come from this time frame.

[19] We code the laws as "treated" from the first full year of enactment. The results presented here are generally insensitive to alternative coding of the laws.

[20] This Brady background check variable assumes states that exempted out of Brady had a background check for the entire period between 1991 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.

year from the WISQARS database 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 coroner's standards for what constitutes a homicide or suicide as opposed to 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 effect. Briggs and Tabarrok (2014) also note that the rare occurrence of accidental gun deaths is problematic because the CDC suppresses state-year death numbers less than 10. We deal with this by creating a dummy variable for having less than 10 accidental firearm deaths per state per year and an additional variable that captures the variation in the logged rate with the number of accidental firearm deaths exceeds 10.

Additionally, we attempt to 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 live in urban areas, as well as the infant mortality rate, the unemployment rate and real per capita income.[21] 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, Yakovlev, and Carson (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

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

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 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, and while we do not have data on the states that require a background check for private sales, in the Background section, we do address background checks generally, and any state that would require a background check for a private sale would require that check to be performed through the same channel as official sales and would thus be captured by in the same manner that we address earlier.

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, W_{it} is an indicator variable that denotes if state i in time t has any sort of delay policy, X is a vector of controls outlined in the data section, γ is a set of year fixed effects, τ is the set of state fixed effects and $\tau_i * t$ are state specific time trends. Each regression is weighted by state population,[22] and standard errors are clustered at the state level.

We expand our specification in Equation (1) to measure whether the length of the statutory delay affects the policy effectiveness with the following specification:

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

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, P_{it} is a dummy variable capturing whether or not the state has a permit or license requirement, and all other variables are the same as in Equation (1). 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 φ can be thought of as any additional effect a long wait may have, and δ can be thought of as any additional effect a license or permit may create.[23]

[22] 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 column 2 of Table 4.

[23] 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.

We also use an event-study specification to both estimate the dynamic effects of purchase delay policies. This framework also 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):

$$\ln(s_{it}) = \sum_{k=-3}^{-2} a_k Pre_k + \sum_{v=0}^{5} b_v T_v + \sum_{z=0}^{4} c_z Post_z + \theta \mathrm{X}_{it} + \gamma_t + \tau_i + \tau_i * t + \varepsilon_{it}, \quad (3)$$

where the Pre_k variables are a set of dummies that indicate the years prior to a purchase delay, with Pre_{-3} indicating three or more years prior and Pre_0 indicating the partial year the law is enacted. The set of T_v variables measure the effect of the policy for each full year the policy is enacted, where T_1 is the first full year of the policy and T_5 represents at least 5 years since enactment. The $Post_z$ variables capture the post-repeal effects of purchase delays where $Post_0$ indicates the effect of the partial year when the policy turns off, $Post_1$ is the first year off, etc., and $Post_4$ represents 4 or more years since the policy was removed. Similar to others, the last full year prior to the enactment of the policy (k=-1) is omitted as the reference category (Colman, Dee, & Joyce, 2013).

Model Validity

As is often the case with any policy passed by legislators, there is concern that the laws were passed endogenously to the outcome variable. We alleviate this concern as much as possible in our main results by including controls that capture the political atmosphere of each state and including state specific time trends. This policy endogeneity story does assume though that those who make laws are aware of the trends in the outcome variable, use those trends as justification for proposing a change in policy, and ultimately are able to successfully navigate the legislative process to see the law changed. While this may seem to be an unlikely series of events, especially as it applies to suicides, if it were to happen, we should observe that our lagged outcome variable should predict the uptake of a law. We formally test this by estimating the following:

$$W_{it} = \sum_{k=1}^{3} \lambda_k \ln(s_{it-k}) + \theta \mathrm{X}_{it} + \gamma_t + \tau_i + \tau_i * t + \varepsilon_{it} \quad (4)$$

where W_{it} is our policy variable described above, s_{it-k} is our outcome variables of interest lagged between one and three years, and the rest carries over from models (1) and (2). The results of Equation (4) are reported for suicides in Table 3 and homicides in Table 4.

In each table, the first block of rows includes just state specific time trends,[24] the second adds state fixed effects, and the third adds the controls mentioned previously. In Table 3, each block of columns includes as the lagged outcome total suicides, firearm suicides, and non-firearm suicides. At the bottom of each column block we report the p value from a joint test of significance on the lagged outcome variables. As noted in Table 3, suicides only predict the uptake of a delay law in two of the 27 specifications at the ten percent level which is about the frequency that we would expect for a Type 1 error. Additionally, an event study analysis of our main suicide results, described in Equation (3) and discussed further below, suggests no significant pre-existing trends in suicides.

We do find, though, that lagged homicide rates much more frequently predict the uptake of a delay law. In Table 4, 12 of the 27 specifications suggest that the lagged crime rates are jointly significant, which may suggest that the laws were passed with homicides in mind. However, the signs on the lagged homicide rates are consistently negative, indicating that higher levels of homicides decrease the probability that the law is passed. This works contrary to the

[24] As mentioned previously, the state specific raw trends suggest the need for time trends.

typical notion that legislators observe high homicide rates and respond endogenously by passing stricter gun laws.[25]

Additionally, we test the exogeneity of the laws by replicating Equation (1), with and without controls, using randomly generated placebo laws that mirror the same proportional motion of policy changes in our dataset.[26] Running this simulation 200 times, we estimate the rejection rate of the null on our placebo policy variable to be 0.12 (no controls) and 0.11 (with controls) at the 10% level.[27] Neither rejection rate is statistically different from the typical Type 1 error rejection rate of 0.10.

Results

Our main results are reported for homicides in Table 5 and suicides in Table 6. Each table is organized into column blocks where each block of columns represents a different outcome and each column within a block (i.e. (1.a.), (1.b), etc.) represents a unique regression. The estimation of Equation (1) for each outcome is reported in the first block of rows in each table and the estimation of Equation (2) in the second block of each table. As is evident in Table 5, regardless of the specification, 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 (Jacobs & Potter, 1995). This suggests that a policy

[25] It may suggest that legislators respond to homicide rates with the purpose of "arming his/her constituents" though this case may seem less likely.
[26] We do this by randomly generating a placebo "Brady year" then randomly assigning that year to 41% of the states and turn that law off after four years. We generate unique random passage years for another 39% of states, and the remaining 20% of states serve as controls. While we think this is the most appropriate placebo law test for this paper, we find no different results when apply a more traditional approach to generating placebo laws.
[27] These results, as well as the empirical density functions the result from these simulations are available upon request.

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.

In contrast to the results in Table 5, we report in Table 6 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 3%. 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 (3.a) through (3.d) in Table 6 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 6, that there is no additional effect from an especially long wait period or a permit, consistent with studies mentioned previously which find that the decision to attempt suicide can be, for many potential victims, discouraged by small interruptions.

We test the robustness of our results to a number of alternative specifications of gun delays, including using a quasi-continuous variable for the length of delay and a specification including a dummy variable for really short waits (2 to 3 days) and really long waits (7 or more days). While these results are generally robust to these alternative specifications, we are cautious to interpret the estimated coefficients of δ and φ in columns (2.c) and (2.d) as they identify off of changes in only a few states, and we prefer the specification in (2.a) and (2.b).

We further test our finding that purchase delay policies have no statistically significant effect on firearm-related homicides using data from the Uniform Crime Reports (UCR). After estimating our preferred specification using the UCR data, we find coefficient estimates that are largely consistent with our estimates from the CDC data and also not statistically significant. Finally, there may be reason to believe that purchase delays could have an effect on "intimate partner" homicide, defined as homicide by a spouse/partner/boyfriend/girlfriend. That is, purchase delays may have a greater impact in deterring impulsive "crimes of passion". We test for this using the Supplemental Homicide Reports of the UCR to narrow the pool of homicides to only include intimate partner homicide and find no evidence of an effect.[28]

Robustness Checks

A major concern with any difference-in-difference model is that seemingly causal point estimates may be the result of pre-existing trends or, more generally, that they may obfuscate a more complex dynamic relationship. We examine the dynamic properties of purchases delay policies using an event study. The estimates of Equation (3) can be found in Table 7 and Figure 2. As can be seen in Figure 2, there is no evidence that pre-existing trends play a role in the determination of gun related suicides, as all of the point estimate are near to and statistically indistinguishable from zero.[29] We observe a relatively stable effect of purchase delays by year on gun related suicides. Interestingly though, this effect 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.

Additionally, we check the robustness of the main results in our preferred specifications by dividing the purchase delay variable into four separate dummy variables. The resulting equation is:

[28] Those results are available in the Appendix.
[29] A joint F test of the policy lead in variables suggest the same.

$$\ln(s_{it}) = \alpha + \beta^1 W_{it}^1 + \beta^2 W_{it}^2 + \beta^1 W_{it}^3 + \beta^4 W_{it}^4 + \delta L_{it} + \varphi P_{it} + \theta \mathrm{X}_{it} + \gamma_t + \tau_i + \tau_i * t + \varepsilon_{it} \quad (5)$$

Where all variables and parameters are the same as Equation (2) except for altering of the purchase delay variable. We alter the purchase delay variables in two ways. First, we calculate the average real mental health spending per capita of each state across all available years and sort states into quartiles from lowest to highest spending.[30] Whereas the estimate of β in Equation (2) measures the average effect of a purchase delay across all levels of per capita mental health spending, here β^1 measures the effect of purchase delays in the lowest quartile of spending states, β^2 measures the effect of purchase delays on the next quartile of spending, and so on. Figure 3 displays the coefficients and 95% confidence intervals for the purchase delay coefficients by quartile of mental health spending. While we urge the same caution mentioned previously with regard to relatively few state/year cells to identify off of for each estimate in Figure 3, what we see generally is that the impact of these purchase delays is felt most strongly in states in the lowest quartile of mental health spending. That is, we find that purchase delays have the biggest effect in states with the fewest resources dedicated to treating mental illness. This is plausible given that mental health interventions have been shown to prevent suicide (Zalsman et al. 2016).

In Figure 4, we report the results of our estimation of Equation (5), except in place of grouping states by mental health expenditures, we group states by the average number of per capita background checks.[31] The idea being, again, that there is reason to believe that purchase delay laws may affect states with lots of background checks, and subsequently lots of available

[30] The lowest quartile of spending, where we find our main result, includes MI, IL, NC, FL, OH, CA, NY, TX, VA, IN, KY, and GA.

[31] While we do not include background checks in our main specifications due to a lack of data prior to 1998, we are able to use the available background check data to calculate state averages and bin states into quartiles.

firearms, different from states with few background checks and maybe relatively fewer guns readily accessible. As seen in Figure 4, we find that purchase delays have the biggest effect among in the lowest two quartiles and a statistically indistinguishable effect among the higher two quartiles. If background checks are a believable proxy for state firearm activity, then these results would suggest that purchase delays could be most effective in states where firearm availability is relatively low. And while we again express caution in the interpretation given limiting identifiable variation, this may be the case because in higher quartile background check states obtaining a firearm through un-official channels—family/friends/neighbor/etc.—is more feasible than in low quartile states. That is, the need to purchase a firearm from licensed sellers—thus being subject to purchase delays—is more likely in states with fewer firearms.

Discussion

In this paper, we compile a database of state-level gun restrictions between 1990 and 2012 to estimate the effects of handgun mandatory purchase delays on firearm related homicides and suicides. We find no statistically significant 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 3 percent. This result is both statistically and substantively significant, corresponding to roughly 400 lives saved annually.[32]

As mentioned previously, this is consistent with previous research that has found that oftentimes guns used in suicide attempts are recent purchases. For example, a two-site Tennessee-Washington study found that 5% of firearm-related suicide victims used a firearm

[32] This estimate assumes some sort of purchase delay adoption by each state that currently does not require a delay.

obtained within two weeks (Kellermann et al., 1992).[33] A smaller New Hampshire study found that at least 8% of firearms used in suicides were purchased or rented within a week of death (Vriniotis et al., 2015). In other words, the results we report here are consistent with other firearm tracing studies that suggest that a nontrivial amount of firearms are purchased near the date of attempted suicide.

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 delays, rather than discourages, suicides. However, the research on suicide suggests the contrary. Surviving the suicidal moment usually avoids suicide altogether, 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).

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

[33] Kellermann et al. (1992) only ask about the date of firearm purchase for the final 162 cases they interview. We scaled those 162 cases by 58% (the overall number of firearm related suicides in their study) to provide the denominator for our calculation of 5%.

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-defense. 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

State	Pre-1990 Wait Length	First Change in Wait (Year)	Second Change in Wait (Year)	Third Change in Wait (Year)	Permit Year	Citation(s)
Alabama	2	5 (1994)	2 (1999)	0 (2001)		Ala. Code § 13A-11-77
Alaska		5 (1994)	0 (1999)			18 U.S.C. § 921
Arizona		5 (1994)	0 (1999)			18 U.S.C. § 921
Arkansas		5 (1994)	0 (1999)			18 U.S.C. § 921
California	15	10 (1998)				Cal. Penal Code § 26815
Colorado						
Connecticut	14	0 (2000)			pre-1990	Conn. Gen. Stat. § 29-28, -37a
Delaware						
D.C.	2	10 (2010)			pre-1990	D.C. Code §§ 22-4508 & 7-2502
Florida		3 (1991)				Fla. Const. art. 1, § 8; Fla. Stat. § 790.0655
Georgia		5 (1994)	0 (1999)			18 U.S.C. § 921
Hawaii	10	14 (1993)				Haw. Rev. Stat. § 134-2
Idaho						
Illinois	3					720 Ill. Comp. Stat. § 5/24-3; 430 Ill. Comp. Stat. § 65/2
Indiana	7	0 (1999)				Ind. Code § 35-47-2-8
Iowa	3					Iowa Code §§ 724.20 & 724.15
Kansas		5 (1994)	0 (1999)			18 U.S.C. § 921
Kentucky		5 (1994)	0 (1999)			18 U.S.C. § 921
Louisiana		5 (1994)	0 (1999)			18 U.S.C. § 921
Maine		5 (1994)	0 (1999)			18 U.S.C. § 921
Maryland	7				1996	Md. Code, Public Safety, §§ 5-123, -124, -117, -117.1
Massachusetts					pre-1990	Mass. Gen. L. 140 §§ 131E, 129B, 131, 131A
Michigan					pre-1990	Mich. Comp. Laws Ann. §§ 28.422, 28.425b
Minnesota	7					Minn. Stat. §§ 624.7132 , .7131, .714
Mississippi		5 (1994)	0 (1999)			18 U.S.C. § 921
Missouri					pre-1990	Vernon's Ann. Mo. Stat. § 571.080
Montana		5 (1994)	0 (1999)			18 U.S.C. § 921
Nebraska					1991	Neb. Rev. Stat. §§ 69-2403, -2404, -2405
Nevada		5 (1994)	0 (1999)			18 U.S.C. § 921
New Hampshire		5 (1994)	0 (1995)			

New Jersey	7				pre-1990	N.J. Stat. §§ 2C:58-2, -3
New Mexico		5 (1994)	0 (1999)			18 U.S.C. § 921
New York					pre-1990	N.Y. Penal Law § 400.00
North Carolina		5 (1994)	0 (1995)		pre-1990	N.C. Gen. Stat. §§ 14-402, -404
North Dakota		5 (1994)	0 (1999)			18 U.S.C. § 921
Ohio		5 (1994)	0 (1999)			18 U.S.C. § 921
Oklahoma		5 (1994)	0 (1999)			18 U.S.C. § 921
Oregon	15	0 (1995)				Ore. Rev. Stat. § 166.420
Pennsylvania	2	5 (1994)	0 (1999)			18 U.S.C. § 921
Rhode Island	3	7 (1991)			pre-1990	R.I. Gen. L. §§ 11-47-35, -35.2
South Carolina		5 (1994)	0 (1999)			18 U.S.C. § 921
South Dakota	2	5 (1994)	2 (1999)	0 (2010)		S.D. Comp. L. § 23-7-9
Tennessee	15	0 (1999)				Tenn. Code § 39-17-1316
Texas		5 (1994)	0 (1999)			18 U.S.C. § 921
Utah						
Vermont		5 (1994)	0 (1999)			18 U.S.C. § 921
Virginia						
Washington	5					Rev. Code Wash. § 9.41.090
West Virginia		5 (1994)	0 (1999)			18 U.S.C. § 921
Wisconsin	2					Wis. Stat. § 175.35
Wyoming		5 (1994)	0 (1999)			18 U.S.C. § 921

Table 2: Summary Statistics

Variable	Mean	S.D.
Suicide Rate	12.92	3.58
Firearm-Related Suicide Rate	7.40	3.06
Non Firearm-Related Suicide Rate	5.52	1.56
Homicide Rate	6.49	6.12
Firearm-Related Homicide Rate	4.31	4.84
Non Firearm-Related Homicide Rate	2.18	1.45
Any Delay Policy	0.48	0.50
Long Gun Wait Period	0.15	0.35
License Requirement	0.20	0.40
Brady Background Check	0.92	0.28
Accidental Poisoning Rate	6.42	4.87
Real Per Capita Mental Health Expenditures	77.59	52.31
Fraction Male 45 to 64	29.33	3.72
Fraction Black	0.09	0.12
Accidental Firearm Death Rate	0.46	0.50
Unemployment Rate	5.73	1.90
Real Per Capita Income	39.32	7.50
Per Capital Ethanol Consumption	2.36	0.53
Urbanization Rate	0.73	0.16
Infant Mortality Rate	756.82	457.63
Proportion State House Democrat	0.52	0.17
Proportion State Senate Democrat	0.52	0.17

Table 3: Predicting Any Delay Policies with Lagged Suicide Rates

	Total Suicides			Firearm Suicides			Non Firearm Suicides		
State Specific Time Trends	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 Year Lag	-0.028	0.118	0.254	-0.114	-0.005	0.107	-0.281†	-0.121	-0.068
	(0.166)	(0.132)	(0.160)	(0.142)	(0.099)	(0.106)	(0.131)	(0.088)	(0.094)
2 Year Lag	.	-0.054	0.025	.	-0.075	0.000	.	-0.222†	-0.133
	.	(0.152)	(0.122)	.	(0.136)	(0.110)	.	(0.100)	(0.084)
3 Year Lag	.	.	-0.068	.	.	-0.132	.	.	-0.144†
	.	.	(0.158)	.	.	(0.151)	.	.	(0.069)
Joint Test P value	{0.867}	{0.454}	{0.349}	{0.424}	{0.783}	{0.533}	{0.037}	{0.094}	{0.169}
SSTT & Fixed Effects	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1 Year Lag	-0.197	-0.207	-0.198	-0.170	-0.171	-0.112	-0.088	-0.119	-0.173^
	(0.187)	(0.150)	(0.137)	(0.117)	(0.117)	(0.109)	(0.121)	(0.089)	(0.095)
2 Year Lag	.	-0.215	-0.267†	.	-0.123	-0.133	.	-0.149	-0.188‡
	.	(0.147)	(0.115)	.	(0.109)	(0.120)	.	(0.105)	(0.077)
3 Year Lag	.	.	-0.120	.	.	-0.032	.	.	-0.132
	.	.	(0.149)	.	.	(0.124)	.	.	(0.080)
Joint Test P value	{0.297}	{0.284}	{0.144}	{0.155}	{0.352}	{0.587}	{0.473}	{0.321}	{0.112}
SSTT, FE, & Controls	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1 Year Lag	-0.150	-0.131	-0.093	-0.096	-0.091	-0.040	-0.093	-0.089	-0.112
	(0.127)	(0.113)	(0.119)	(0.091)	(0.094)	(0.096)	(0.091)	(0.068)	(0.069)
2 Year Lag	.	-0.189	-0.203^	.	-0.066	-0.067	.	-0.170^	-0.183‡
	.	(0.121)	(0.109)	.	(0.097)	(0.103)	.	(0.094)	(0.075)
3 Year Lag	.	.	-0.123	.	.	-0.043	.	.	-0.131^
	.	.	(0.115)	.	.	(0.098)	.	.	(0.073)
Joint Test P value	{0.246}	{0.282}	{0.312}	{0.294}	{0.628}	{0.931}	{0.316}	{0.200}	{0.118}

Notes: Each column represents a unique regression, and each suicide outcome is measured as a logged rate. Each observation is at the state-year level. The dependent variable is an indicator variable that specifies state-years that have some sort of hand gun delay. The standard errors are clustered at the state-year level. The controls included in bottom block are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: real mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, real income, ethanol consumption, urbanization, and infant mortality. ^ p<0.10 † p<0.05 ‡ p<0.01

Table 4: Predicting Any Delay Policies with Lagged Homicide Rates

	Total Homicides			Firearm Homicides			Non Firearm Homicides		
State Specific Time Trends	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 Year Lag	-0.133^	-0.102^	-0.066	-0.058	-0.061	-0.020	-0.181‡	-0.157‡	-0.130‡
	(0.070)	(0.054)	(0.055)	(0.067)	(0.052)	(0.054)	(0.070)	(0.056)	(0.050)
2 Year Lag	.	-0.030	-0.062	.	0.023	-0.035	.	-0.138‡	-0.128‡
	.	(0.077)	(0.044)	.	(0.085)	(0.034)	.	(0.048)	(0.047)
3 Year Lag	.	.	0.061	.	.	0.069	.	.	-0.050
	.	.	(0.117)	.	.	(0.113)	.	.	(0.041)
Joint Test P value	{0.064}	{0.175}	{0.214}	{0.394}	{0.513}	{0.346}	{0.013}	{0.020}	{0.073}
SSTT & Fixed Effects	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1 Year Lag	-0.121	-0.096	-0.067	-0.139†	-0.101†	-0.074^	-0.049	-0.047	-0.036
	(0.090)	(0.067)	(0.053)	(0.065)	(0.050)	(0.040)	(0.051)	(0.047)	(0.046)
2 Year Lag	.	-0.122†	-0.114‡	.	-0.122‡	-0.113‡	.	-0.058	-0.054
	.	(0.055)	(0.047)	.	(0.047)	(0.039)	.	(0.047)	(0.044)
3 Year Lag	.	.	-0.061	.	.	-0.069	.	.	-0.022
	.	.	(0.060)	.	.	(0.066)	.	.	(0.035)
Joint Test P value	{0.184}	{0.093}	{0.064}	{0.038}	{0.017}	{0.031}	{0.346}	{0.396}	{0.328}
SSTT, FE, & Controls	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
1 Year Lag	-0.086^	-0.080^	-0.070	-0.108‡	-0.094‡	-0.081†	-0.024	-0.024	-0.018
	(0.047)	(0.044)	(0.043)	(0.038)	(0.038)	(0.037)	(0.026)	(0.026)	(0.025)
2 Year Lag	.	-0.044	-0.046	.	-0.067^	-0.068^	.	-0.010	-0.009
	.	(0.038)	(0.042)	.	(0.037)	(0.040)	.	(0.025)	(0.025)
3 Year Lag	.	.	0.012	.	.	-0.013	.	.	0.019
	.	.	(0.041)	.	.	(0.046)	.	.	(0.023)
Joint Test P value	{0.076}	{0.183}	{0.336}	{0.007}	{0.049}	{0.172}	{0.362}	{0.427}	{0.369}

Notes: Each column represents a unique regression, and each homicide outcome is measured as a logged rate. Each observation is at the state-year level. The dependent variable is an indicator variable that specifies state-years that have some sort of hand gun delay. The standard errors are clustered at the state-year level. The controls included in bottom block are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: real mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, real income, ethanol consumption, urbanization, and infant mortality. ^ $p<0.10$ † $p<0.05$ ‡ $p<0.01$

Table 5: Purchase Delays on Homicides

	1.Total Homicides		2.Firearm Homicides		3.Non Firearm Homicides	
	(1.a)	(1.b)	(2.a)	(2.b)	(3.a)	(3.b)
Any Purchase Delay	-0.037	-0.020	-0.067	-0.049	-0.003	0.014
	(0.044)	(0.036)	(0.046)	(0.033)	(0.039)	(0.038)
Sample Size	1224	1224	1224	1224	1223	1223
R Squared	0.937	0.942	0.944	0.953	0.828	0.835
	(1.c)	(1.d)	(2.c)	(2.d)	(3.c)	(3.d)
Any Purchase Delay	-0.040	-0.023	-0.071	-0.058	-0.002	0.020
	(0.046)	(0.037)	(0.049)	(0.035)	(0.041)	(0.040)
Long Wait (7+ Days)	0.025	0.056	0.045	0.112	-0.038	-0.057
	(0.055)	(0.045)	(0.094)	(0.081)	(0.079)	(0.072)
License/Permit Requirement	-0.001	-0.025	-0.008	-0.027	0.041	-0.006
	(0.114)	(0.099)	(0.154)	(0.126)	(0.040)	(0.049)
Controls		X		X		X
Sample Size	1224	1224	1224	1224	1223	1223
R Squared	0.937	0.942	0.944	0.953	0.828	0.835

Notes: Each column block 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 columns 1.b, 1.d, 2b,2d etc. are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: real mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, real income, ethanol consumption, urbanization, and infant mortality. ^ p<0.10 † p<0.05 ‡ p<0.01

Table 6: Purchase Delays on Suicides

	1.Total Suicides		2.Firearm Suicides		3.Non Firearm Suicides	
	(1.a)	(1.b)	(2.a)	(2.b)	(3.a)	(3.b)
Any Purchase Delay	-0.019	-0.016	-0.031†	-0.022†	-0.004	-0.017
	(0.013)	(0.011)	(0.013)	(0.011)	(0.018)	(0.016)
Sample Size	1224	1224	1224	1224	1224	1224
R Squared	0.958	0.965	0.982	0.983	0.897	0.914
	(1.c)	(1.d)	(2.c)	(2.d)	(3.c)	(3.d)
Any Purchase Delay	-0.016	-0.014	-0.029†	-0.020^	0.000	-0.014
	(0.013)	(0.011)	(0.013)	(0.010)	(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)	(0.038)
License/Permit Requirement	0.033	0.022	0.043	0.025	0.014	0.017
	(0.029)	(0.028)	(0.032)	(0.035)	(0.034)	(0.031)
Controls		X		X		X
Sample Size	1224	1224	1224	1224	1224	1224
R Squared	0.958	0.965	0.982	0.983	0.897	0.914

Notes: Each column block 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 columns 1.b, 1.d, 2b,2d etc. 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 7: 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: Geographical Variation in Purchase Delay Laws

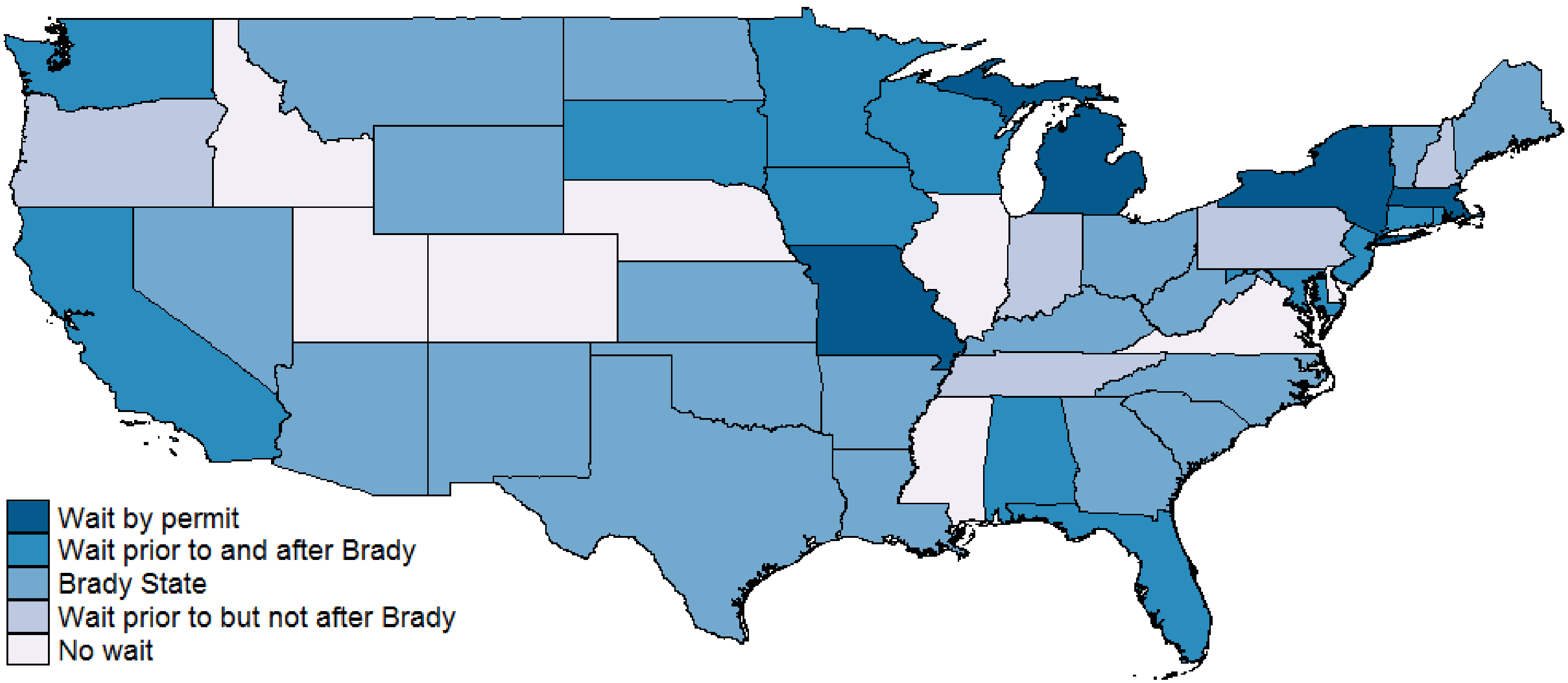
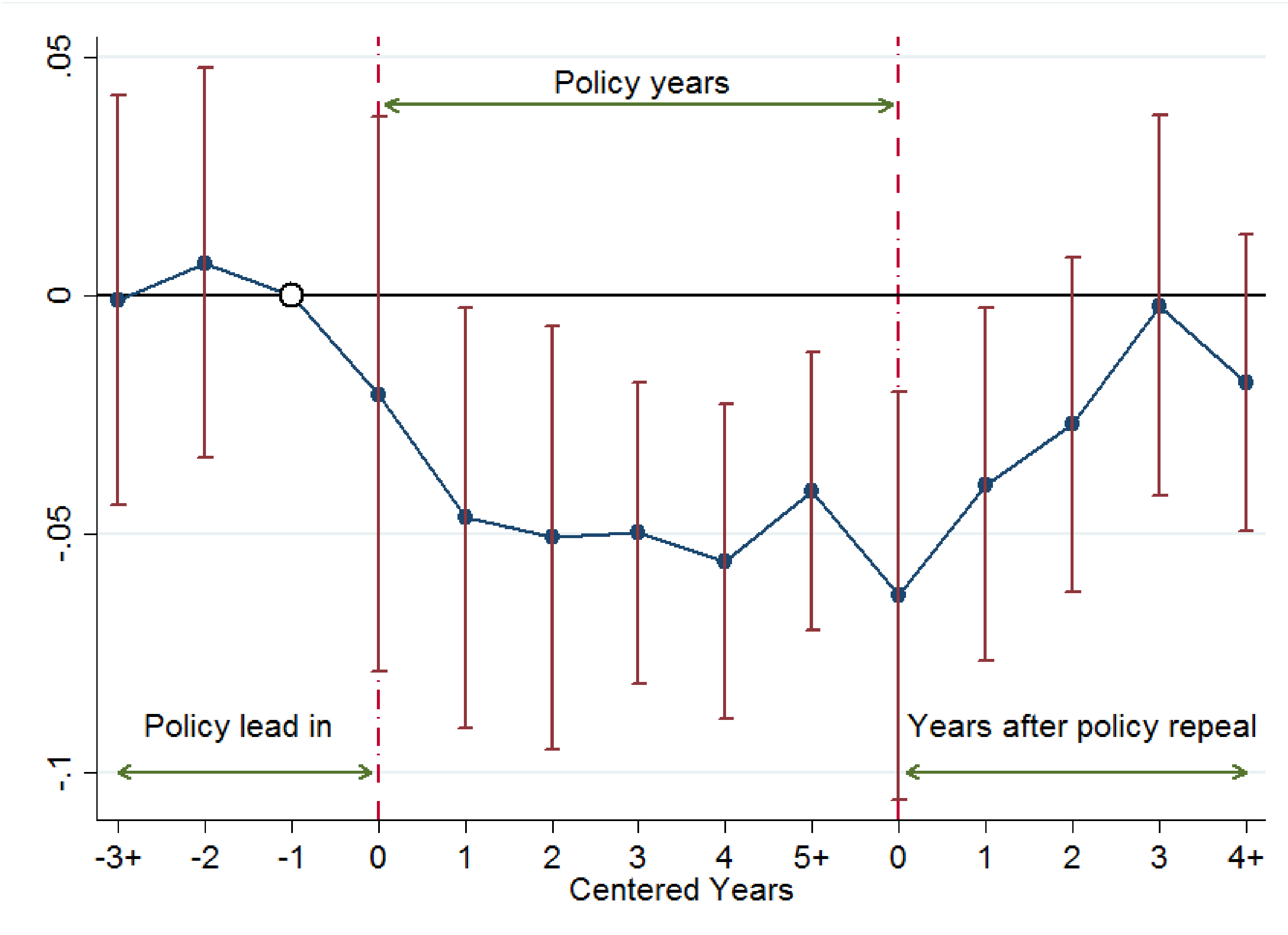
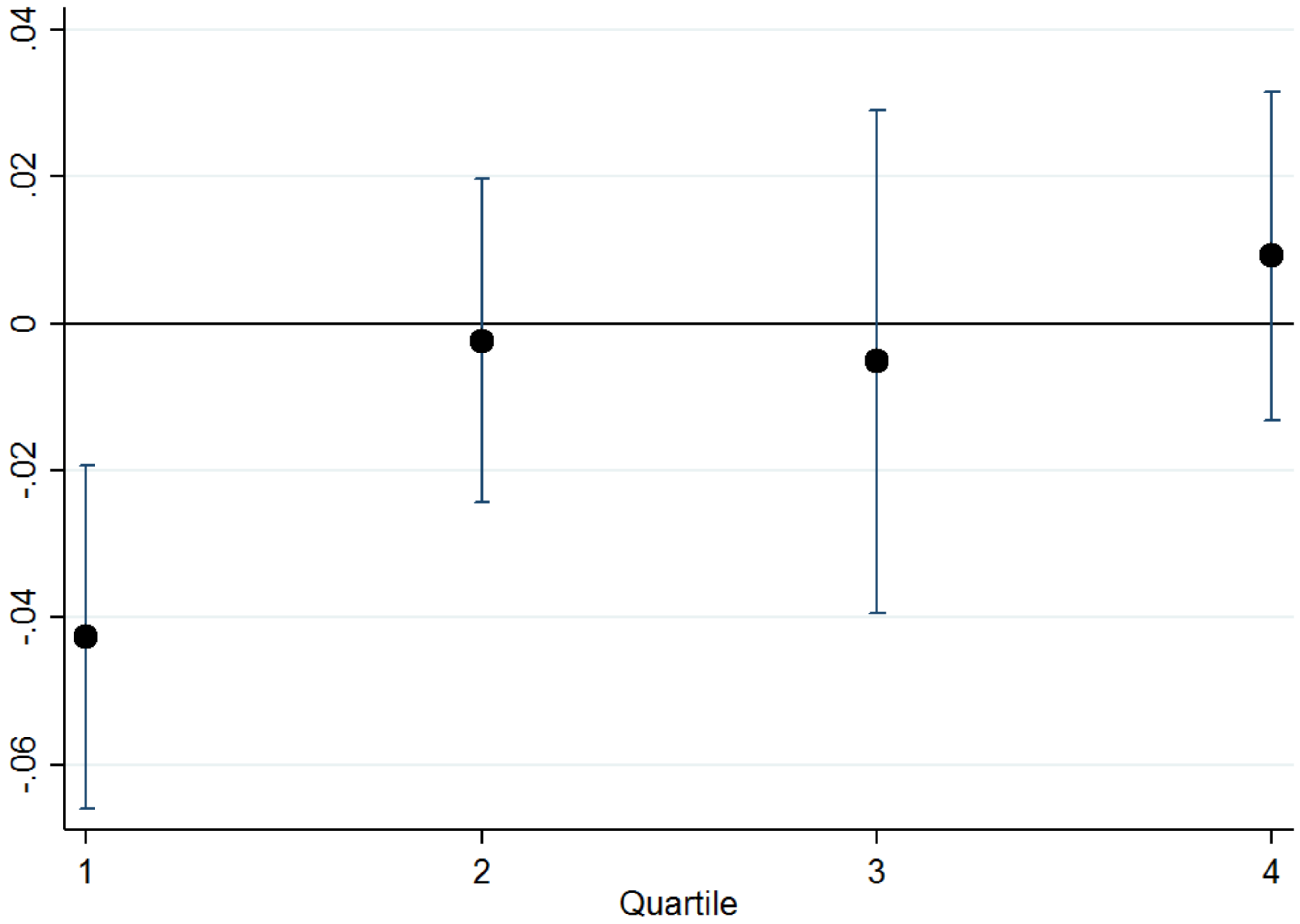


Figure 2: Event Study of Firearm Suicides



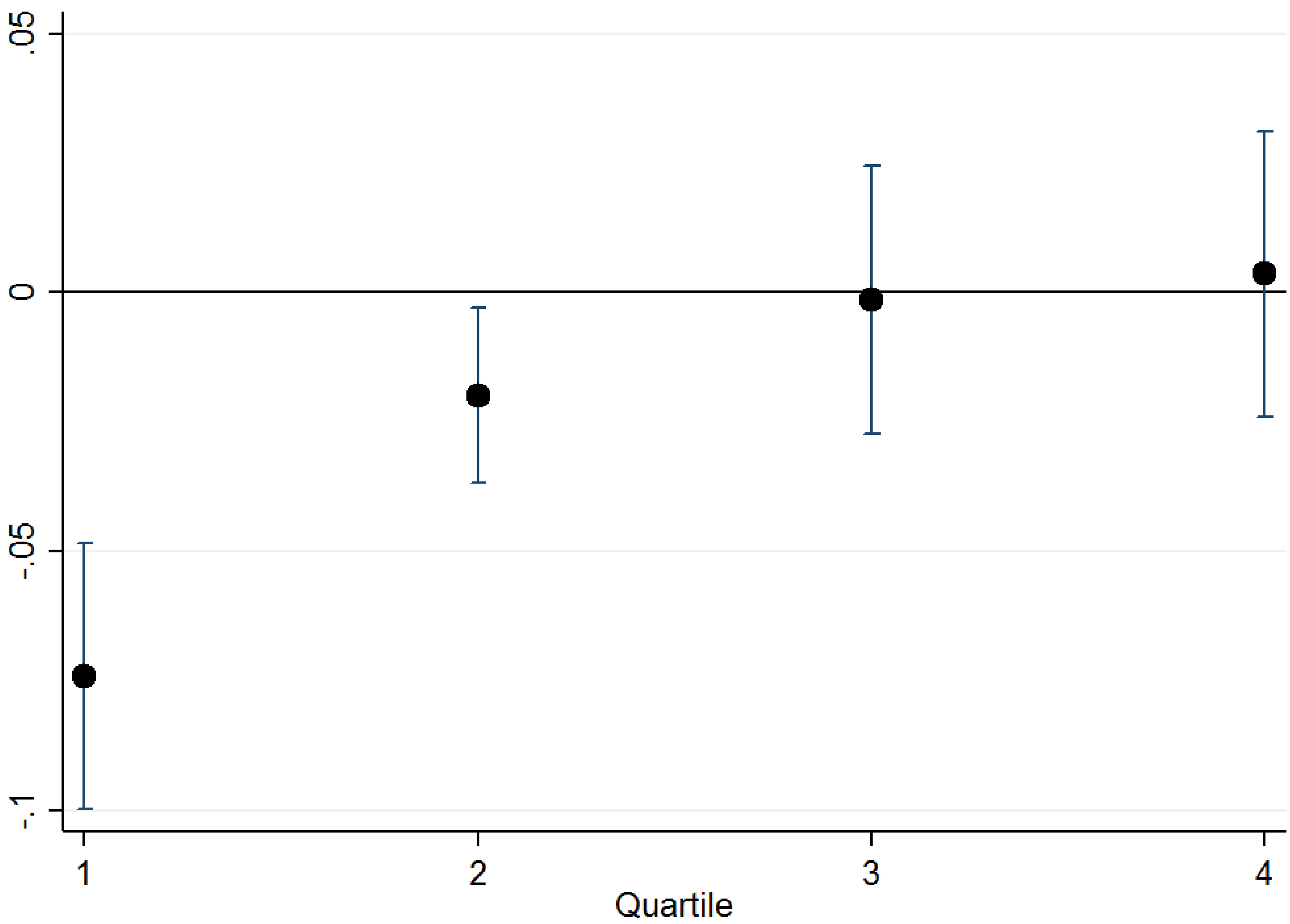
Notes: This figure show results from Table 7. The dots represent coefficients and the bands represent the 90% confidence interval.

Figure 3: Heterogeneous Purchase Delay Effect by Quartile of Mental Health Expenditures



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

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



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

Appendix

Table A1: Purchase Delays on Homicides using UCR Data

	1.Total Homicides		2.Firearm Homicides		3.Non Firearm Homicides	
	(1.a)	(1.b)	(2.a)	(2.b)	(3.a)	(3.b)
Any Purchase Delay	-0.080	-0.079	-0.122	-0.127	-0.058	-0.089
	(0.087)	(0.108)	(0.088)	(0.107)	(0.106)	(0.143)
Sample Size	1080	1080	1080	1080	1080	1080
R Squared	0.824	0.831	0.804	0.819	0.663	0.673
	(1.c)	(1.d)	(2.c)	(2.d)	(3.c)	(3.d)
Any Purchase Delay	-0.095	-0.095	-0.142	-0.152	-0.076	-0.108
	(0.094)	(0.120)	(0.096)	(0.119)	(0.115)	(0.158)
Long Wait (7+ Days)	0.177	0.138	0.222	0.193	0.170	0.061
	(0.144)	(0.135)	(0.194)	(0.161)	(0.195)	(0.163)
License/Permit Requirement	-0.046	0.014	-0.035	0.065	0.045	0.159
	(0.099)	(0.086)	(0.129)	(0.110)	(0.044)	(0.125)
Controls		X		X		X
Sample Size	1080	1080	1080	1080	1080	1080
R Squared	0.824	0.831	0.805	0.819	0.663	0.673

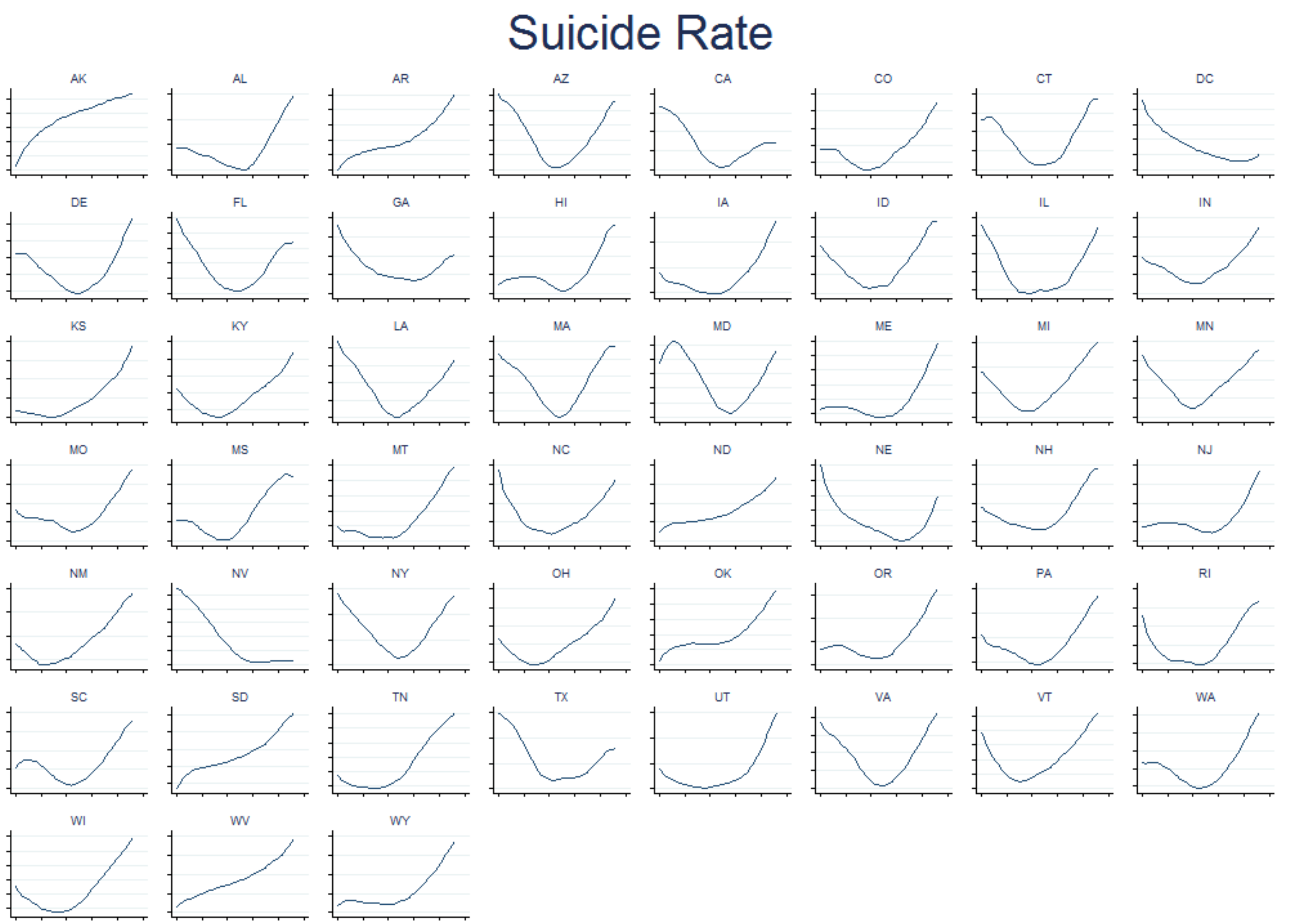
Notes: Each column block represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the various homicide rates and the standard errors are clustered at the state level. All specifications include state and year fixed effects and state specific time trends. The controls included in columns 1.b, 1.d, 2b,2d etc. are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: real mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, real income, ethanol consumption, urbanization, and infant mortality. ^ p<0.10 † p<0.05 ‡ p<0.01

Table A2: Purchase Delays on Intimate Partner Homicides

	1.Total Homicides		2.Firearm Homicides		3.Non Firearm Homicides	
	(1.a)	(1.b)	(2.a)	(2.b)	(3.a)	(3.b)
Any Purchase Delay	-0.075	-0.095	-0.069	-0.087	-0.053	-0.101
	(0.095)	(0.126)	(0.100)	(0.126)	(0.146)	(0.185)
Sample Size	1080	1080	1080	1080	1080	1080
R Squared	0.644	0.650	0.656	0.661	0.414	0.425
	(1.c)	(1.d)	(2.c)	(2.d)	(3.c)	(3.d)
Any Purchase Delay	-0.074	-0.093	-0.069	-0.085	-0.062	-0.113
	(0.103)	(0.136)	(0.108)	(0.135)	(0.154)	(0.202)
Long Wait (7+ Days)	-0.039	-0.092	-0.068	-0.107	0.077	-0.020
	(0.196)	(0.205)	(0.228)	(0.251)	(0.256)	(0.254)
License/Permit Requirement	0.043	0.102	0.141^	0.116	0.013	0.183
	(0.034)	(0.106)	(0.080)	(0.089)	(0.100)	(0.182)
Controls		X		X		X
Sample Size	1080	1080	1080	1080	1080	1080
R Squared	0.644	0.650	0.656	0.661	0.414	0.425

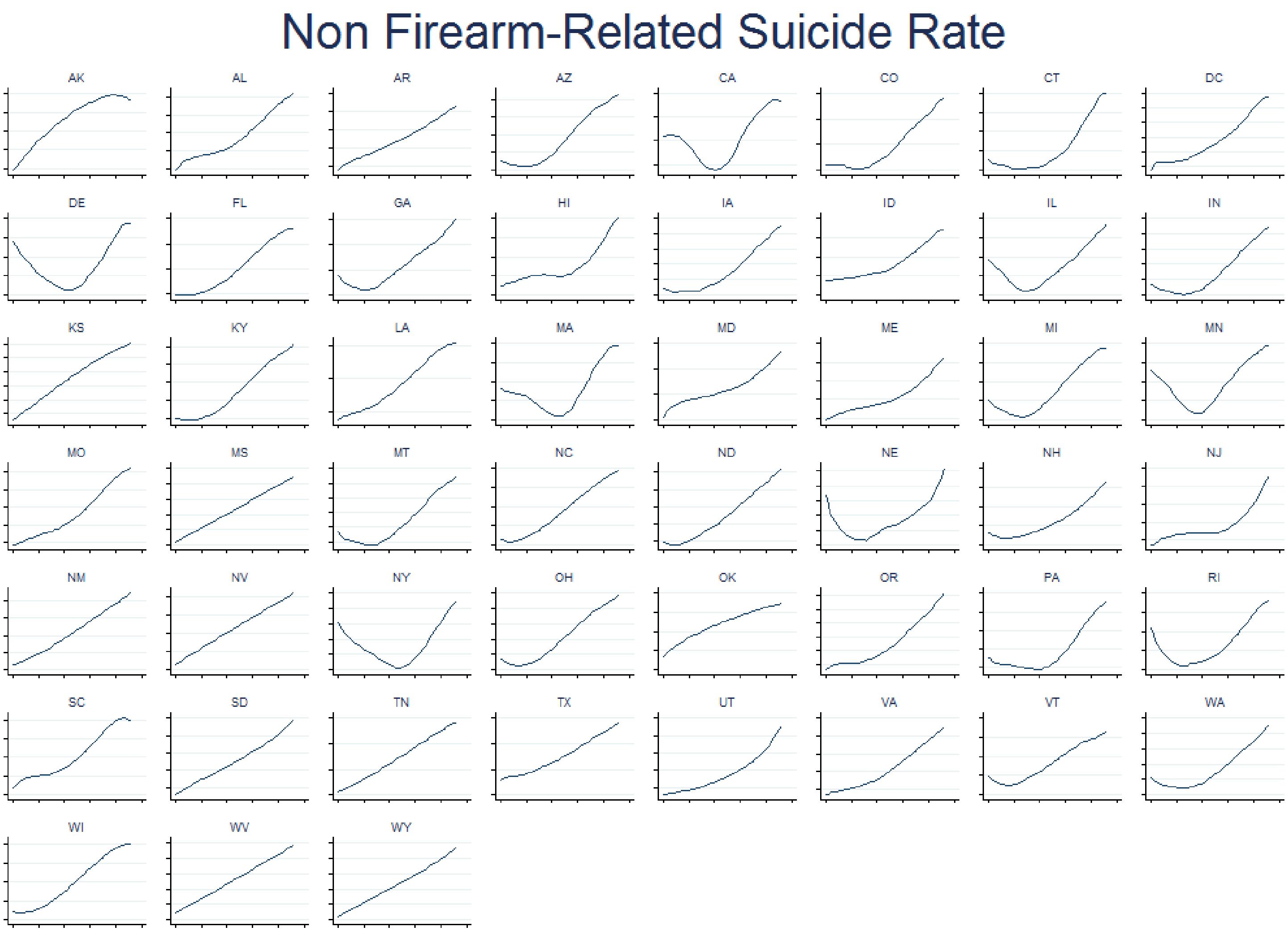
Notes: Each column block represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the various homicide rates and the standard errors are clustered at the state level. All specifications include state and year fixed effects and state specific time trends. The controls included in columns 1.b, 1.d, 2b,2d etc. are the percent of the state house and senate that are democrat, a Brady dummy, and the following rates: real mental health expenditures, accidental poisoning deaths, accidental firearm deaths, males between 45 and 64, black, unemployment, real income, ethanol consumption, urbanization, and infant mortality. ^ $p<0.10$ † $p<0.05$ ‡ $p<0.01$

Figure A1



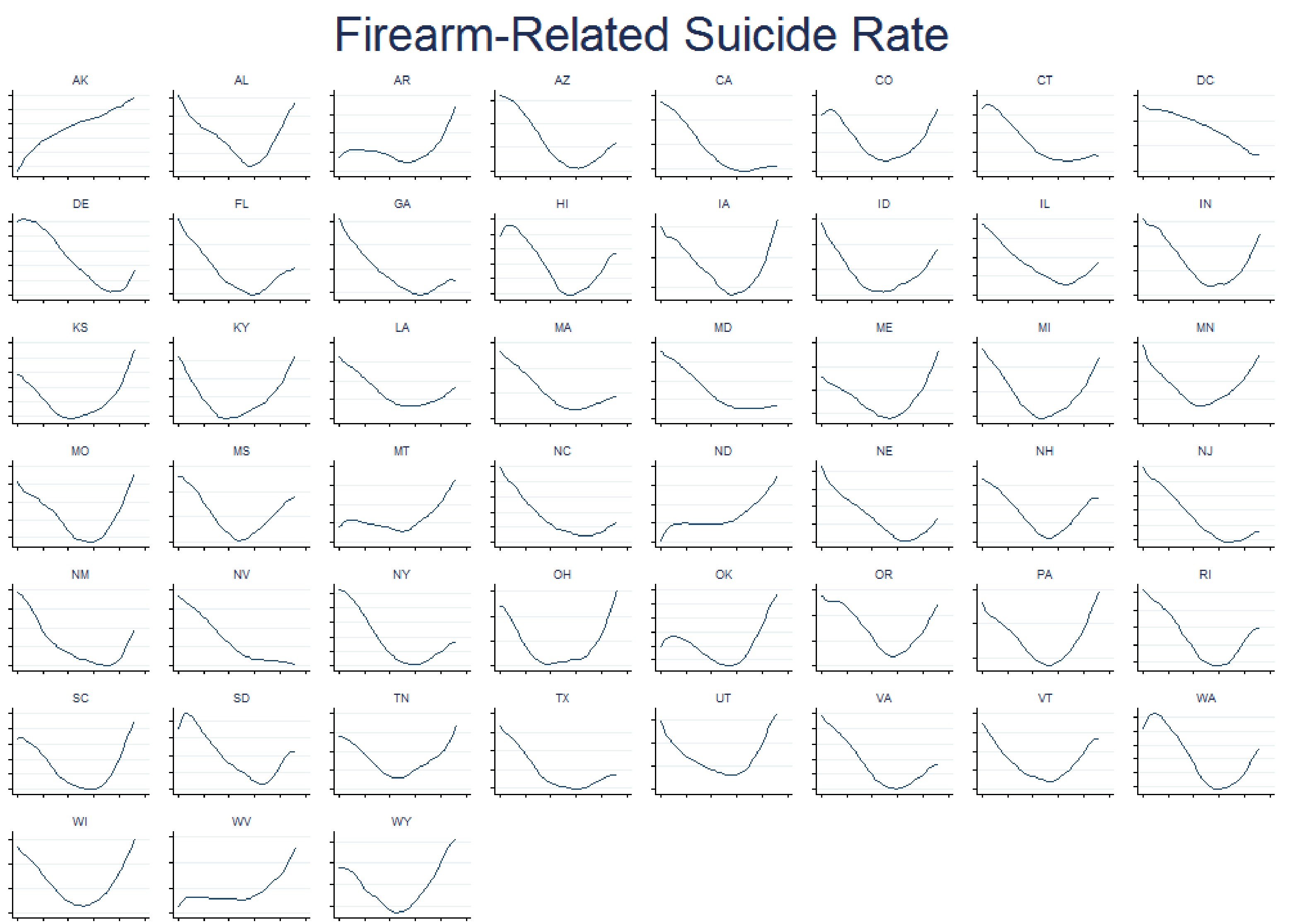
Notes: Each figure, A1-A4, represents a nonparametric estimate of each state's trend in suicide/homicide rates. The horizontal axis measures the un-centered year, and the vertical axis the associated rate.

Figure A2



Non Firearm-Related Suicide Rate
AK
AL
AR
AZ
CA
CO
CT
DC
DE
FL
GA
HI
IA
ID
IL
IN
KS
KY
LA
MA
MD
ME
MI
MN
MO
MS
MT
NC
ND
NE
NH
NJ
NM
NV
NY
OH
OK
OR
PA
RI
SC
SD
TN
TX
UT
VA
VT
WA
WI
WV
WY

Figure A3


Firearm-Related Suicide Rate
AK
AL
AR
AZ
CA
CO
CT
DC
DE
FL
GA
HI
IA
ID
IL
IN
KS
KY
LA
MA
MD
ME
MI
MN
MO
MS
MT
NC
ND
NE
NH
NJ
NM
NV
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OH
OK
OR
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RI
SC
SD
TN
TX
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VA
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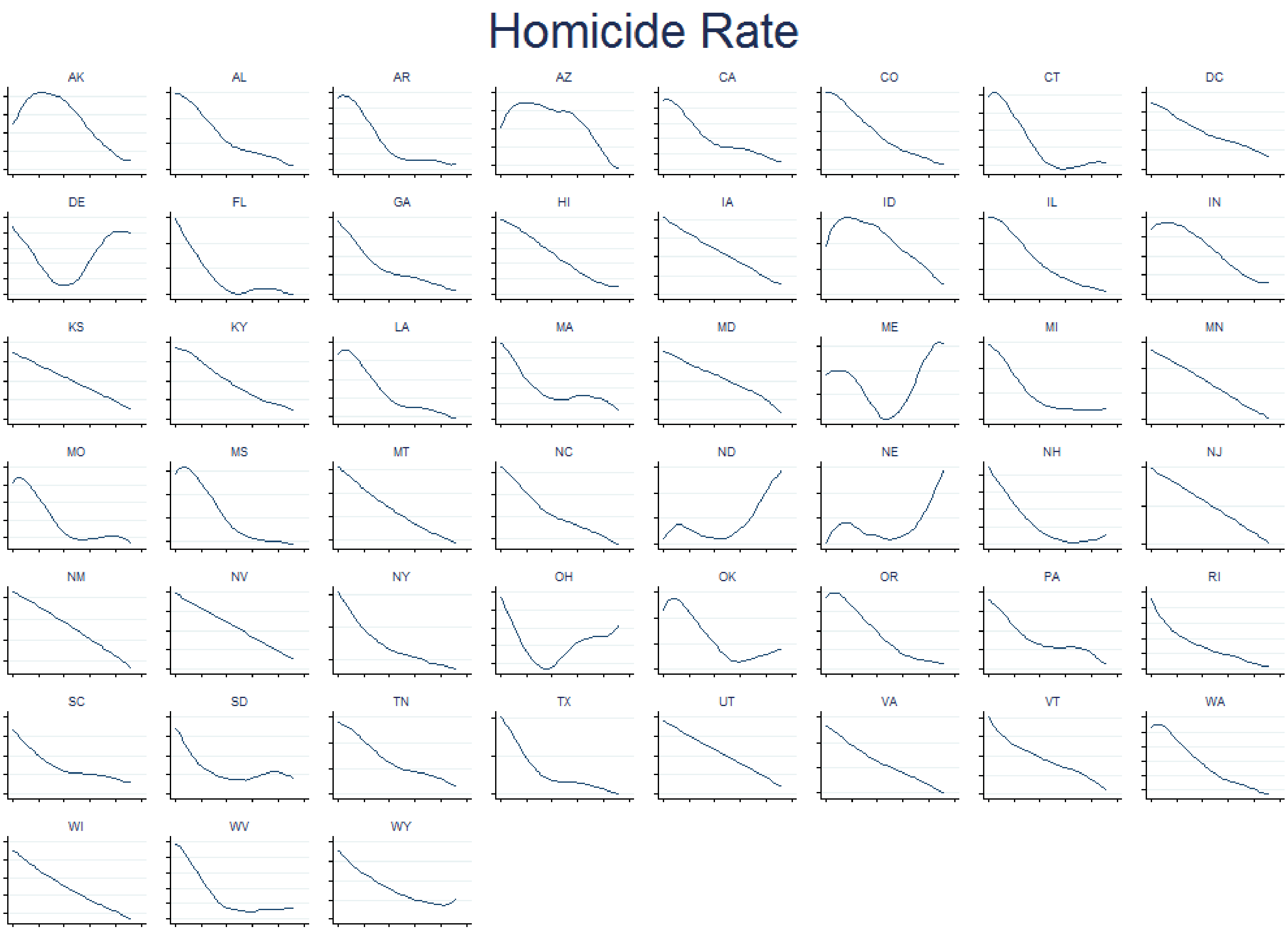
WA
WI
WV
WY

Figure A4


Homicide Rate
AK
AL
AR
AZ
CA
CO
CT
DC
DE
FL
GA
HI
IA
ID
IL
IN
KS
KY
LA
MA
MD
ME
MI
MN
MO
MS
MT
NC
ND
NE
NH
NJ
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NV
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OR
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WA
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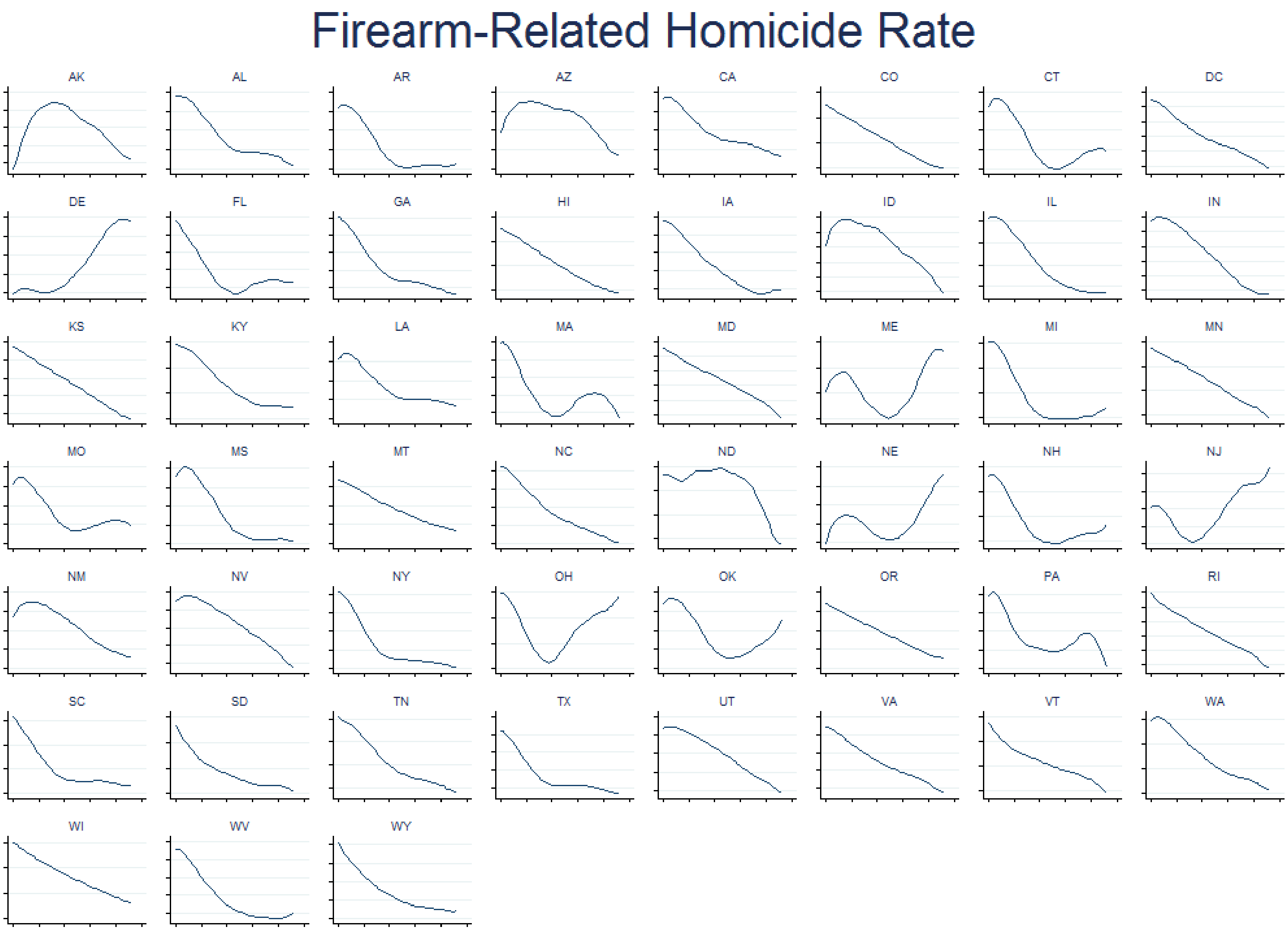
WV
WY

Figure A5


Firearm-Related Homicide Rate
AK
AL
AR
AZ
CA
CO
CT
DC
DE
FL
GA
HI
IA
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KS
KY
LA
MA
MD
ME
MI
MN
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NC
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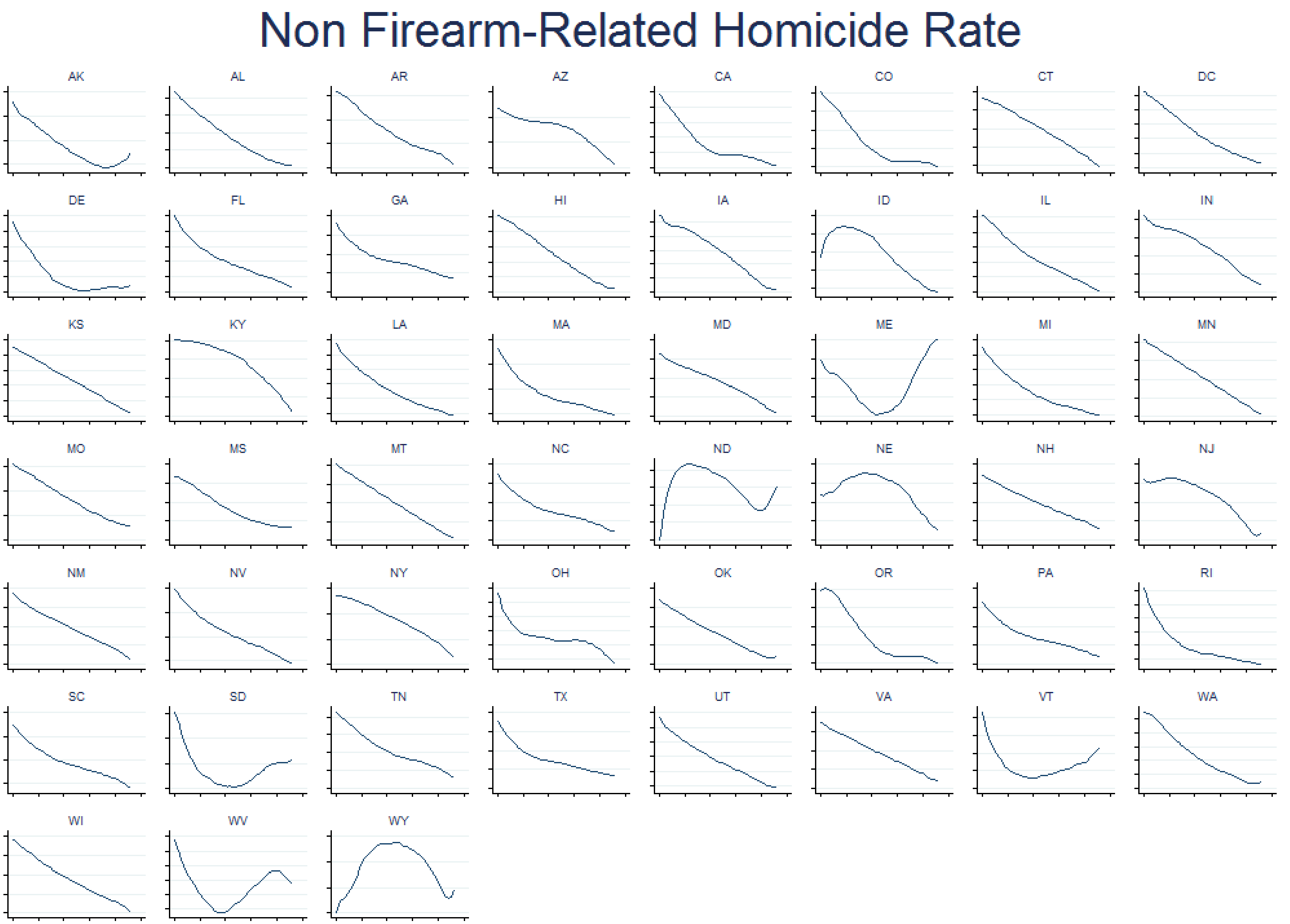
Figure A6


Non Firearm-Related Homicide Rate
AK
AL
AR
AZ
CA
CO
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DC
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FL
GA
HI
IA
ID
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IN
KS
KY
LA
MA
MD
ME
MI
MN
MO
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MT
NC
ND
NE
NH
NJ
NM
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NY
OH
OK
OR
PA
RI
SC
SD
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WA
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WV
WY