LOOKING DOWN THE BARREL OF A LOADED GUN: THE EFFECT OF MANDATORY HANDGUN PURCHASE DELAYS ON HOMICIDE AND SUICIDE

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We exploit within-state variation across time in both the existence and length of statutory delays – both explicit wait periods and delays created by licensing requirements – between the purchase and delivery of a firearm to examine the effect of purchase delays on homicides and suicides. We find that the existence of a purchase delay reduces firearm-related suicides by between 2% and 5% with no statistically significant increase in non-firearm suicides. Purchase delays are not associated with statistically significant changes in homicide rates.

The frequency of mass shootings in recent years has energised the long-standing and hotly debated topic of gun control in the US. 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 60 individuals per day die from firearm-related suicides.[2] To put this in perspective, self-inflicted gun shots kill more 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 third worst mass shooting.[3] Thus, evaluating ways to reduce these more common and costly sources of firearm-related deaths remain 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 characterised 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

[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 Las Vegas, NV shooting in 2017 claimed at least 58 lives, the Orlando, FL shooting in 2016 claimed 50 lives, and the Virginia Tech shooting in 2007 claimed 32 lives.

[4] See Manski and Pepper (2017) for an overview of the right to carry literature and challenges inherent the gun control literature.

Handgun Violence Prevention Act (Brady) implemented a temporary five-day waiting period on handgun purchases for federally licenced 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 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 causes 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% and 5%, and we find no statistically significant substitution towards 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 data and approach offer advantages over previous work in this area. First, some previous studies in the medical and public health literature examine changes in purchase delay laws within single states (Webster *et al.*, 2014; Rudolph *et al.*, 2015). Our differences-in-differences approach levies variation *within* states *across* time and is more resilient against threats to identification, such as political endogeneity. Second, we build upon the seminal work by Ludwig and Cook (2000). Ludwig and Cook (2000) examine the effect of Brady, which included both wait periods and background checks, and find some evidence that Brady reduced suicides and homicides among certain populations. Our longer panel and detailed legal survey of the purchase delays and other gun laws of each state allow us to isolate the effect of just wait periods independent of background checks and exploit state-level variation in gun laws leading into and out of Brady.

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

1. Background

1.1. *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

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[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 extend the wait period provision.

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 *et al.*, 2009; Ross *et al.*, 2012). Concern about unregulated private sales has led many policy makers to work at closing the 'gun show loophole'. However, Duggan *et al.* (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 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 (Lott, 1998; Duggan, 2001; Ayres and Iii, 2003; Moody and Marvell, 2005; Mocan and Tekin, 2006; Siegel *et al.*, 2013; Lang, 2016). 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 (Lang, 2013; Miller *et al.*, 2013; Phillips, 2013; Anglemyer *et al.*, 2014; Briggs and Tabarrok, 2014), there are three stylised 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 and Miller, 2012; Vriniotis *et al.*, 2015). Second, the decision to attempt suicide is, for at least some victims, often made within a few hours of suicide ideation (Peterson *et al.*, 1985; Miller *et al.*, 2012). Third, for many potential victims of suicide, suicidal thoughts are impulses that can be diverted and discouraged (Clarke and Mayhew, 1988; 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 and 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 *et al.*, 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 US on the efficacy of reducing the stock of guns in a population (Leigh and Neill, 2010; Lenis *et al.*, 2010). In the US, 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 US, 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 percentage 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 and Tabarrok, 2014), examining subscriptions to gun-related magazines (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, 2015).

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 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]

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[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.

[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 data set 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.

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, Webster *et al.* (2014), Rudolph *et al.* (2015), employed a short panel (Kleck and Patterson, 1993), or did not include information on wait period legislation (Cook *et al.*, 2009; Anestis *et al.*, 2015).

There are multiple dimensions of variation in states' purchase delay laws, and we track this variation over two decades. Prior to Brady, many states had some form of purchase delay such as a permit, licencing requirement or statutorily explicit wait period, and many of these states kept or reverted to their old policies after Brady expired. Additionally, there are differences with regard to the lengths of each purchase delay. This variation across states and time that happened prior to and continued after Brady provides a natural setup for a quasi-experimental study. In the subsection below, we detail the types of statutory sources of delay and their evolution since 1990.

1.2. *Statutory Sources of Delay*

Statutory delay of a handgun purchase falls into three broad categories. First, many states impose no delay. As of 2013, 32 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 licence, 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 realised delay create by a permit.[12] Given this, we include as part of the (2) specification 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 article 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

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[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 the sake of ease, we refer to all sort of document-based delay (permits, licences, certificates, etc.) as permits, recognising the differences in each.

[12] These caps ranged from three days in Nebraska (Neb. Rev. Stat. § 69-2405 (West 2015)) to six months in New York (N.Y. McKinney's Penal Law § 400.00 (2013)).

Table 1

State Handgun Delays Year of Change

State	Pre-1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002–9	2010–3	Permit year
Alabama							5				2		0		
Alaska		0					5					0			
Arizona		0					5					0			
Arkansas		0					5					0			
California					15							10			
Colorado		0					Exempt					0	0		Pre-1990
Connecticut					14						0				
Delaware		0					Exempt					0			Pre-1990
D.C.							2							10	Pre-1990
Florida		0					5					0			
Georgia		0					5					0			
Hawaii		10							14						
Idaho		0					Exempt					0			
Illinois							3								
Indiana					7							0			
Iowa							3								
Kansas		0					5					0			
Kentucky		0					5					0			
Louisiana		0					5					0			
Maine		0					5					0			
Maryland							7								1996
Massachusetts							Permit	Permit							Pre-1990
Michigan							Permit	Permit							Pre-1990
Minnesota							7	7							
Mississippi							5	Permit				0			Pre-1990
Missouri		0					Permit								
Montana	0	0					5					0			1991
Nebraska								Permit							
Nevada		0					5					0			
New Hampshire		0					Exempt					0			
New Jersey							7	7							Pre-1990
New Mexico			5				5					0			Pre-1990
New York		0					5	Permit							

Table 1
(*Continued*)

State	Pre-1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002–9	2010–3	Permit year
North Carolina							Permit								Pre-1990
North Dakota		0					5					0			
Ohio		0					5					0			
Oklahoma		0					5					0			
Oregon							15								
Pennsylvania	3	2					5					0			
Rhode Island								7							Pre-1990
South Carolina		0					5					0			
South Dakota		2					5					2		0	
Tennessee					15		5					0	0		
Texas							Exempt					0			
Utah		0					5					0			
Vermont		0					5					0			
Virginia		0					Exempt					0			
Washington							5	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 pre-existing background check systems. States labelled '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_{lt}. Dark shading identifies the states that contribute to the identifying variation of the long purchase delay.
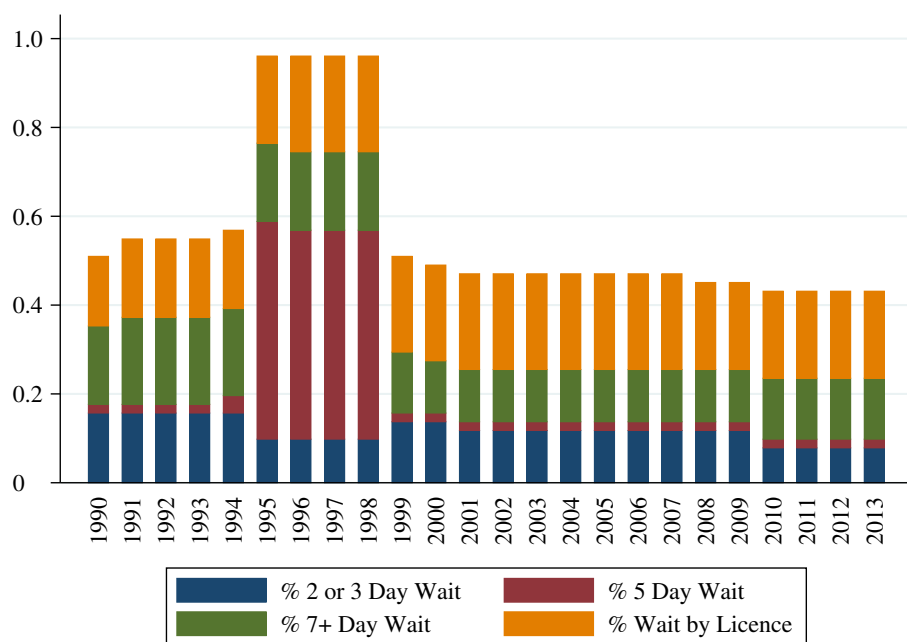
Fig. 1. *Timeline of Purchase Delays*

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 licence requirement. Colour figure can be viewed at wileyonlinelibrary.com.
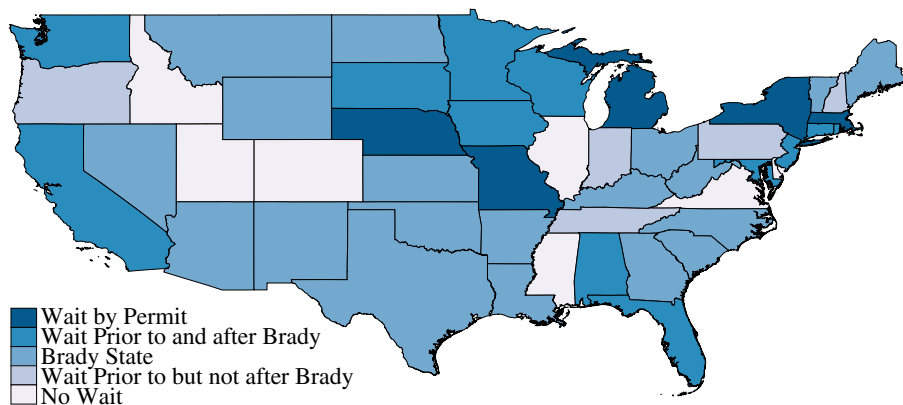
Fig. 2. *Geographical Variation in Purchase Delay Laws*

Note. Colour figure can be viewed at wileyonlinelibrary.com.

display the spatial variation of laws, and there is no apparent spatial correlation. We more formally test for political endogeneity in our Results and online Appendix sections.

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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 Hinckley'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, District of Columbia (DC), 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 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 existence and length of purchase delays in both their treatment and control states prevent Ludwig and Cook (2000) from isolating the effects of purchase delays on violent deaths. For instance, of the 32 treated states in Ludwig and Cook (2000), eight states had a mandatory wait period, two states had permit requirements prior to Brady, and the remaining 22 states had no purchase delay prior to Brady. Likewise, of the 19 control states, 13 had a mandatory wait period, three had permit requirements, and three had no purchase delay.[17] Thus, Ludwig and Cook's analysis primarily estimates the effects of criminal background check requirements in which purchase delays are a confounding factor.[18] On the contrary, our analysis specifically examines the effect of purchase delays on violent deaths while controlling for the presence of background checks.

[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 two days then eventually removed it all together while DC 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.
[17] Nevada, which Ludwig and Cook classify as a control state, actually had the five-day Brady wait period imposed because they lost their exempt status.
[18] Ludwig and Cook (2000) do not control for the presence of purchase delays prior to or during the Brady Act.

2. 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 Centre for Health Statistics,[19] we collect the number of firearm and non-firearm-related homicides and suicides between 1990 and 2013.[20]

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.[21] 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% 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% 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.[22]

As discussed previously, a good measure of the stock, flow or usage of firearms in the US 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

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[19] 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 online Appendix.

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

[21] The results presented here are generally insensitive to alternative coding of the laws such as counting the first partial year as receiving the treatment the entire year and characterisations of the first year as a proportion of the year treated (i.e. a law passed in June 2001 would be coded as 0.5 for 2001).

[22] 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.

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)
Licence 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 45–64	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 capita ethanol consumption	2.36	2.34	2.38
	(0.53)	(0.42)	(0.61)
Urbanisation 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

the infant mortality rate, the unemployment rate and real *per capita* income.[23] 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.

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

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 not just by mental health spending but also by the rollout of new psycho-pharmaceuticals and antidepressants (Marcotte and Markowitz, 2011). We attempt to capture these within state trends of prescription drug usage by including 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.

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.

3. Methods

We utilise 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:

$$\ln(s_{it}) = \alpha + \beta W_{it} + \theta \mathbf{X}_{it} + \gamma_t + \tau_i + \tau_i \times t + \varepsilon_{it}, \tag{1}$$

where $\ln(s_{it})$ is the natural log of the homicide or suicide rate, $\mathbf{X}$ 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 $\tau_i \times t$ 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.[24] Each regression is weighted by state population,[25] 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.

Mapping each state's raw trend in homicides and suicides suggest the need for state-specific trends. Those graphs are available in Figures 3 and 4. Based on the raw trends,

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[24] Additionally, the Box–Cox test of functional form for firearm suicides, our main outcome, yields a test statistic ($\theta = 0.35$) that suggests the log model as the most appropriate.

[25] 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.
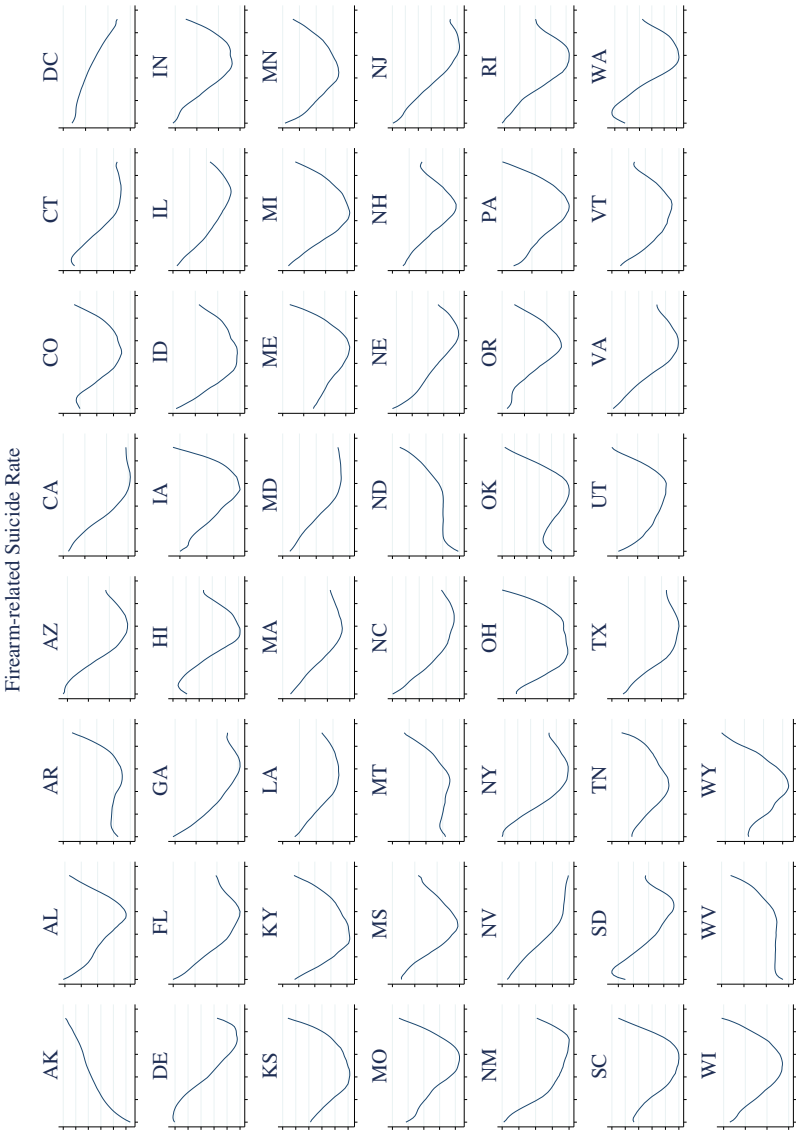
Fig. 3. *Trends in State Rates*

Notes. Each subfigure represents a non-parametric estimate of each state's trend in gun-related suicide rate. The horizontal axis measures the un-centred year, and the vertical axis the associated rate. Similar graphs for total/non-gun suicides are available in the online Appendix. Colour figure can be viewed at wileyonlinelibrary.com.
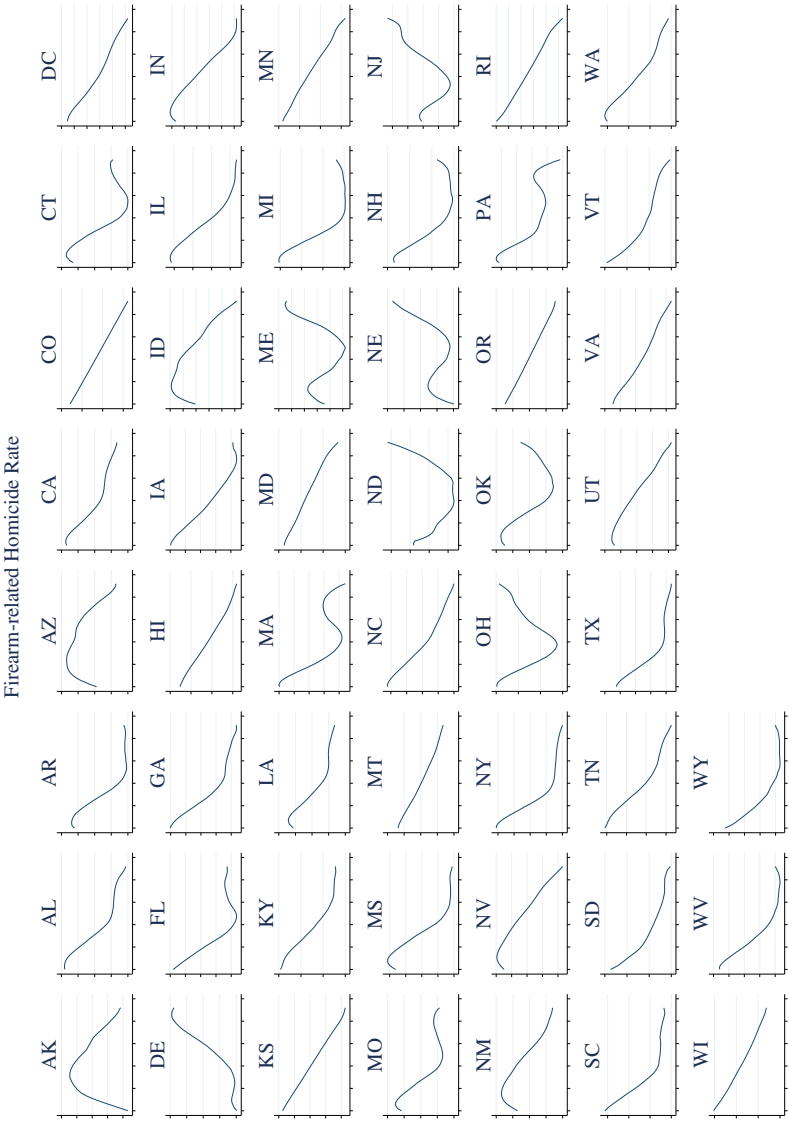
Fig. 4. *Trends in State Rates*

Notes. Each subfigure represents a non-parametric estimate of each state's trend in gun-related homicide rate. The horizontal axis measures the uncentred year, and the vertical axis the associated rate. Similar graphs for total/non-gun suicides are available in the online Appendix. Colour figure can be viewed at wileyonlinelibrary.com.

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. As seen in the Results Section, our main result that purchase delays decrease gun-related suicides is unaffected by the inclusion/exclusion of state-specific time trends. However, some of our null findings, including the homicide regressions and the non-firearm suicide regressions, have some sensitivity to state-specific time trends. Given the raw trends in Figures 3 and 4 and the fact that inclusion of state-specific time trends is generally a more conservative modelling approach, we prefer the specifications that include state-specific time trends.

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 (two, three, five, seven, 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 (1) to measure whether the length of the statutory delay affects the policy effectiveness with the following specification:

$$\ln(s_{it}) = \alpha + \beta W_{it} + \delta L_{it} + \theta X_{it} + \gamma_t + \tau_i + \tau_i \times t + \varepsilon_{it}, \tag{2}$$

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 (1) including a 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.[26] 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 one to three, five and seven+ 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 (1):

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

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

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[26] 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.

than equal to five 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 *et al.*, 2013). All other notations remain the same from the previous equations.

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 online 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 seem to be unrelated to trends in the outcome.

4. Results

Our main results are reported for homicides in Table 3 and suicides in Tables 4 and 5. Tables 3 and 4 are organised such that each column represents a unique regression.

Table 3

Purchase Delays on Homicides

	(1)	(2)	(3)	(4)
Total homicides				
Any purchase delay	−0.118**	−0.037	−0.022	−0.025
	(0.04)	(0.044)	(0.035)	(0.037)
Long wait (7+ days)				0.054
				(0.046)
R^2	0.897	0.937	0.942	0.942
Firearm homicides				
Any purchase delay	−0.150**	−0.066	−0.051	−0.061*
	(0.055)	(0.047)	(0.033)	(0.034)
Long wait (7+ days)				0.123
				(0.081)
R^2	0.901	0.947	0.955	0.956
Non-firearm homicides				
Any purchase delay	−0.055*	−0.001	0.014	0.019
	(0.028)	(0.039)	(0.038)	(0.04)
Long wait (7+ days)				−0.05
				(0.077)
R^2	0.797	0.829	0.835	0.835
Controls			X	X
SS time trends		X	X	X
Sample size	1,224	1,224	1,224	1,224

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. The controls included in corresponding 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, urbanisations and infant mortality. Time trends are linear. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 4

Purchase Delays on Suicides

	(1)	(2)	(3)	(4)
Total suicides				
Any purchase delay	−0.049**	−0.019	−0.016	−0.014
	(0.019)	(0.013)	(0.010)	(0.011)
Long wait (7+ days)				−0.045
				(0.027)
R^2	0.935	0.958	0.965	0.965
Firearm suicides				
Any purchase delay	−0.091***	−0.031**	−0.022**	−0.020*
	(0.027)	(0.013)	(0.011)	(0.010)
Long wait (7+ days)				−0.041
				(0.030)
R^2	0.968	0.982	0.983	0.983
Non-firearm suicides				
Any purchase delay	−0.059**	−0.004	−0.017	−0.014
	(0.027)	(0.018)	(0.016)	(0.016)
Long wait (7+ days)				−0.05
				(0.039)
R^2	0.866	0.897	0.914	0.914
Controls			X	X
SS time trends		X	X	X
Sample size	1,224	1,224	1,224	1,224

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. The controls included in corresponding 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, urbanisations and infant mortality. Time trends are quadratic. *p < 0.10, **p < 0.05, ***p < 0.01.

The estimation of (2) for each outcome is reported in column (4) and the estimation of (1) 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'.[27]

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% in our preferred specification

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[27] Available in online Appendix.

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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	1,224		
R^2	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 includes state and year fixed effects and state specific time trends. The controls included are the percentage 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, urbanisations and infant mortality. $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

in column (4). One concern may be a substitution effect between firearm suicides and non-firearm suicides (Briggs and Tabarrok, 2014). That is, discouraging firearm suicides may actually just encourage suicides by other means. The bottom panel of Table 4 examines non-firearm-related suicides, and we find little evidence of a 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, little evidence to suggest that any 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 5, we analyse a dynamic event study model of purchase delays as described in (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.[28] The event study suggests that purchase delays reduce gun-related suicides by approximately 5%

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[28] 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.
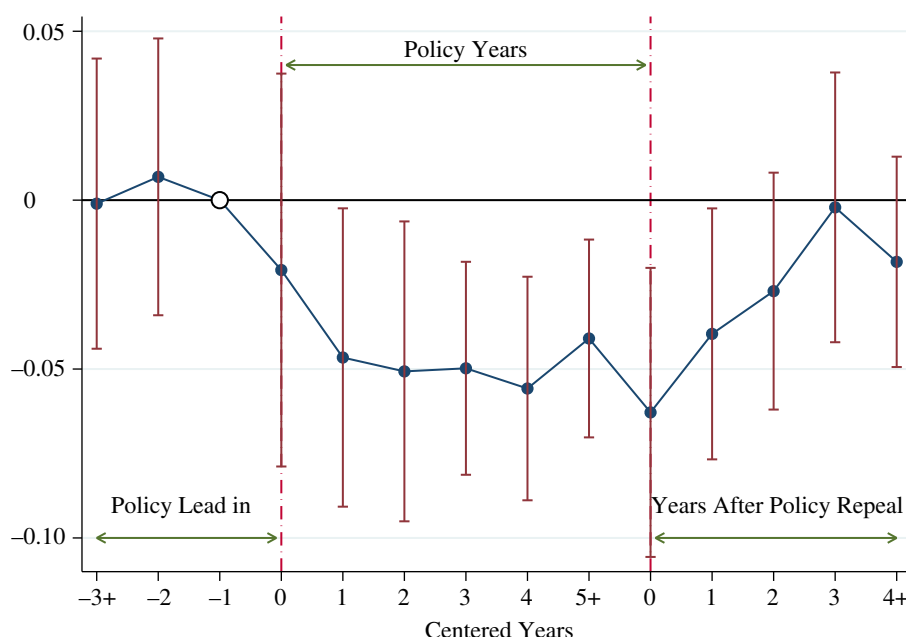
Fig. 5. *Event Study of Firearm Suicides*
Notes. This figure show results from Table 5. The dots represent coefficients and the bands represent the 90% confidence interval. Colour figure can be viewed at wileyonlinelibrary.com.

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.

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 should not 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 (Webster *et al.*, 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.[29] 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 online 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

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[29] 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.
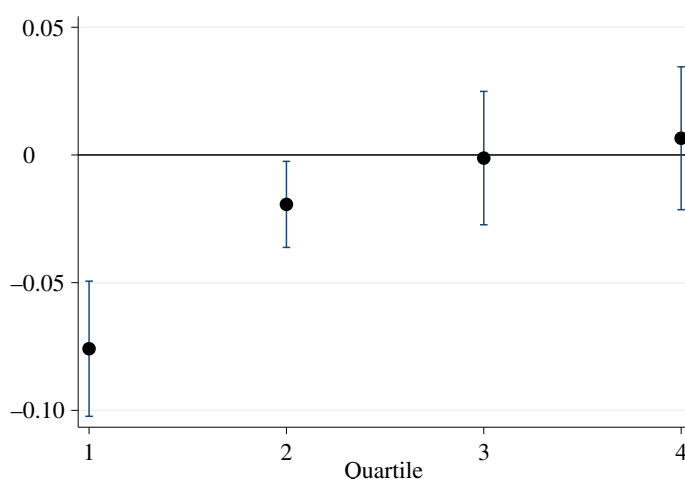
Fig. 6. *Heterogeneous Purchase Delay Effect by Quartile of Background Checks*
Notes. Bands represent 90% confidence intervals. Associated tables are available upon request.
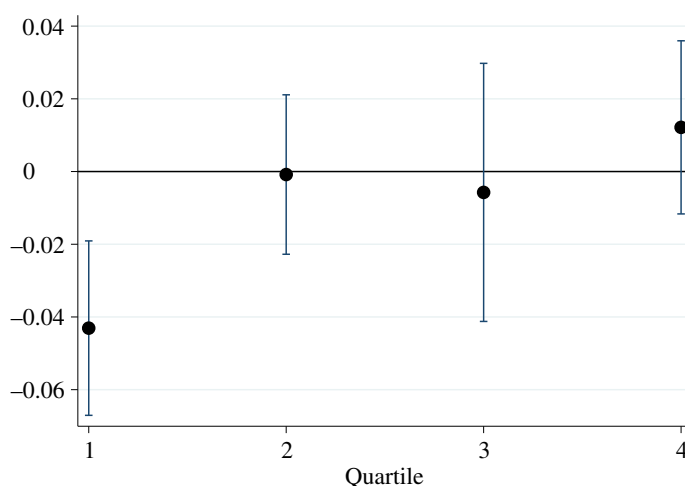Colour figure can be viewed at wileyonlinelibrary.com.

Fig. 7. *Heterogeneous Purchase Delay Effect by Quartile of Mental Health Expenditures*
Notes. Bands represent 90% confidence intervals. Associated tables are available upon request.
Colour figure can be viewed at wileyonlinelibrary.com.

policy variable in (1) and (2). Graphically displayed in Figure 6, 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 7. A further discussion of these results is available in the online Appendix.

5. Discussion

In this article, 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% to 5%.[30] This result is both statistically and substantively significant. It suggests that if all 32 states currently without a mandatory purchase delay were to adopt one, then it would save more than 600 lives per year.[31] 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% 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%. It is important to remember, though, that this article surveyed single site studies where the intervention was highly localised 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 studies surveyed likely totals less than 100.[32] Furthermore, we are only able to estimate the effect of handgun purchase delays as a suicide intervention 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% 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% of all suicides committed by those individuals affected by the policy.[33] Under these

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[30] The 2% 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.

[31] This assumes a 5% decline from the combined 12,914 firearm-related suicides in these 32 states in 2013.

[32] As an example, one study might find a change from eight suicides a year to four 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 four.

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

assumptions, purchase delays have a similar efficacy to other suicide intervention methods.

While we find little evidence to suggest that especially long waits have a larger effect relative to any wait, given the limitations of the data, our results do not exclusively advocate for only short waits. Short waits are, however, an attractive policy option since they are probably more politically obtainable relative to longer waits, and a body of literature exists that suggests they still may have the desired effect of discouraging some 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 (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, 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% (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 polarising 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 depolarising 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 minimised by not imposing excessively long delays.

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Additional Supporting Information may be found in the online version of this article:

Appendix A. Web Appendix.
Data S1.

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