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Discussion paper

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Understanding Society

Impulse Purchases, Gun Ownership and Homicides: Evidence from a Firearm Demand Shock*

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Abstract

Do firearm purchase delay laws reduce aggregate homicide levels? Using quasi-experimental evidence from a 6-month countrywide gun demand shock starting in late 2012, we show that U.S. states with legislation preventing immediate handgun purchases experienced smaller increases in handgun sales. Our findings are hard to reconcile with entirely rational consumers, but suggest that gun buyers behave time-inconsistently. In a second step, we demonstrate that states with purchase delays also witnessed 3% lower homicide rates during the same period compared to states allowing instant handgun access. We report suggestive evidence that lower handgun sales primarily reduced impulsive assaults and domestic violence.

JEL codes: K42, H76, H10, K14

Keywords: Guns, murder, Sandy Hook, gun control, impulsiveness

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1 Introduction

The relationship between firearm ownership and criminal activity has been one of the most polarizing topics in U.S. politics over the past decades. Supporters of *gun rights* often claim that arming citizens will lead to decreases in crime, while supporters of *gun control* point to the high numbers of victims from gun-related violence. Fowler et al. (2015) report that 32,000 Americans are killed and another 67,000 injured by firearms every year. Based on their calculations, any policy measure effectively reducing these numbers would thus have the potential for welfare gains of almost $50 billion each year. Curbing gun violence was also the intention behind many of the 130 gun control policy measures which have been enacted so far across U.S. states (Siegel et al., 2017).

One group of such policy measures, specifically targeted at preventing impulsive acts of gun violence, are *firearm purchase delay laws*. These measures, by now in place in 14 U.S. states, create a temporal distance between the decision to buy a gun and its eventual receipt.[1] Purchase delays can work *directly* through mandatory waiting periods or *indirectly* through time-consuming bureaucratic hurdles such as mandatory purchasing permits. Both of these measures provide gun buyers with a "cooling-off period" during which those with transient violent intentions may reconsider their planned actions (Cook, 1978; Andrés and Hempstead, 2011). While the life-saving potential for gun buyers with suicidal or homicidal intentions appears straightforward, little is known as to whether these measures also affect the behavior of law-abiding consumers without such transient violent motives at the time of purchase.

This paper investigates the effects of handgun purchase delay laws in the wake of an aggregate shock to firearm demand. In a first step, we show that the existence of purchase delays led to a relative reduction in handgun sales during the six months after the 2012 Presidential election and the shooting at Sandy Hook Elementary School. During this period, fear of more restrictive gun control legislation and higher perceived need of self-defense capabilities led to record sales of firearms across the entire United States (Vox, 2016; CNBC, 2012). We use a difference-in-differences (DiD) framework, comparing monthly handgun sale background checks in states with handgun purchase delays to states without such delays during the six-month window of increased firearm demand. Our baseline results indicate that states with purchase delay laws witnessed a

[1] These delays vary from as short as 2 days to as long as 6 months. Details can be found in Section 2.1.

relative 14% *decrease* in handgun sales. These findings hold across several specifications and survive numerous robustness checks, effectively showing that the effect is particular to the time period we study.

One potential challenge to our identification strategy could be that asymmetric changes in the attractiveness of firearms (potentially due to different preferences for gun ownership) between states were causing the diverging patterns of handgun purchases. Utilizing Google search data, we do not find evidence for an association between delay laws and comparatively lower public interest for buying firearms during the demand shock period. Handgun purchase laws thus do not seem to affect consumer *interest* for firearms but only whether this translated into actual *purchases*. Furthermore, we investigate whether supply shortages in states with purchase delays may have pushed consumers into less regulated, secondary markets (i.e. gun shows instead of licensed gun dealers). Such a scenario would be particularly problematic if sales in non-regulated markets had an independent effect on violent crime. Using Google search data, we fail to find strong evidence that demand for gun shows tilted towards any group of states.

In order to rationalize why delay laws differentially affect handgun sales, and to explain observed patterns in the data, we sketch a simple model of firearm purchases. The basic framework builds on previous work by Conlin, O'Donoghue, and Vogelsang (2007) for consumer choice under projection bias. We extend their model to include present bias as a second potential source of time inconsistency. This extended model generates two important predictions: First, independent of whether consumers behave time-consistently or not, they will always have a lower propensity to purchase a handgun in states with delay laws. This difference in sales is predicted to increase during a demand shift, a prediction in line with the overall effect we observe. Second, even if delays are very short, we expect to observe a pronounced sales gap during the demand shock, but only if consumers behave in a time-inconsistent fashion. Using variation in the length of the delays, as well as through visual inspection of the data, we are able to provide empirical support that time inconsistency, rather than fully time-consistent behavior, is the more likely mechanism behind our findings.

In the second part of our analysis, we then exploit the detected temporary differences in handgun sales as a novel way of identifying the relationship between gun ownership and homicides. Using the same DiD framework, we find that counties in states imposing purchasing delays experience a relative 3% *decrease* in overall homicide rates during the demand spike, which is entirely driven by homicides involving firearms. Our baseline estimate implies that about 280 lives could have been saved in the six-month period alone if handgun purchase delays had been in place in all U.S. states. An extensive set of robustness checks shows that these results are specific to the period of the demand hike, invariant to various trend specifications, and not driven by single states or choice of the sample.

Having established the robustness of our baseline findings, we look into the circumstances and demographics of the additional homicides in states without handgun purchase delays.[2] Since time-inconsistent behavior was the more likely driver behind handgun purchases during the demand shock, we would expect to also see more impulsive homicides if time inconsistency was linked to impulsive behavior in general. We find that the additional victims are more likely to be middle-aged. This is noteworthy since those demographic groups are usually less likely to die from gun-related homicides which in turn may indicate that delay laws avoided firearms ending up in the hands of particularly unlikely offenders. For females, the evidence points towards instances of domestic violence, as the majority of additional female homicides occur inside the victim's home and arise from an argument. The affected killings of males occur mainly outside of their home but are similarly strongly related to arguments. Taken together, the results suggest that handgun purchase delay laws can be an effective measure to prevent impulsive homicides as they reduce the probability of arguments to turn lethal. One possible explanation could be that delay laws prevent handgun purchases by time-inconsistent consumers who may have a higher inclination towards impulsive violence.

This study is related to three important streams of research. First, our evaluation of gun purchase delay laws contributes to the growing literature analyzing the role of behavioral biases in designing public policies (overviews are provided in Chetty, 2015; Bernheim and Taubinsky, 2018). Within the field of behavioral economics, our theoretical framework is furthermore linked to studies of time-inconsistent decision making, in particular O'Donoghue and Rabin (2001) and Conlin, O'Donoghue, and Vogelsang (2007). To the best of our knowledge, we are the first study to investigate behavioral biases in the context of gun ownership. We also relate to studies at the intersection between behavioral economics and economics of crime linking impulsiveness

[2] All statements regarding a relative *increase* in handgun sales and homicides in states without handgun purchase delays are just the flip side of the relative *decrease* in handgun sales and homicides in states with such delays.

with criminal activity and violent behavior. Dahl and DellaVigna (2009) investigate the effect of movie violence on violent crimes and find that attendance of movies serves as a substitute for violent behavior. Card and Dahl (2011) find that unexpected losses of the home football team increase instances of domestic violence. We complement this literature with the first study to establish a link between firearm availability and fatal consequences of impulsive behavior.

The second line of related research is the large literature on the relationship between firearm ownership and violent crime in economics, criminology and public health.[3] A majority of studies finds a positive relationship (see e.g. Cook and Ludwig, 2006; Duggan, 2001; Miller, Azrael, and Hemenway, 2002; Miller, Hemenway, and Azrael, 2007; Siegel, Ross, and King, 2013). Some studies, however, also report no effect (Duggan, Hjalmarsson, and Jacob, 2011; Moody and Marvell, 2005; Kovandzic, Schaffer, and Kleck, 2013; Lang, 2016). In order to move beyond mere correlations, the literature has increasingly relied on legislative changes as a way to establish causality. Lott and Mustard (1997) found negative effects of *Concealed Carry Weapon* (CCW) laws on crime rates which, however, could not be confirmed in follow-up work (Donohue, Aneja, and Weber, 2017; Ayres and Donohue, 2003; Duggan, 2001; Manski and Pepper, 2018). Fleegler et al. (2013), on the other hand, show that the number of state firearm laws is negatively correlated with gun-related deaths.

Several studies within this literature have also looked at externalities from gun legislation. Knight (2013), for instance, shows that firearms flow from states with lenient gun laws into states with stricter legislation. Dube, Dube, and García-Ponce (2013) and Chicoine (2016) find that the expiration of the Federal Assault Weapons Ban significantly increased violent crimes in Mexican municipalities. While these studies focus on externalities across *space*, our study presents an analysis of an externality across *groups*. Although providing a "cooling-off period" to gun buyers with transient violent intentions, handgun purchasing delay laws should not affect regular consumers' carefully made purchasing decisions. We contribute to this literature by providing suggestive evidence that delay laws can in fact also reduce firearm homicides through deterring gun purchases by individuals whose general inclination towards impulsive behavior would translate into violent behavior at a later point in time.

[3] Due to space constraints we confine ourselves to the most relevant literature. An excellent survey discussing in particular the early contributions is provided by Hepburn and Hemenway (2004), newer contributions are discussed by Kleck (2015).

Some papers have also looked specifically at the impact of purchase delays on crime rates. Ludwig and Cook (2000), for example, study the effects of introducing waiting periods through the Brady Act and find no clear-cut evidence that these had an impact on violent crime. The introduction of Connecticut's mandatory pistol purchasing permit in 1995 is analyzed in Rudolph et al. (2015) who find a strong relative decrease in homicide rates. Edwards et al. (2017) look at all delay laws since the 1990s and find negative effects on yearly rates of gun-related suicides, but not on homicides. The study by Luca, Malhotra, and Poliquin (2017) starts in the 1970s and jointly evaluates the introduction of waiting periods and the NICS background check system. Their results indicate that delay laws yield a 17% reduction in homicide rates. As the adoption of firearm purchase delay laws may not be exogenous, our paper substantially advances this part of the literature by providing novel and credible identification through exploiting a demand shock in conjunction with pre-existing delay laws.

Third, our work relates to the recent literature concerned with the impact of increased gun control debates after mass shootings. Luca, Malhotra, and Poliquin (2016) find that shootings generally increase the introduction and passage of gun-related bills in the state where they take place depending on the current majority party. Similarly, Yousaf (2017) shows that shootings increase the importance of gun policy in U.S. elections and particularly hurt votes for Republicans. Levine and McKnight (2017) also focus particularly on the Sandy Hook shooting and study how elevated gun exposure translated into higher rates of firearm-related accidents. Their identification strategy, however, uses vote shares for President Obama in 2012 as an instrument for diminished reactions in gun exposure. This approach may not satisfy the required exclusion restriction that correlates of voting behavior, such as education, are orthogonal to accidental firearm deaths. We add to this line of research by using an identification design that relies on frictions in the purchasing process and that is robust to a careful assessment of the identifying assumptions. Our findings contrast Levine and McKnight (2017) by showing that the primary detrimental effect of increased gun ownership after the Sandy Hook shooting was actually an increase in gun-related *homicides*.

This paper is organized as follows: Section 2 provides relevant background information regarding U.S. gun laws and the gun demand shock we consider, as well as a description of our theoretical model. Sections 3 and 4 introduce the data and empirical strategy used in this paper, respectively. Our first set of results on handgun sales are presented in Section 5. The discussion of delay laws' effects on homicide rates and their circumstances follow in Section 6. Section 7 concludes.

2 Background and Theoretical Motivation

2.1 Background: Gun Laws in the United States

The Second Amendment to the United States Constitution protects the fundamental right of citizens to keep and bear arms. The federal government, as well as state and local governments, however, have in the past enacted laws that make it harder or require more effort from citizens to acquire firearms. On the federal level, two important pieces of legislation are the Gun Control Act of 1968 and the Brady Handgun Violence Prevention Act. The Gun Control Act requires that all professional gun dealers must have a Federal Firearms License (FFL). Only they can engage in inter-state trade of handguns, are granted access to firearm wholesalers and can receive firearms by mail. The Brady Act was enacted on November 30, 1993, and mandated background checks for all gun purchases through FFL dealers. Initially, the bill also imposed a five-day waiting period on handgun purchases, which upon successful lobbying by the National Rifle Association (NRA), was set to expire when the National Instant Criminal Background Check System (NICS) took effect in 1998. The NICS is a computer system operated by the FBI which handles all background checks related to the sales of firearms. While there is little regulation regarding firearm ownership at the federal level compared to other similarly developed countries, there is substantial heterogeneity in restrictions imposed by the states.[4] Most of the constraints on private firearm ownership at the state level attempt to either prohibit convicted felons or otherwise potentially dangerous people from acquiring guns, or restrict the usefulness of firearms for unlawful purposes independent of the buyer.

In this study, we focus on handguns, as these, in contrast to long guns, have to be purchased in the state of residence, are a popular choice for self-defense, can be carried concealed, and are used in homicides substantially more often than long guns (Federal Bureau of Investigation, 2016). We utilize two types of gun control measures that impose a delay between the decision to purchase a handgun and the moment when

[4] Overviews of all restrictions in the respective states can be found in NRA (2018) and Giffords Law Center to Prevent Gun Violence (2018).

the gun is actually transferred. The first measure is the imposition of mandatory waiting periods. While the establishment of waiting periods through the Brady Act aimed to give law enforcement agencies sufficient time to conduct background checks, they also provide a "cooling-off" period and can therefore help to prevent impulsive acts of violence (Cook, 1978; Andrés and Hempstead, 2011). In practice, buyers will perform a purchase (select a handgun, pass a NICS background check, and pay for the gun), but can only receive their handgun after the waiting period has elapsed. Between December 2010 and November 2013, the period of our study, nine states (California, Florida, Hawaii, Illinois, Maryland, Minnesota, New Jersey, Rhode Island, Wisconsin) and the District of Columbia had imposed mandatory waiting periods on the purchase of handguns.[5]

With respect to the second measure, some states require a license to possess or buy a handgun prior to the actual purchase, which due to bureaucratic hurdles can also impose a de-facto waiting time. Prospective buyers have to request the permit at a local authority (e.g. a sheriff's office), pass a NICS background check and pay the associated fee.[6] Only after the permit has been processed and issued, they may proceed to conduct the firearm purchase at their local dealer (usually without a renewed background check). Connecticut, Hawaii, Illinois, Maryland, Massachusetts, New Jersey, New York, Nebraska, North Carolina, and Rhode Island all require a purchasing permit during the period of our study. Michigan abolished their handgun permit requirement in December 2012, making it the only state to switch from imposing to not imposing delays during the time period we consider. Table 1 summarizes the waiting periods and license requirements for handguns across states and more details are provided in Appendix E. For the remainder of this paper, we will refer to a state that implemented a mandatory waiting period, required a purchasing permit, or both, according to Table 1 as a *Delay* state.[7] All other states we refer to as *NoDelay* states.

2.2 Background: The Firearm Demand Shocks of Late 2012

In the 2012 Presidential Election, President Barack Obama ran for a second term against Republican candidate Mitt Romney. While Romney took a more liberal position towards

[5] Wisconsin repealed its 48 hour waiting time on handguns in 2015.

[6] Fees can range from only $10 to several hundred dollars. See `https://www.cga.ct.gov/2013/rpt/2013-R-0048.htm`.

[7] For purchasing permits, Table 1 states the maximum delay that the law allows. There is no reliable information on average delays that we are aware of. As we binarize the treatment, this is inconsequential for our analysis.

Table 1: Handgun waiting periods and handgun purchasing license delay by state 2011-2013

State	AL	AK	AZ	AR	CA	CO	CT	DE	FL
Mandatory Waiting Period	0	0	0	0	10	0	0	0	3
Maximum Purchasing Permit Delay	0	0	0	0	0	0	90	0	0
State	GA	HI	ID	IL	IN	IA	KS	KY	LA
Mandatory Waiting Period	0	14	0	3	0	3	0	0	0
Maximum Purchasing Permit Delay	0	20	0	30	0	0	0	0	0
State	ME	MD	MA	MI	MN	MS	MO	MT	NE
Mandatory Waiting Period	0	7	0	0	7	0	0	0	0
Maximum Purchasing Permit Delay	0	30	30	10**	0	0	0	0	3
State	NV	NH	NJ	NM	NY	NC	ND	OH	OK
Mandatory Waiting Period	0	0	7	0	0	0	0	0	0
Maximum Purchasing Permit Delay	0	0	30	0	180	14	0	0	0
State	OR	PA	RI	SC	SD	TN	TX	UT	VT
Mandatory Waiting Period	0	0	7	0	0	0	0	0	0
Maximum Purchasing Permit Delay	0	0	14	0	0	0	0	0	0
State	VA	WA	WV	WI	WY	DC			
Mandatory Waiting Period	0	0	0	2*	0	10			
Maximum Purchasing Permit Delay	0	0	0	0	0	0			

Mandatory Waiting Period refers to the amount of time in days to pass between the purchase and the receipt of a firearm. If a state has different waiting periods for different types of firearms, the number refers to the purchase of handguns. Maximum Purchasing Permit Delay refers to the maximum time in days that can pass before a permit that will allow the holder to purchase one or more handguns will be issued or denied. 0 means that no permit is needed or will be issued instantaneously. * Repealed in 2015. ** Abolished in December 2012. Source: `http://lawcenter.giffords.org/`

gun rights, earning him the endorsement of the NRA, President Obama favored stricter gun control laws. Towards October, the race between both tickets moved towards a tie, with almost all polls showing the race as within the margin of error (Real Clear Politics, 2012). President Obama's victory on election night came then unexpected for Mitt Romney, who apparently did not even prepare a concession speech (International Business Times, 2017) as internal polls showed him winning (Silver, 2012). Just like after President Obama's first election in 2008, gun sales increased after his re-election but with considerable larger magnitude (CNN, 2008; CNN Money, 2012; Depetris-Chauvin, 2015). A likely reason for this was presumably because the President had started to speak more openly about favoring increased gun control measures in the wake of recent mass shootings, especially the one at a movie theater in Aurora, Colorado in July 2012.

A little more than one month later, on December 14, 2012, then 20-year-old Adam Lanza of Newtown, Connecticut first shot and killed his mother at their home before driving to Sandy Hook Elementary School, where he shot and killed six adult school employees and 20 students, who were between six and seven years old. Lanza committed suicide shortly after the first law enforcement officers arrived at the scene. His motives are still not fully understood, but it has been suggested that he had a history of mental illness. His father reported to have observed strange and erratic behavior in Lanza that he might have falsely attributed to his son's Asperger syndrome, rather than a developing schizophrenia (New Yorker, 2014). The massacre being the deadliest shooting at a U.S. high or grade school and the third deadliest mass shooting in U.S. history at the time, combined with the fact that most of the victims were defenseless children, sparked a renewed and unprecedented debate about gun control in the United States.

A few days after the shooting, President Barack Obama announced that he would make gun control a central issue of his second term. A gun violence task force under the leadership of Vice President Joe Biden was quickly assembled with the purpose of collecting ideas how to curb gun violence and prevent mass shootings. The task force presented their suggestions to President Obama in January 2013, who announced to implement 23 executive actions. These were aimed at expanding background checks, addressing mental health issues and insurance coverage of treatment, as well as enhancing safety measures for schools and law enforcement officers responding to active shooter situations. Additionally, the task force proposed twelve congressional actions, including renewing the Federal Assault Weapons Ban, expanding criminal background checks to all transactions, banning high capacity magazines, and increase funding to law enforcement agencies.

The proposals were met by fierce opposition from the NRA and some Republican legislators. At the end of January 2013, Senator Dianne Feinstein introduced a bill aimed at reinstating the Federal Assault Weapons Ban. While the bill passed the Senate Judiciary Committee in March 2013, it eventually was struck down on the Senate floor 40-60 with all but one Republicans and some Democrats opposing the bill. A bipartisan bill to be voted on at that same day, introduced by Senators Joe Manchin and Pat Toomey, aimed at introducing universal background checks, also failed to find the necessary three-fifths majority with 54-46, leaving federal legislation eventually unaffected.

Even though no new federal regulations eventually followed the events at Sandy Hook Elementary School, gun sales soared further in the months after the shooting. Fear of tougher gun legislation and a higher perceived need of self-protection drove up sales for both, handguns and rifles (Vox, 2016). While gun sales had surged after every prior mass shooting during the Obama administration, the increase in sales was unprecedented after the shooting at Sandy Hook. The extreme demand shift even created supply problems for some dealers, who were hoping to see sales increases of a magnitude of up to 400% (CNBC, 2012; Huffington Post, 2013). Several executives in the gun industry have

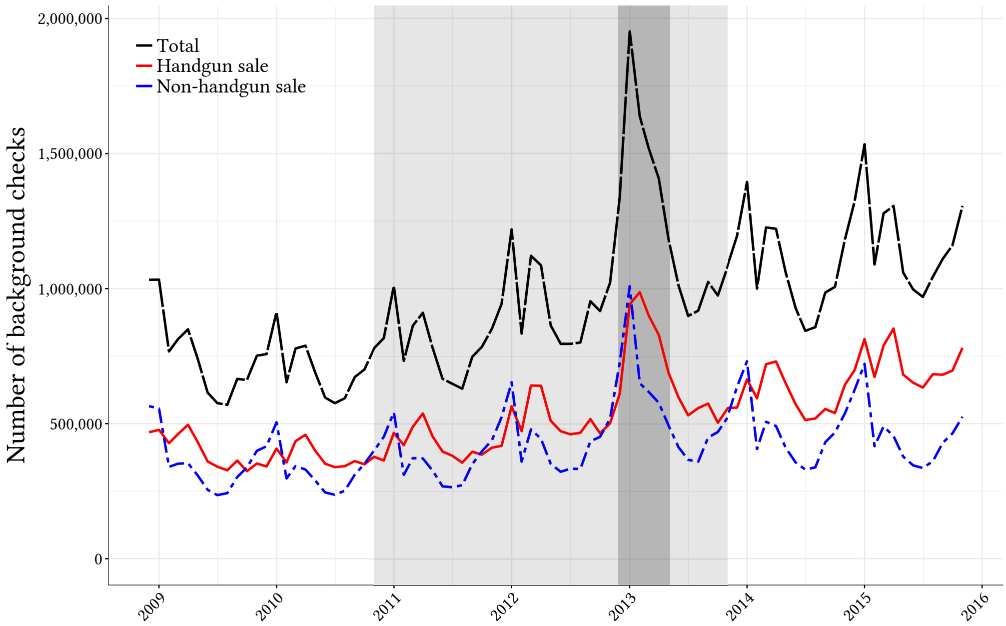

FIGURE 1: NICS BACKGROUND CHECKS

Monthly federal NICS gun sale background checks plotted over time between 2009 and 2015 in absolute numbers. The light gray area is our sample window, the dark grey area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line all other firearm-related background checks, and the black line displays the sum of the two.

stated that they view mass shootings as a boon to their business, attracting especially first-time gun owners. Tommy Millner, CEO of Cabela's in response to the Sandy Hook shooting said "the business went vertical ... I meant it just went crazy [... We] got a lot of new customers." and James Debney of Smith & Wesson explained that "the tragedy in Newtown and the legislative landscape [...] drove many new people to buy firearms for the first time." (The Intercept, 2015). Figure 1 shows the spike in gun sales over time, before and after the 2012 election and the Sandy Hook shooting. While gun sales generally increase at the end of the year, this particular spike is much more pronounced than in the years immediately before and after.

2.3 A Model of Firearm Purchases and Delay Laws

To understand why such firearm demand shocks may lead to persistent differences in gun sales between states that do and do not implement handgun purchase delays, and to explain our empirical findings, we present a simple theoretical framework. The model builds on existing work by Conlin, O'Donoghue, and Vogelsang (2007), who investigate the effect of changing weather patterns and projection bias on returns of catalog orders for cold-weather apparel. According to their model, consumers are more likely to return cold-weather apparel if the temperature on the order date is very low, or if it is very high shortly after delivery of the order. The driver of this prediction is projection bias over future climatic conditions that entices consumers to make decisions based on the weather at the time of purchase and/or receipt, rather than expectations over the item's life cycle. For the purpose of our paper, we extend their model to include naïve present bias in the spirit of O'Donoghue and Rabin (1999, 2001) as an additional source of time inconsistency.[8]

Purchasing Behavior of Perfectly Rational Agents Our analysis starts by assuming a perfectly rational representative agent i and her utility, actual and expected, from owning a gun in period t:

$$\nu(\mu(\boldsymbol{x_i}), \gamma_i, \omega_t) = [\mu(\boldsymbol{x_i}) + \gamma_i]\omega_t \tag{1}$$

$$\mathbb{E}_{H_t}[\nu(\mu(\boldsymbol{x_i}), \gamma_i, \omega_t)] = [\mu(\boldsymbol{x_i}) + \gamma_i]\mathbb{E}_{H_t}[\omega_t] \tag{2}$$

The agent's utility consists of two components. The first term (in square brackets) represents i's personal preference for owning a firearm in t and consists itself of two sub-parts. The first sub-part $\mu(\boldsymbol{x_i})$ of her personal preference can be explained by observables such as age, wealth or employment status, while the latter sub-part γ_i depends on unobserved variables. The distribution of γ_i is captured by $G(\gamma)$. As for the second component, the personal preference is scaled by an instrumental utility ω_t that describes time variations in utility that are common to all consumers. In the case of firearms, these could be the start of hunting season or, as in the case of the natural experiment we consider in this paper, country-wide shocks such as mass shootings that widely affect perceptions about the usefulness of firearms. The distribution of ω at time t is assumed to be $H_t(\omega)$. In our analysis, we interpret nation-wide shifts in firearm demand as shocks to ω_t. Equation 1 then refers to the actual utility, while equation 2

[8] The simultaneous presence of projection bias and naïve present bias has empirically been documented by Augenblick and Rabin (2018). Note that we do not claim projection bias and present bias to be the only possible drivers of time-inconsistent behavior in our setting. Our empirical results will show that *some* form of time-inconsistent behavior can better explain the patterns in the data than time consistency, but we are not interested in pinpointing specific biases. Alternative mechanisms are manifold, for examples see Imas, Kuhn, and Mironova (2016) and Gabaix and Laibson (2017).

describes the expected utility when personal preferences are known, but the future, and thus instrumental utility, is still uncertain.

Naturally, a gun is not used over a single period as the above equations suggest but over a (finite) lifetime T.[9] Hence, for i to consider buying a firearm, she will evaluate her net present value of the purchase in period 0 over the gun's entire lifetime T subject to exponential discounting with discount factor δ_i. We also model gun purchase delay laws for state s which dictate that i can only start using her firearm after the period $D_s \geq 0$.[10] i's expected net present value then reads:

$$
\begin{aligned}
U_{i0s} &= \sum_{t=D_s}^{D_s+T} \delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\boldsymbol{x_i}), \gamma_i, \omega_t)] \\
&= [\mu(\boldsymbol{x_i}) + \gamma_i]\Psi_{i0s} \\
\text{with} \qquad & \Psi_{i0s} \equiv \sum_{t=D_s}^{D_s+T} \delta_i^t \mathbb{E}_{H_t}[\omega_t]
\end{aligned}
\tag{3}
$$

In order to buy a firearm, the prospective owner will have to incur state-specific expenses in the form of a gun price p_s and transaction costs c_s. We normalize the agent's outside option to zero. A rational agent's decision to purchase a gun in period 0 then depends on whether discounted lifetime purchase utility exceeds these expenses:

$$
\begin{aligned}
P[Buy_{i0s}] &= P\Big[U_{i0s} - p_s - c_s > 0\Big] \\
&= P\Big[[\mu(\boldsymbol{x_i}) + \gamma_i]\Psi_{i0s} - p_s - c_s > 0\Big] \\
&= P\Big[\gamma_i > \frac{p_s + c_s}{\Psi_{i0s}} - \mu(\boldsymbol{x_i})\Big] \\
&= P\Big[\gamma_i > \bar{\gamma}_{i0s}\Big]
\end{aligned}
\tag{4}
$$

[9]That firearms need to be well maintained to not break is well known among gun enthusiasts. Gun parts such as springs, stocks, magazines, and grips need to be regularly replaced due to wear and tear—and exposure to the elements facilitates corrosion.

[10]Throughout the paper, we make the implicit assumption that prospective buyers are well-informed about gun purchasing delays in their state when they decide to buy a firearm. We deem this assumption adequate for several reasons. First, most potential buyers are presumably aware of the fact that gun legislation (and therefore ease of access to firearms) differs across states. Therefore, we would expect them to research the process of obtaining a gun before finalizing their decision on whether to purchase a firearm or not. Second, we would not expect prospective buyers to never have considered buying a firearm before. This is especially true if the shock did not extremely shift preferences for guns. Buyers who in the past were relatively close to considering arming themselves should have a higher inclination to learn about gun laws, and therefore should be more informed.

From the above it follows that i will only buy a gun in period 0 if her innate gun valuation γ_i surpasses the threshold level $\bar{\gamma}_{i0s}$. This threshold is endogenous to socio-demographics $\boldsymbol{x_i}$, gun prices p_s, transaction costs c_s and discounted future instrumental expected utility values Ψ_{i0s}, which in turn depend on the state's gun purchase delay laws.[11] The only difference in Ψ_{i0s} between states with and without delay laws is caused by a shift of consumption streams into the future. We assume differences in gun prices and transaction costs to be negligible across states, so that we can derive the following predictions:

R1. The difference in $P[Buy_{i0s}]$ between states with and without delays in the absence of demand shocks increases smoothly with delay length D_s.

R2. There should be *almost* no difference in $P[Buy_{i0s}]$ between states with short delays and states without delays in the absence of demand shocks.

R3. An increase in ω_0 will disproportionately increase the differences in $P[Buy_{i0s}]$ between states with and without delays.

R4. There should be no response in $P[Buy_{i0s}]$ to shocks in ω_0 in states with delays.

Prediction R1 arises because the differences in future discounted instrumental utility streams Ψ_{i0s} for $D_s > 0$ and $D_s = 0$ will become very small if delays are short and changes in (expected) instrumental utility over short temporal distances are not overly large $\omega_0 \approx \mathbb{E}_{H_{D_s}}[\omega_{D_s}]$. The latter should be true, because if ω_0 is close to its expectation $\mathbb{E}_{H_0}[\omega_0]$, then it should also be close to the expectation $\mathbb{E}_{H_{D_s}}[\omega_{D_s}]$ if D_s was, for instance, only one day. Likewise, expectations for ω_T would then also be close to ω_{D_s+T}. R1 implies that we should observe monotonically decreasing levels of handgun sales with increasing purchase delays if fully rational consumers were behind the gun sales patterns we observe. Prediction R2 follows immediately from R1 and arises because short delays should not impact decisions much unless consumers discount heavily. Prediction R3 suggests that pre-existing differences in $P[Buy_{i0s}]$ will be amplified by shocks to instrumental utility. Finally, prediction R4 claims that the differences prescribed by R3

[11] To keep the model simple, we consider waiting periods and purchasing permits together. Similar to the purchasing price of firearms when facing waiting periods, purchasing permits require up-front fees. Additionally, waiting periods require exactly two trips to complete a gun purchase, and this is technically also feasible for purchasing permits, such that opportunity costs of time, transportation costs, and psychological costs should be roughly equal for both measures.

will arise because i's decision will only be affected if she can use the gun right away. The reason behind this is that a change in contemporary expected utility of a firearm should only affect a purchasing decision if the gun can be used instantaneously, while future considerations should be unaffected.[12]

Purchasing Behavior of Behavioral Agents The above model with a perfectly rational agent predicts that delay laws should have a rather smooth effect on demand which will be exacerbated by shocks to instrumental utility. One reason for that is that agents behave time-consistently, i.e. they will not change a once made decision at a later point in time. Behavioral economists, however, have identified several cognitive biases that may render decision behavior time-inconsistent. Following Conlin, O'Donoghue, and Vogelsang (2007), we first introduce projection bias in the fashion of Loewenstein, O'Donoghue, and Rabin (2003) with degree α_i. Additionally, and moving beyond Conlin, O'Donoghue, and Vogelsang, we then impose naïve present bias of degree β_i as described in O'Donoghue and Rabin (1999).[13]

Projection bias leads to the following changes in period t utility when expectations are formed in period 0:

$$\tilde{\nu}(\mu(\boldsymbol{x_i}), \gamma_i, \omega_t | \omega_0) = [\mu(\boldsymbol{x_i}) + \gamma_i][(1 - \alpha_i)\omega_t + \alpha_i \omega_0] \tag{5}$$

$$\mathbb{E}_{H_t}[\tilde{\nu}(\mu(\boldsymbol{x_i}), \gamma_i, \omega_t | \omega_0)] = [\mu(\boldsymbol{x_i}) + \gamma_i][(1 - \alpha_i)\mathbb{E}_{H_t}[\omega_t] + \alpha_i \omega_0] \tag{6}$$

The degree of projection bias α_i now captures the extent to which the current period's common utility component determines preferences relative to expectations based on the

[12] This assumes a short and transient demand shift. The case of a more permanent demand shift is similar to the case of projection bias, which we explore in the following paragraph. Alternatively, the model sketched in the following paragraph could assume a permanent demand shock instead of projection bias to arrive at similar conclusions. Note that present bias, however, would still be needed to generate time-inconsistent behavior in line with our empirical findings. We believe a short transitory shock to be more realistic, as the data shows quickly receding handgun sale background checks after the defeat of gun control bills in the U.S. senate.

[13] The importance of naïvete over sophistication has been well documented experimentally (Augenblick and Rabin, 2018; Fedyk, 2017) and is the more interesting case as naïvete makes present bias particularly costly (DellaVigna and Malmendier, 2006).

distribution H_t. The present bias parameter β_i comes into play when calculating the consumer's lifetime utility:

$$
\begin{aligned}
\tilde{U}_{i0s} &= \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\boldsymbol{x_i}), \gamma_i, \omega_t | \omega_0)] \qquad (7)\\
&= [\mu(\boldsymbol{x_i}) + \gamma_i] \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t [(1-\alpha_i)\mathbb{E}_{H_t}[\omega_t] + \alpha_i \omega_0]\\
&= [\mu(\boldsymbol{x_i}) + \gamma_i] \tilde{\Psi}_{i0s}
\end{aligned}
$$

$$
\begin{aligned}
\text{with} \quad & \tilde{\Psi}_{i0s} \equiv (1-\alpha_i)\bar{\Psi}_{i0s} + \alpha_i \tilde{m}_{i0s}\\
\text{and} \quad & \bar{\Psi}_{i0s} \equiv \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_t}[\omega_t]\\
\text{and} \quad & \tilde{m}_{i0s} \equiv \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \omega_0 = \delta_i^{D_s}\Big[\beta_i^{\mathbb{1}(D_s>0)} + \beta_i \delta_i \frac{1-\delta_i^T}{1-\delta_i}\Big]\omega_0
\end{aligned}
$$

The probability of a positive lifetime utility for the behavioral agent can then be written as follows:

$$
\begin{aligned}
P\Big[\tilde{U}_{i0s} - p_s - c_s > 0\Big] &= P\Big[[\mu(\boldsymbol{x_i}) + \gamma_i]\tilde{\Psi}_{i0s} - p_s - c_s > 0\Big] \qquad (8)\\
&= P\Big[\gamma_i > \frac{p_s + c_s}{\tilde{\Psi}_{i0s}} - \mu(\boldsymbol{x_i})\Big]\\
&= P\Big[\gamma_i > \tilde{\tilde{\gamma}}_{i0s}\Big]
\end{aligned}
$$

Both α_i and β_i may render i's behavior time-inconsistent. In contrast to hypotheses R1, projection bias will make shocks to ω_0 influence i's evaluation of a gun's lifetime utility even if delay laws forbid her to use the firearm in the present period. Present bias β_i, on the other hand, may keep i from purchasing even if she has a positive lifetime utility at time 0. The reason for this is that immediate expenditures are disproportionally discounted for future periods and may make a purchase in, say, period 1 more attractive than in period 0. Since the same decision process applies in period 1, naïvete will lead the consumer to never buy a firearm if she does not buy immediately. This *buy today* probability can also be expressed formally:

$$
\begin{aligned}
&P[\tilde{U}_{i0s} - p_s - c_s > \tilde{U}_{i1s} - \beta_i\delta_i p_s - \beta_i\delta_i c_s] \\
=&P\Bigg[\sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)}\delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\boldsymbol{x_i}),\gamma_i,\omega_t|\omega_0)] - p_s - c_s \\
&> \sum_{t=D_s+1}^{D_s+T+1} \beta_i\delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\boldsymbol{x_i}),\gamma_i,\omega_t|\omega_0)] - \beta_i\delta_i p_s - \beta_i\delta_i c_s\Bigg] \\
=&P\Big[[\mu(\boldsymbol{x_i})+\gamma_i]\Delta\tilde{\Psi}_{i0s} > (1-\beta_i\delta_i)(p_s+c_s)\Big] \\
=&P\Big[\gamma_i > \frac{(1-\beta_i\delta_i)(p_s+c_s)}{\Delta\tilde{\Psi}_{i0s}} - \mu(\boldsymbol{x_i})\Big] \\
=&P\Big[\gamma_i > \tilde{\gamma}_{i0s}\Big]
\end{aligned}
\tag{9}
$$

$$
\begin{aligned}
\text{with} \qquad & \Delta\tilde{\Psi}_{i0s} \equiv (1-\alpha_i)\Delta\bar{\Psi}_{i0s} + \alpha_i\Delta\tilde{m}_{i0s} \\
\text{and} \qquad & \Delta\bar{\Psi}_{i0s} \equiv \delta_i^{D_s}\Big[\beta_i^{\mathbb{1}(D_s>0)}\mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \beta_i\delta_i^{T+1}\mathbb{E}_{H_{T+D_s+1}}[\omega_{T+D_s+1}]\Big] \\
\text{and} \qquad & \Delta\tilde{m}_{i0s} \equiv \delta_i^{D_s}\Big[\beta_i^{\mathbb{1}(D_s>0)} - \beta_i\delta_i^{T+1}\Big]\omega_0
\end{aligned}
$$

Since a behavioral agent needs to have a positive lifetime utility *and* decide buying today, her probability of purchasing is somewhat more complex: $P[Buy_{i0s}] = P[\tilde{U}_{i0s} - p_s - c_s > 0 \cap \tilde{U}_{i0s} - p_s - c_s > \tilde{U}_{i1s} - \beta_i\delta_i p_s - \beta_i\delta_i c_s]$. As shown in Appendix Section A, the lifetime utility constraint $\tilde{U}_{i0s} - p_s - c_s > 0$ is highly unlikely to be ever binding such that $P[Buy_{i0s}] \approx P[\gamma_i > \tilde{\gamma}_{i0s}]$. Similar to the rational case, i will only buy a gun at time 0 if her gun valuation γ_i surpasses some threshold level which is now $\tilde{\gamma}_{i0s}$. Based on this threshold, and again assuming negligible expenditure differences across states, one can derive the equivalent hypotheses for the behavioral version of the model:

B1. The difference in $P[Buy_{i0s}]$ between states with and without delays in the absence of demand shocks increases sharply for $D_s \geq 1$ and then further smoothly with delay length D_s if consumers are present-biased ($\beta_i < 1$ and for any value of α_i).

B2. There should be a substantial difference in $P[Buy_{i0s}]$ between states with short and without delays in the absence of demand shocks if consumers are present-biased and not projection-biased ($\beta_i < 1$ and $\alpha_i = 0$). With increasing degree of projection bias ($\alpha_i \to 1$), this substantial difference should also hold during demand shocks.

B3. An increase in ω_0 will disproportionately increase the differences in $P[Buy_{i0s}]$ between states with and without delays (for any value of α_i and β_i).

B4. There should be no response in $P[Buy_{i0s}]$ to shocks in ω_0 for states with delay laws in place ($D_s > 0$) in the absence of projection bias ($\alpha_i = 0$ and for any value of β_i).

The reasoning behind B1 is that D_s decreases both $\Delta\bar{\Psi}_{i0s}$ and $\Delta\tilde{m}_{i0s}$ sharply when $\mathbb{1}(D_s > 0)$ applies and then smoothly for higher values of D_s. This is because present bias leads to strong discounting of all gun utility as soon as it is postponed to future periods. Statement B2 follows a similar logic as R2. If one assumes $\beta_i \to 1$, the divergence must be generated by δ^{D_s} and differences in $\mathbb{E}_{H_{D_s}}[\omega_{D_s}]$ and $\mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}]$. If we assume a discount factor close to 1, then $\delta_i^{D_s}$ should not matter a lot when D_s is short. The same applies to $\mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}]$. The more problematic component is $\mathbb{E}_{H_{D_s}}[\omega_{D_s}]$ which can only be similar to ω_0, even for short delays, when demand shocks are absent at time 0. The higher the degree of projection bias, however, the more agents will solely rely on ω_0 and thus the above also holds when instrumental utility peaks. In other words, because consumers believe to always have a high utility from owning a gun, present bias will, even for very short delays, severely discount future consumption streams. Statement B3 is borne out of the fact that for pure projection bias ($\alpha_i = 1$), the different levels of $\Delta\tilde{m}_{i0s}$ are simply amplified by ω_0. This, however, also holds for $\alpha_i = 0$ since ω_0 does not enter $\Delta\bar{\Psi}_{i0s}$ if $D_s > 0$, i.e. when current instrumental utility should not have an effect on the purchase criterion since the consumer does not benefit from the firearm anymore in period 0. Prediction B4 essentially restates R4 when assuming no projection bias. This means that projection bias is necessary to generate behavioral adjustments in states with delay laws when demand shocks occur.

The model predictions derived in this section demonstrate that with time-inconsistent agents, even relatively short delays can have substantive impacts on gun sales in the wake of a demand shock, such as a mass shooting. Using the data described in the following section, we will make a case for the patterns in the data being more consistent with many gun buyers behaving time-inconsistently.

3 Data

3.1 Handgun Purchases

One of the main issues in establishing changes in firearm ownership is the absence of a central database for gun owners and firearm sales. In order to overcome this, researchers have turned to proxy variables from surveys, vital statistics, crime data and gun magazine subscriptions. While some of these indicators have performed quite well for cross-sectional estimation, they have been found unsuitable for tracking gun ownership over time (Kleck, 2004). As mentioned above, Federal law dictates that since November 1998, a background check has to be carried out for every firearm transaction through an FFL dealer. Background check data from the National Instant Criminal Background Check System (NICS) has the advantage of being comparable across time, providing high coverage at monthly frequency and distinguishes between different types of transactions and firearms.[14] In our analysis, we use monthly NICS handgun sale background checks in a given state between December 2010 and November 2013, divided by the 2010 population in 100,000. In order to interpret our results as semi-elasticities while keeping potential zero observations, we apply the *inverse hyperbolic sine transformation* (arcsinh) rather than taking natural logarithms (Burbidge, Magee, and Robb, 1988).[15]

However, as pointed out in a few recent studies, the NICS data also exhibits important drawbacks (Lang, 2013, 2016; Levine and McKnight, 2017). First, it does not allow any inference on the *stock* of firearms and ownership levels, but can only measure flows of weapons. Second, these flows might be substantially understated as about 22% of firearm sales are between private parties and occur in states which do not require background checks for private transactions (Miller, Hepburn, and Azrael, 2017). Third, a background check can occur for an exchange of an old for a new firearm, as well as for the purchase of multiple weapons. Finally, some states require a background check for a concealed carry permit application but not for a handgun purchase itself. Other states are running regular or irregular re-checks on permit holders regardless of guns being bought and thereby inflate the counts.

[14]The data is available for download at `https://www.fbi.gov/file-repository/nics_firearm_checks_-_month_year_by_state_type.pdf`.

[15]For convenience, we refer to the *arcsinh* transformation as *log* throughout the paper. We provide robustness checks in *levels* for all main specifications in the appendix which confirm our findings.

We believe that our setup mitigates at least some of these problems. One reason is that many handgun purchases during the demand shock in late 2012 were made by new gun owners according to the anecdotal evidence described earlier and findings by Studdert et al. (2017). This should substantially mitigate the difference between gun sales and changes in gun ownership, and better reflect the inflow of new firearms. Our analyses in later sections will furthermore explicitly tackle gun sales on secondary markets as an important challenge to identification. To capture cases in which buyers obtain a permit in order to purchase a handgun, we add background checks for permits to our measure of handgun sales and remove three states (Hawaii, Illinois and Massachusetts) where this is not feasible as permit checks in these states may also include permits for long guns.[16]

Furthermore, we need to remove Iowa, Kentucky, Maryland, Michigan, Pennsylvania, Utah and Wisconsin from the sample in order to be able to properly measure flows of handguns. Iowa changed their gun laws in 2011, removing a requirement for demonstrating firearm proficiency before a firearm could be acquired. This led to unusual background check jumps in early 2011. Kentucky performs monthly rechecks of existing permit holders, artificially inflating the data (Lang, 2013, 2016). Maryland changed its gun laws with respect to licensing in 2013, leading to a massive increase around the same time (New York Times, 2015). As already mentioned earlier, Michigan changed from requiring a permit to not requiring a permit in the period of observation. Pennsylvania didn't record a single handgun sale background check in 2012. Utah performed quarterly rechecks of existing permit holders in 2011 leading to strong spikes in background checks. Finally, Wisconsin passed a concealed-carry bill in 2011 leading to a one-off jump in background checks in November 2011.[17] Connecticut is furthermore excluded as it was host to the shooting at Sandy Hook, and including the state may thus violate our identification assumptions as homicides change through the shooting. While we prefer this restricted sample for our analysis, robustness checks for our main results will show that even less restrictive sample definitions will generate qualitatively similar results.

[16] Any further reference to handgun background checks implicitly includes background checks made for permits, unless otherwise noted.

[17] In Appendix F we plot the temporal variation in handgun sale and permit NICS background checks for each state separately to demonstrate the data irregularities for these states.

3.2 Homicide and Mortality

The main outcome of interest in this paper are homicides. There are two main sources of homicide statistics for the United States: death certificates from the *National Vital Statistics System* (NVSS) and police reports from the FBI's *Uniform Crime Reporting Program* (UCR). Despite the UCR data being widely used to study crime, they are known to suffer from reporting issues that need to be taken into account by removing affected areas from the data (Targonski, 2011). Coverage is therefore not universal. The NVSS data, however, consists of all U.S. death certificates in a given year. We obtained the data via the *Center for Disease Control and Prevention* (CDC) for the entire sample period between December 2010 and November 2013. The data set contains ICD-10 codes for the underlying cause of each death recorded in the United States, as well as the victim's demographics, county of residence and circumstances of the injury such as location and date. The ICD-10 codes allow us to distinguish not only between homicides, suicides and fatal accidents but also whether any of these were inflicted through a handgun or not.[18] We aggregate this data at the county-month level to obtain a balanced panel of 3,050 counties and normalize by the county's 2010 population in 100,000.[19] Figure 2 shows the states represented in our NICS sample and the counties represented in our NVSS sample.

In order to cross-validate our results and delve deeper into homicide circumstances, we also utilize the aforementioned UCR data, bearing in mind the limitations of the data. In order to determine the circumstances of the observed murders, we exploit the UCR *Supplementary Homicide Reports* (SHR) series. These reports are compiled from voluntary submissions by individual law enforcement agencies to the FBI and contain detailed information such as demographics of victim and offender, the type of weapon used as well as murder circumstances (e.g. argument, gang-related crime). We collapse

[18] Our measure of handgun-related incidents also encompasses instances when an undetermined type of firearm was used. This should not bias our estimates in any way, and it is corroborated by the fact that the vast majority of homicides are carried out with handguns.

[19] We remove Connecticut and Michigan from the sample. As explained before, including Connecticut may invalidate our identification, as the shooting at Sandy Hook mechanically increased homicides. Michigan switched from requiring a permit to not requiring a permit in our sample period. When we present our results, we will also present robustness checks that apply more or less stringent sample restrictions and deliver very similar results.

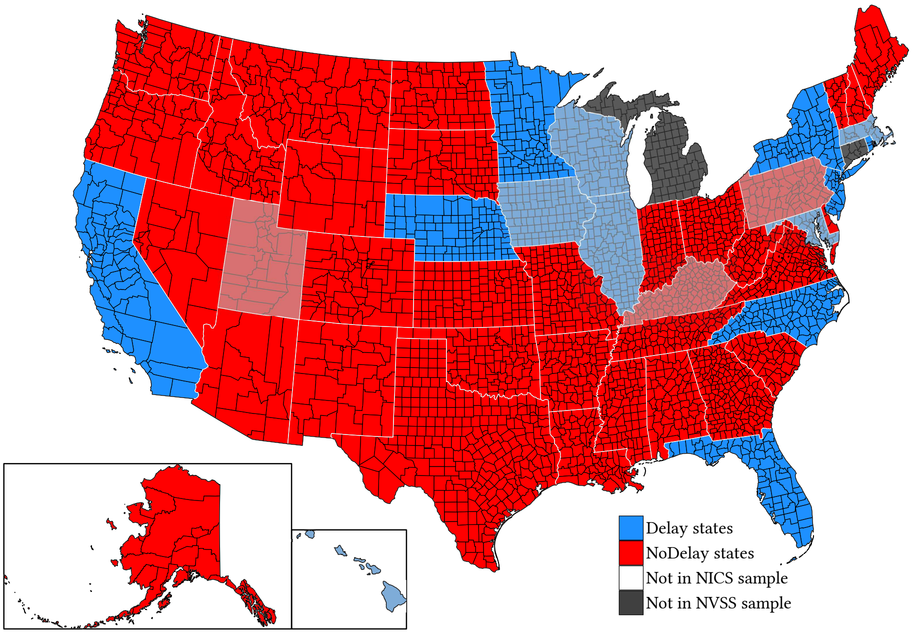

Figure 2: States and counties represented in the NICS and NVSS samples

Map of the United States showing the states contained in the NICS background check data, and the counties contained in the NVSS homicide data. Red counties are located in *NoDelay* states. Blue counties are located in *Delay* states. Shaded states are not present in the NICS sample. Grey counties are not present in the NVSS sample.

the cleansed UCR homicide data to the county-month level to obtain a balanced panel with data from 2,232 counties.[20]

3.3 Gun Interest, Gun Shows and Controls

In order to assess whether consumers in states with and without handgun purchase delays are similar in their preferences, and to judge the adequacy of modeling consumers as time-inconsistent, we would like to separate intentions from actions. While the NICS data measure the latter, we rely on internet search data from *Google Trends* as a proxy for people's intention to purchase firearms. We focus on searches for the term "gun store", which has been shown to be a good predictor of firearm purchasing intentions by prior research (Scott and Varian, 2014). Crucially, Google search data is not available at an absolute level and always scaled on a 0-100 interval with respect to the maximum volume within the specified time and geographic area.

[20] The cleaning procedure applied to the UCR data sets on homicide and other crimes is discussed in Appendix G.

To circumvent this restriction, we adopt a technique similar to the one used by Durante and Zhuravskaya (2018): First, we queried *relative* "gun store" searches across U.S. states from 01/01/2008 until 31/12/2016 and divided the numbers by 100 to construct a pseudo-ranking of states. Next, we obtained the relative monthly "gun store" volume for each state *individually* over the same time period and divided again by 100. Multiplying the results from these two stages already offers a coherent monthly state-panel for the relative search volume from 2008 until the end of 2016. In order to zoom further into the monthly variation, we then queried the relative daily "gun store" volume for each state in 3 month intervals, re-scaled each month to a 0-100 interval and finally multiplied each month's daily volumes with the state-month weights constructed before. Despite being at a daily frequency, we aggregated each state's series within the panel for our analysis to a weekly level in order to reduce noise.

Handgun purchases on secondary markets (such as gun shows) that are not reflected in the NICS background check data might lead our outcome measure of gun sales to be biased. We therefore collected data on the demand and supply of gun shows. Our measure of gun show demand is constructed using Google search data for the term "gun show" in the same way as we did for "gun store" searches. In Appendix D.1 we also use a measure of gun show supply for which we obtained data on locations of gun shows across the United States from `http://www.gunshowmonster.com/`. This website allows users to make submissions, which will be published after editorial approval. Our final sample contains 8,764 geo-located gun shows between July 2009 and December 2014 across almost all U.S. states. These numbers are again aggregated to the county-month level and normalized by the 2010 population in 100,000. We note that the sample is surely incomplete and possibly even skewed towards certain states with easier access to guns. Consequently, we only use this data in supplementary estimations to show that the effects regarding the supply and demand for gun shows are most likely going in similar directions.

Finally, we use several control variables to account for potential confounds as well as differences in socio-economic characteristics across counties and states. Our core set of covariates includes log of population, the shares of population living in rural areas and below the poverty line as well as the percentages of black and hispanic inhabitants. All variables were obtained from the 2010 U.S. Decennial Census at the county level (and aggregated for state-level analyses). In addition, we collected state level data on the percentage of households with internet access from the 2010 American Community Survey which we include in regressions using Google search data. In selecting these control variables, we broadly followed the choices made in prior studies which have investigated the relationship between firearm prevalence and crime (e.g. Cook and Ludwig, 2006; Duggan, 2001).[21]

4 Empirical Strategy

4.1 Difference-in-Differences Approach

To estimate the mitigating effect of delay laws on handgun purchases and mortality during a demand shock, we use a *Difference-in-Differences* (DiD) model which exploits time-series variation from the six-month surge in firearm demand across the United States. Our theoretical framework predicts that shifts in gun valuation triggered by these events should translate into a comparatively smaller likelihood of buying a handgun when delay laws are in place. We denote all states that required handgun buyers to observe a waiting period or to possess a permit/license according to Table 1 as *Delay* (as opposed to *NoDelay*) states.[22] Next, we create an indicator variable $Post1_t$ for time periods starting after the Obama re-election on November 6$^{\text{th}}$, 2012 and ending after April 17$^{\text{th}}$, 2013 when the proposals for a renewed assault weapons ban and universal background checks were defeated in the U.S. Senate. We also use a second time dummy $Post2_t$ which equals one for time periods starting after April 17$^{\text{th}}$ 2013 to investigate effects beyond the six months. Our proposed instrument for new gun owners is thus the interaction term $Delay_s \times Post1_t$.

[21] Due to the use of location-specific fixed effects as described in the next section, we will be interacting our control variables with month fixed effects, such that each control variable will enter the regressions 36 times. This approach prescribes a parsimonious use of control variables. The exact choice of covariates does not seem to be crucial to the results. In an earlier version of this paper where we used a slightly altered set of covariates, we obtain very similar results. See http://www.efm.bris.ac.uk/economics/working_papers/pdffiles/dp18694.pdf.

[22] These states are California, Florida, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Minnesota, Nebraska, New Jersey, New York, North Carolina, Rhode Island, Wisconsin and the District of Columbia.

Our instrument can then be plugged into a typical DiD regression equation in order to estimate the effect of the demand shock on the rates of new gun owners and homicides:

$$
\begin{aligned}
log(HandgunSales_{st}) &= \alpha + \beta_1(Delay_s \times Post1_t) + \beta_2(Delay_s \times Post2_t) \\
&\quad + \delta \mathbf{X_{st}} + \gamma_s + \lambda_t + t_s + \epsilon_{st} \qquad (10) \\
log(Homicides_{ct}) &= \alpha + \beta_1(Delay_s \times Post1_t) + \beta_2(Delay_s \times Post2_t) \\
&\quad + \delta \mathbf{X_{ct}} + \gamma_c + \lambda_t + t_c + \epsilon_{ct} \qquad (11)
\end{aligned}
$$

Our primary coefficient of interest is β_1, capturing the average difference of $log(Handgun\ Sales_{ct})$ and $log(Homcides_{ct})$ in $Delay$ states over $NoDelay$ states during the demand shock.[23] Apart from the aforementioned outcome and treatment variables, this model also features fixed effects for counties γ_c (or states, γ_s), and time periods λ_t, as well as location-specific linear time-trends denoted t_c and t_s respectively.[24] Our results do not depend on the inclusion of these trends but increase the precision of our main estimates. Furthermore, our regression models each also feature a set of control variables $\mathbf{X_{st}}$ and $\mathbf{X_{ct}}$, respectively. In order to avoid a "bad control" problem, we use interactions of pre-determined, time-invariant factors and time fixed effects instead of time-varying controls. The variables included in this way are % hispanics, % black, % rural, log of population, and % poverty. $\epsilon_{ct}/\epsilon_{st}$ denotes the residual term. The standard errors used for inference are clustered by state as the level of treatment to account for serial correlation in the error terms. Regressions are weighted by the state/county population to reduce the impact of less densely populated areas and to obtain U.S. wide average effects.

A potential alternative to estimating equation 11 directly would be to use equation 10 as a first-stage in an instrumental variables regression with homicide rates as the dependent variable, and directly estimate a gun owner-homicide elasticity. Our preference for the reduced-form relation stems from two factors. The first reason comes from data limitations which have already been discussed previously. NICS background checks do not allow to draw inference on changes in the existing stock of guns, making an elasticity hardly comparable to other studies. This concern is compounded by issues

[23] Alternative specifications where we consider levels instead of logs will be presented together with the results.

[24] We use county level data whenever available to increase statistical power.

of measurement error, as not all background checks lead to firearm purchases and not all purchases are reflected in the background check counts. Our second concern is that we do not expect the effect of guns on homicides to be overly large since the vast majority of legally acquired guns are usually used for lawful purposes (Fabio et al., 2016). In order to precisely estimate such a small effect, one would need a fairly large sample at the county level for which, however, no NICS data exists. We thus estimate the raw effect of handgun purchase frictions on sales and homicide rates during a demand shock but do not pin down a precise elasticity given the absence of reliable panel data on firearm ownership.

4.2 Validity of Identifying Assumptions

To assure credible identification and the validity of our DiD design, we need two assumptions to be fulfilled. First, our outcome measures were following similar trends in *Delay* and *NoDelay* states to prevent that our estimates are simply picking up pre-treatment divergence. As we can see from Figures 3 and 4, the raw data shows that handgun sales and homicides in both groups of states are only sharply diverging during the six month window of increased firearm demand.[25] Econometrically, we further address this concern by allowing for location-specific time trends, testing for various other trend-specifications and including an event-study analysis as an additional robustness check. As an appropriate sample length, we use data between December 2010 and November 2013, exactly 24 months before and 12 months after the 2012 election. This choice is motivated by the reasoning of Wolfers (2006), who argues that, in order to be able to credibly identify pre-existing trends, sufficient time periods before the studied event should be considered. This should also, he argues, ameliorate any bias due to more complex dynamics than just a simple structural break.

The second prerequisite for our DiD design is the absence of other events occurring around the treatment period which may be responsible for the observed effects. As argued above, we believe that the outcome of the 2012 election as well as the timing of the Sandy Hook shooting were exogenous to any relevant outcome variables. Nevertheless, both events could have had asymmetric impacts on people's attitudes towards firearms across states or brought pre-existing differences in underlying firearm preferences to light. This is an important concern since state-specific demand shocks would also

[25] Appendix Figures 10 and 11 depict the evolution in levels.

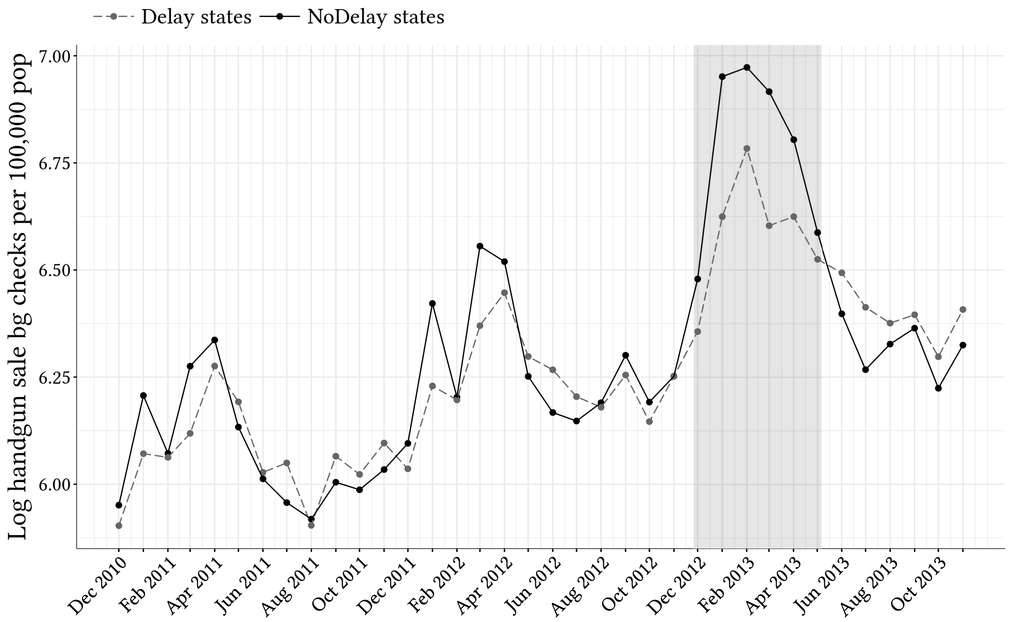

Figure 3: Log background check rate for handguns in *Delay* vs *NoDelay* states

Log of monthly NICS background checks per 100,000 inhabitants for handguns in *Delay* states and *NoDelay* states between December 2010 and November 2013. The sample encompasses data for all states for which NICS data is included in our main specification. The grey-shaded area includes the first six months after the 2012 election and the shooting at Sandy Hook. For better visibility, the graphs have been re-scaled to coincide on the last observation before the treatment.

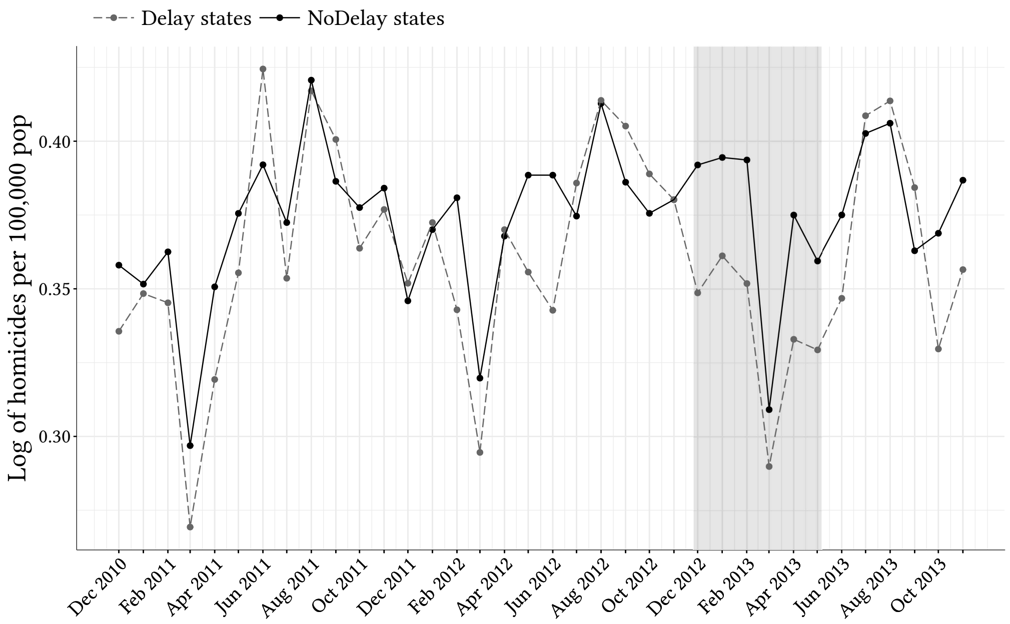

Figure 4: Log homicide rate in *Delay* vs *NoDelay* states

Log of monthly homicides per 100,000 inhabitants in *Delay* states and *NoDelay* states between December 2010 and November 2013. The sample encompasses data for all states for which NICS data is included in our main specification. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook. For better visibility, the graphs have been re-scaled to coincide on the last observation before the treatment.

violate an important assumption of our model. We address this issue by checking for differential changes in gun purchasing intentions proxied by Google searches for "gun store" to establish comparability of *Delay* and *NoDelay* states. Placebo regressions serve as additional tests whether any findings are also due to seasonality.

Another potential objection would be that our reduced-form effect on homicides in equation 11 is not the result of prevented firearm sales but rather a *direct* outcome of the firearm demand shock. To mitigate this concern, in a first step, we remove the state of Connecticut (where the Sandy Hook shooting took place) from our regressions since the homicide rate was immediately affected by the treatment. Secondly, by including a set of covariates with time-varying influence, we should be able to filter out the influence of factors that are commonly associated with homicides. Finally, the fact that we are considering two very different kind of events that have both been shown to influence firearm demand, makes a direct effect on homicides very unlikely.

5 The Effect of Delay Laws on Firearm Purchases

5.1 Results

Our empirical analysis with respect to firearm purchases has three objectives. First, we test the econometric validity of our proposed instrument $Delay_s \times Post1_t$ as suggested by our model hypotheses R3/B3 and Figure 3. Second, we evaluate the robustness of the results, as well as the plausibility of alternative explanations. Third, we test for the specific mechanisms sketched in our model in Section 2.3, especially whether our findings are in line with purely rational behavior.

We start by investigating the effect of the 2012 Presidential election and the shooting at Sandy Hook Elementary School on our NICS handgun sale background check measure depending on whether states implemented handgun purchase delay laws. Figure 3 shows an unusually strong increase in log background checks for both groups of states at the end of 2012. In line with our model predictions R3 and B3, gun sales increase less strongly in *Delay* states until about May 2013 when they rise above those in *NoDelay* states. At first sight, the data suggests that the sales deficit before May 2013 appears larger than the excess afterwards. This is important, as although simply postponing the firearm purchase would still provide useful time variation to explore the effect on

Table 2: Handgun sale background checks

	Log of background checks per 100,000 inhabitants					
	Handgun Sale				Total	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post	−0.038					
	(0.038)					
Delay × Post1		−0.177***	−0.202***	−0.149**	−0.082*	−0.000
		(0.060)	(0.064)	(0.063)	(0.042)	(0.066)
Delay × Post2		0.101	0.067	0.018	0.056	0.115
		(0.079)	(0.064)	(0.065)	(0.064)	(0.113)
State FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
State FE×t	N	N	Y	Y	Y	Y
Controls	N	N	N	Y	Y	Y
States	40	40	40	40	40	40
Observations	1,440	1,440	1,440	1,440	1,440	1,440
Mean DV	5.96	5.96	5.96	5.96	6.58	5.76
R^2	0.965	0.968	0.973	0.981	0.986	0.984
$p(\beta_1 = -\beta_2)$		0.32	0.01	0.03	0.75	0.5

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

homicides, it is something ruled out by the rational model.[26] In a regression analysis, we thus investigate whether *Delay* states actually experience comparatively fewer handgun purchases over the entire time period or if this is compensated by higher sales rates later.

Table 2 reports the results from regressing the log of monthly handgun sale background checks per 100,000 inhabitants on $Delay_s \times Post1_t$. As explained in Section 4.1 above, the reported coefficient β_1 is the percentage difference of the sales rate response to the demand shock for *Delay* states compared to *NoDelay* states.[27] In columns 2 to 4 we split the treatment period into two equal halves of six-months and report the p-value from a Wald-test of coefficient equality of β_1 and β_2 to investigate whether gun purchases were simply postponed. Column 1 shows the unadjusted DiD regression estimate for the entire *Post* period which yields a negative but insignificant coefficient. Splitting

[26] The reasoning behind this is that with stable prices and rational forecasting, the consumer is always better off in doing her purchase immediately rather than postponing due to the relatively high utility of gun consumption directly after the shock.

[27] Appendix Table 12 compares the outcomes when reporting the dependent variable both, in logs and in levels. Appendix Table 13 shows regression results when including Connecticut and Michigan, when including all states but only dates without unusual spikes in the data, and when including all available data. The qualitative results remain the same.

the *Post* period into two parts shows that the previous pooled estimate was masking a significant negative effect in the first six months after the Presidential election and a positive non-significant effect in the second period.

When accounting for potentially diverging pre-trends by adding state-specific linear time trends in column 3, the estimate for β_1 increases in magnitude while β_2 is reduced by one third and stays insignificant. This suggests that sales rates in *Delay* states might have possibly been on a steeper upward trajectory than those in *NoDelay* states during the sample period. Adding controls in column 4 decreases both coefficients in size but does not yield any qualitative changes in the results. Our preferred estimate is the most conservative specification in column 4. The results imply that sales rates were about 14% lower in *Delay* states during the first six months than in *NoDelay* states.[28] The $p-$values from the post-estimation Wald tests in the bottom row reject the hypothesis of a pure postponement effect for both of the more rigorous specifications in columns 3 and 4. We interpret this as tentative evidence that despite some anticipatory purchases in *NoDelay* states, firearm purchase delay laws actually did prevent some consumers to buy firearms.

One concern with our preferred estimate could be that pre-trends are non-linear and would thus not be sufficiently captured by the inclusion of state-specific trends. We investigate this possibility using an event-study design in which we allow for monthly treatment effects. Normalization is done using the first period in our sample and the last pre-intervention period to avoid the under-identification problem arising for event studies with unit trends and a binary treatment, following recent work by Borusyak and Jaravel (2017). The results from this regression are depicted in Figure 5 and show no indication of postponed firearm purchases or non-linear pre-trends. In the two years before November 2012, we do not observe a clear pattern of up- or downward trends in our estimation. After the 2012 Presidential election, however, the effect of *Delay* states on handgun sales becomes significantly negative and peaks in size after the shooting at Sandy Hook in December 2012. Starting in March 2013, the coefficients gradually move back to the pre-period level and remain insignificant for the entire *Post2* period.

We provide further robustness checks in the appendix, Figure 12 explores the robustness with respect to the exclusion of specific states from the sample and Figure 13

[28]For coefficients exceeding 0.1 in absolute value, we use the exact formula $exp(\beta) - 1$ in order to calculate the semi-elasticity.

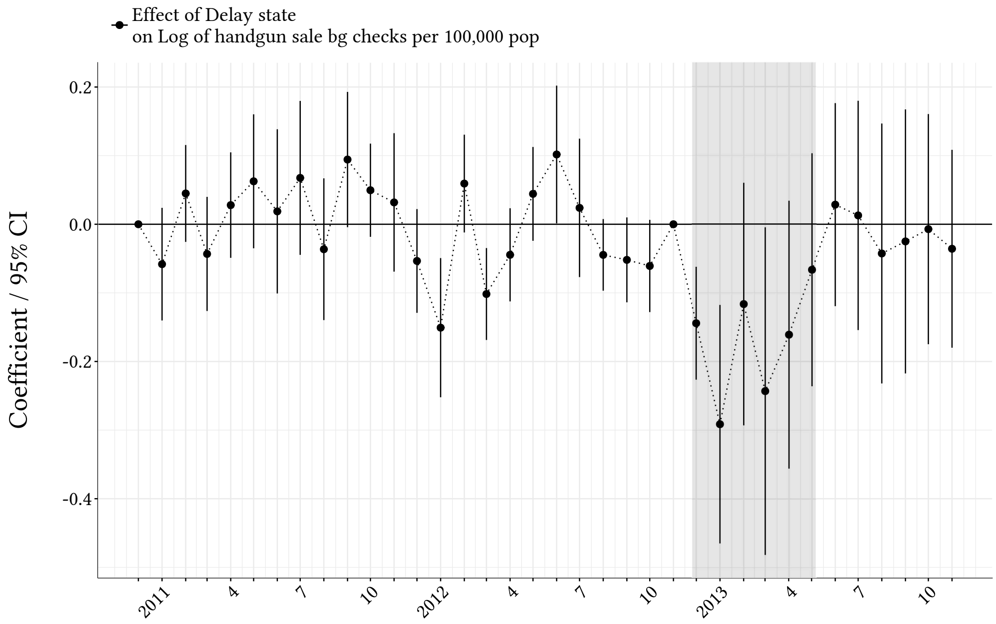

FIGURE 5: EVENT STUDY GRAPH FOR NICS BACKGROUND CHECKS

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on the log of NICS background checks per 100,000 inhabitants for handguns for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

with respect to the sample length. Figure 14 reports results from a permutation test with 10,000 iterations, where we shuffle the assignment of *Delay* and *NoDelay* states. In Table 14 we explore different weighting options and show that our effect seems to predominantly arise from more populated states. Finally, Table 15 explores different time trend specifications, such as quadratic trends, linear and quadratic trends estimated from the pre-event period only as suggested by Wolfers (2006), and seasonality effects.

5.2 Alternative Channels

While the previous regression results appear robust, there could be alternative explanations for why we observe the more moderate spike in *Delay* states. A first legitimate question to ask is whether handgun purchases were actually *prevented* or simply *displaced*. While our regression results have not delivered any indication for a temporal displacement, prospective buyers could have been diverted to secondary markets such as gun shows. Indeed, the majority of states do not require background checks for private, non-commercial transactions. Most transactions at gun shows, however, are presumably carried out by licensed dealers who are legally required to carry out a background check

(Bureau of Alcohol, Tobacco and Firearms, 1999). Nevertheless, we investigate whether the demand for gun shows tilted towards *Delay* states during our treatment period. In order to do this, we use the log of weekly standardized relative "gun show" Google searches as an outcome in our baseline regression equation. The results reported in Table 3 reveal that, if anything, relative demand for gun shows was *falling* in *Delay* states during the gun demand shock.[29] A possible explanation for this finding would be that *NoDelay* states actually experienced a larger demand shift towards secondary markets due to the reported handgun supply shortage. By this logic, displacement would actually understate the true preventive effect of delay laws.[30]

Another alternative explanation for our results is that the reaction to the demand shock was not identical across states. Handgun purchase delay laws as such could just be the result of unobserved heterogeneity in firearm preferences which may also manifest itself in lower handgun sales. Not only would such an omitted variable story undermine the role of these laws but it would also violate the crucial assumption in our model that changes in instrumental utility are uniform. We test this possibility using Google searches for the term "gun store" as a proxy for public interest in buying a gun. Previous research by Scott and Varian (2014) has identified this variable as a good predictor of firearm purchasing intentions. Table 4 repeats the regression specification of Table 3 using the log of standardized relative Google searches for "gun store" as dependent variable.[31]

Column 1 to 3 seem at first to confirm that consumers in *Delay* states indeed reacted differently to those in *NoDelay* states in the aftermath of the shock. Upon inclusion of our controls in column 4, however, these significant differences completely disappear. To phrase this in the language of our model, preferences for firearms ($\mu(\boldsymbol{x_i}) + \gamma_i$) differ across consumers from different states. As long as we condition on all relevant observables, however, observing identical intentions to purchase a firearm implies that we can assume $G(\gamma)$ to be the same across states.[32] Since we do not observe that the inclusion of these controls diminishes the effect on handgun sales in Table 2, but only of

[29]Appendix Table 16 provides qualitatively similar findings in levels instead of logs.

[30]Figure 15 in the appendix graphically depicts the evolution of Google searches. Section D.1 in the appendix provides additional evidence that the supply of gun shows also did not tilt towards *Delay* states. The results qualitatively match the findings for gun show demand.

[31]A regression using levels and producing similar results can be found in Appendix Table 17.

[32]The estimates in column 4 are not driven by the inclusion of any specific variable in our set of controls. Results are available upon request.

Table 3: Google searches for "gun show"

	Log of standardized share of Google searches for "gun show"			
	(1)	(2)	(3)	(4)
Delay × Post	−0.167**			
	(0.066)			
Delay × Post1		−0.093	−0.010	−0.012
		(0.094)	(0.107)	(0.142)
Delay × Post2		−0.236***	−0.119	−0.280*
		(0.058)	(0.106)	(0.156)
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
State FE×t	N	N	Y	Y
Controls	N	N	N	Y
States	49	49	49	49
Observations	7,693	7,693	7,693	7,693
Mean DV	3.9	3.9	3.9	3.9
R^2	0.660	0.660	0.671	0.724

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, % males aged 18-24 and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 4: Google searches for "gun store"

	Log of standardized share of Google searches for "gun store"			
	(1)	(2)	(3)	(4)
Delay × Post	−0.227***			
	(0.087)			
Delay × Post1		−0.278***	−0.235**	0.038
		(0.105)	(0.099)	(0.097)
Delay × Post2		−0.181**	−0.121	−0.038
		(0.083)	(0.111)	(0.113)
State FE	Y	Y	Y	Y
Week FE	Y	Y	Y	Y
State FE×t	N	N	Y	Y
Controls	N	N	N	Y
States	49	49	49	49
Observations	7,693	7,693	7,693	7,693
Mean DV	3.58	3.58	3.58	3.58
R^2	0.607	0.607	0.625	0.677

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, % males aged 18-24 and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

differences in purchasing intentions, we feel confident that our findings are not simply due to unobserved heterogeneity or differences in preferences across *Delay* and *NoDelay* states.[33]

5.3 Model Hypotheses

Having established the significant differential reactions to handgun sales in *Delay* and *NoDelay* states, we can now evaluate whether our findings are more in line with the reaction being driven by rational or by behavioral agents using the predictions generated by our model in Section 2.3.

First, note that the previous section has shown that although we observe a differential reaction in handgun sales, we do not observe a similar reaction in the intention to purchase a firearm (conditional on covariates). This is a clear sign of the presence of time-inconsistent agents. If all consumers were time-consistent, intentions and actions should not diverge, but time inconsistency may make consumers adapt their decisions at later points in time.

Second, our model predicted that the demand shock increases differences in handgun sales (R3 and B3), but for different reasons. In the rational version of the model, consumers in *Delay* states would not react to the shock (R4), while they would if they were suffering from projection bias (B4). Visual inspection of Figure 3 shows that indeed both groups of states seem to react to the demand shock, rendering time inconsistency due to projection bias the more likely alternative. Note, however, that projection bias per se will have a dampening effect on the shock as it makes the decision problem of consumers in *Delay* states more similar to those in *NoDelay* states (which is also what we observe in Figure 3).

Third, prediction B2 states that the demand shock should induce strong differences in gun sales between states with short and without delays if consumers suffer from projection bias and from present bias. Table 5 uses our preferred specification from Table 2, with each new column excluding states with a certain delay length. Columns 1 through 4 show relatively stable and significant coefficients as we gradually reduce the maximum delay length to 10 days, which arguably constitutes a short delay already. Reducing the maximum delay length further to 7 and then to 3 days reduces the coefficient and

[33] Figure 16 in the appendix shows the development of Google searches between November 2010 and December 2013 graphically.

Table 5: Handgun background checks depending on delay length

	Log of handgun sale background checks per 100,000 inhabitants					
Maximum delay length	Baseline	$D \leq 30$	$D \leq 14$	$D \leq 10$	$D \leq 7$	$D \leq 3$
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post1	−0.149**	−0.157**	−0.145**	−0.181**	−0.061	−0.072
	(0.063)	(0.070)	(0.073)	(0.088)	(0.053)	(0.067)
Delay × Post2	0.018	0.018	0.002	0.034	−0.047	−0.064
	(0.065)	(0.069)	(0.067)	(0.078)	(0.055)	(0.051)
State FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
States	40	39	38	36	34	33
Observations	1,440	1,404	1,368	1,296	1,224	1,188
Mean DV	5.96	6.08	6.14	6.16	6.27	6.26
R^2	0.981	0.972	0.962	0.964	0.953	0.952
$p(\beta_1 = -0.149)$		0.91	0.96	0.72	0.1	0.25

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, % males aged 18-24 and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

it loses statistical significance. Due to relatively large standard errors, however, one cannot confidently reject the hypothesis of equality of coefficients between column 1 and all other columns. While this constitutes no conclusive evidence of a reaction in line with present bias, it is also no clear evidence of the contrary. We therefore conclude that our findings suggest that time inconsistency (possibly due to projection bias and present bias) are more likely to drive gun buyers during this gun demand shock than fully rational deliberations.

6 The Effect of Delay Laws on Homicides

6.1 Results

Starting from the observation that handgun sales increased significantly less in *Delay* states during the 2012 firearm demand shock, we investigate if there was also a corresponding effect on homicide rates.[34] Table 6 shows the results from regression equation

[34] Appendix Section D.2 investigates the effect on crime other than murder, providing a test of the "more guns, less crime" hypothesis. Appendix Section D.3 investigates the effect on suicides and accidents.

Table 6: Baseline: homicide rates

	Log of homicides per 100,000 inhabitants					
	Any				Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post	−0.011					
	(0.010)					
Delay × Post1		−0.028**	−0.032**	−0.030*	−0.033***	0.004
		(0.012)	(0.016)	(0.018)	(0.012)	(0.014)
Delay × Post2		0.006	0.001	0.001	−0.013	0.020
		(0.013)	(0.018)	(0.024)	(0.019)	(0.013)
County FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
County FE×t	N	N	Y	Y	Y	Y
Controls	N	N	N	Y	Y	Y
Counties	3050	3050	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800	109,800	109,800
Mean DV	0.36	0.36	0.36	0.36	0.25	0.14
R^2	0.453	0.453	0.470	0.473	0.498	0.177

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

11.[35] Observations are now at the county level, and the sample now includes states which were previously omitted due to measurement error in the background check data.[36] Column 1 shows that *Delay* states experienced an (insignificant) reduction of homicide rates by 1.1% after the start of the firearm demand shock. Column 2 reveals that this effect is concentrated and statistically significant only during our treatment period *Post1*. The estimate for this time period implies a 2.8% reduction and an insignificant effect close to zero for the *Post2* period. Controlling for observables and county-trends in columns 3 and 4 leaves the results qualitatively unchanged. Our preferred specification in column 4 implies a 3.0% drop in *Delay* states' homicide rates during the treatment period. Columns 5 and 6 show that this effect does not reflect a general increase in violent behavior but results entirely from homicides committed with a handgun.

The simultaneity of prevented handgun sales and lower rates of gun-related killings over the six-month period provides strong evidence for a positive relationship between handgun sales and homicides. As firearm purchase delay legislation is often intended

[35] Appendix Table 18 displays regression results in levels instead of logs.

[36] We still exclude Connecticut because of identification concerns, and Michigan because the state switched treatment assignment in the period of observation. Appendix Table 19 shows that results are very similar when restricting the sample to the NICS sample (with or without the months of unusual spikes in the background check data), or expanding the sample to include Connecticut and Michigan.

to provide "cooling-off" periods for angry or upset individuals intending to commit violent acts, our results suggest that there is a positive effect on regular, yet impulsive gun buyers as well. Delays can therefore unfold their positive effects not only through providing time for second thoughts to potential offenders, but also by keeping firearms out of the hands of impulsive individuals who may need a "cooling-off" period in the future.

6.2 Robustness and Sensitivity Checks

In order to ensure that our findings of lower homicide rates in *Delay* states during the period of high firearm demand are not a statistical artefact, we examine their robustness and sensitivity. First, we investigate whether the assumption of common trends is sensible, by checking for non-linear pre-trends using the same event-study design as before. Figure 6 indeed does not show any systematic effect for handgun-induced homicides before the onset of the treatment. Starting in November 2012, however, there is a clearly visible negative impact which becomes weaker and more noisy after May 2013. Figure 7 instead shows no systematic effect on non-handgun homicides before or after the onset of the treatment. If anything, there exists a small upward time trend. Additionally, regressions reported in Appendix Table 20 show results under the assumption of quadratic trends and trends estimated only from the pre-treatment period. Each of these iterations does not qualitatively change our results.

Second, *Delay* states could experience shocks with a negative impact on homicide around the same time as the events we are considering unfold. For instance, our regressions could be picking up different seasonal effects in *Delay* and *NoDelay* states. In columns 4 to 6 of Table 7, we repeat our main regressions but pre-date our sample and treatment periods by one year. The results turn out insignificant and much closer to zero, suggesting that the previously uncovered effect can be attributed to the treatment rather than seasonal variation across groups of states. Relatedly, we consider whether our estimates may reflect diverse reactions to the re-election of President Obama and shift sample and treatment periods backwards by 4 years to the 2008 Presidential election, when gun control was not a focus of then-candidate Barack Obama. Again, columns 7 to 9 of Table 7 do not yield any significant coefficients which would indicate that violent

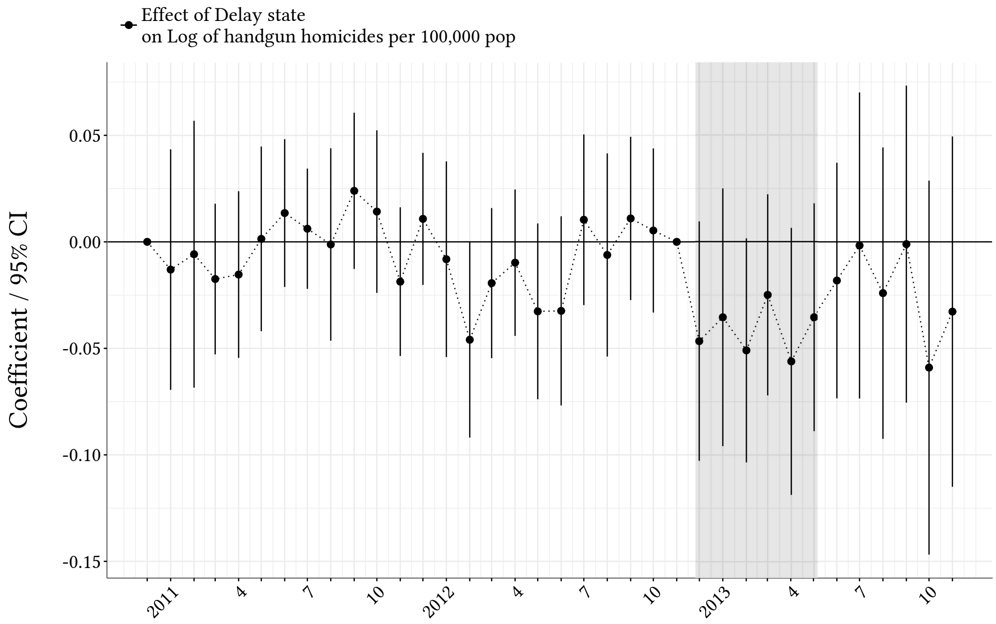

Figure 6: Event study graph for handgun homicide rate

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on the log homicide per 100,000 inhabitants committed with a handgun for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

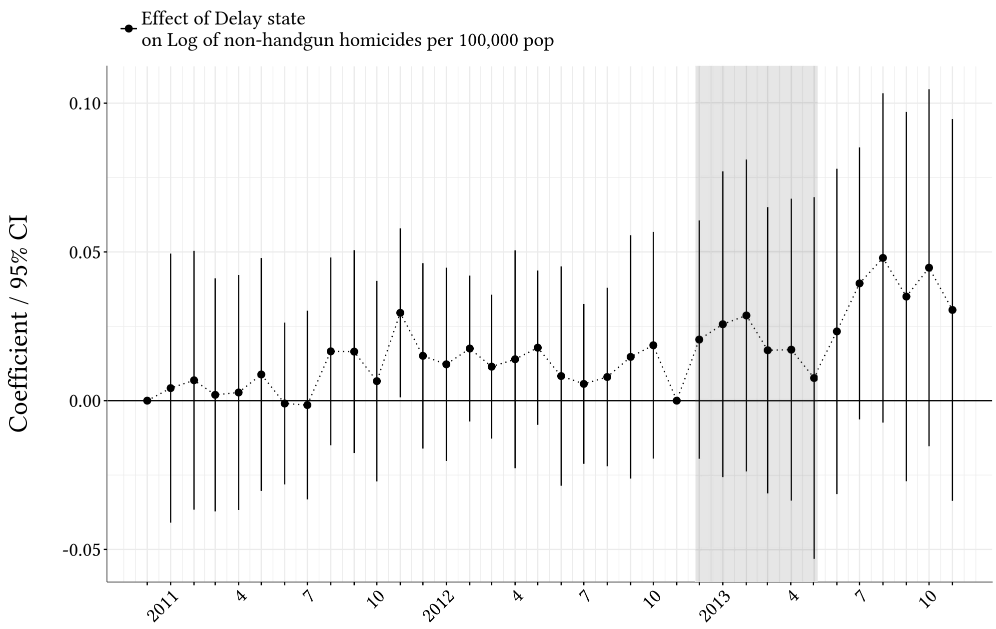

Figure 7: Event study graph for non-handgun homicide rate

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on the log homicide per 100,000 inhabitants committed without a handgun for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

Table 7: Placebo regressions of homicide rates

	Log of homicides per 100,000 inhabitants								
	Baseline (2011-2013)			-1 Year (2010-2012)			Obama I (2007-2009)		
	Any	Handgun	Other	Any	Handgun	Other	Any	Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Delay × Post1	−0.030*	−0.033***	0.004	−0.011	−0.016	0.001	−0.016	−0.010	−0.005
	(0.018)	(0.012)	(0.014)	(0.014)	(0.014)	(0.009)	(0.015)	(0.011)	(0.008)
Delay × Post2	0.001	−0.013	0.020	−0.009	−0.008	−0.006	−0.023	−0.001	−0.020*
	(0.024)	(0.019)	(0.013)	(0.015)	(0.012)	(0.011)	(0.018)	(0.016)	(0.011)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	3050	3050	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800
Mean DV	0.36	0.25	0.14	0.37	0.25	0.14	0.39	0.26	0.15
R^2	0.473	0.498	0.177	0.482	0.503	0.185	0.528	0.552	0.216

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

reactions or racial tensions were driving our homicide results. In sum, both tests provide evidence against correlated shocks driving our results.

Third, we test whether our estimates are a result of our sample choice. We narrow the chosen time window sequentially to check whether we picked a period that is particularly favorable to generate our results. Figure 8 shows how the estimate on handgun homicides for the *Post1* period changes when reducing the sample along the time dimension. The coefficient initially remains stable and then increases as we shorten the sample. Due to increasing standard errors when reducing the sample size, the coefficient becomes insignificant as we shorten the sample to include 6/3 months and beyond. The coefficient estimate, however, always remains above our 24/12 specification. Given these results, we are confident that our results are not simply driven by our chosen baseline time period.

Finally, we address the concern of outliers. In particular, one may be concerned that falsely categorizing states as either instant or delayed, or extreme patterns in homicide within a specific state before and after the events in late 2012 may be driving our baseline results. We therefore perform a series of regressions where single states are omitted from the regression. Figure 9 reports the results from our baseline homicide regression with 95% confidence intervals, removing one state at a time for all states in our sample. The coefficient is of similar magnitude across all regressions and remains significant throughout. Unsurprisingly, the most extreme estimates are obtained from populous states, which generate comparatively strong changes in the composition of the treatment group.

We provide further robustness checks in the Appendix. In Table 21 we remove counties in *Delay* states that border *NoDelay* states and obtain very similar results. Table 22 reports results from a regression at the state level. We obtain very similar coefficient estimates, but the drop in sample size from 109,800 observations to 1,764 observations comes at the cost of reduced statistical power, such that the effect is only significant at the 10% level. Figure 17 shows estimated coefficients from a permutation test with 10,000 iterations, in which we randomly reshuffled each state's designation as either *Delay* or *NoDelay*. We find that only 2.44% of all generated coefficients are more extreme than our estimates.

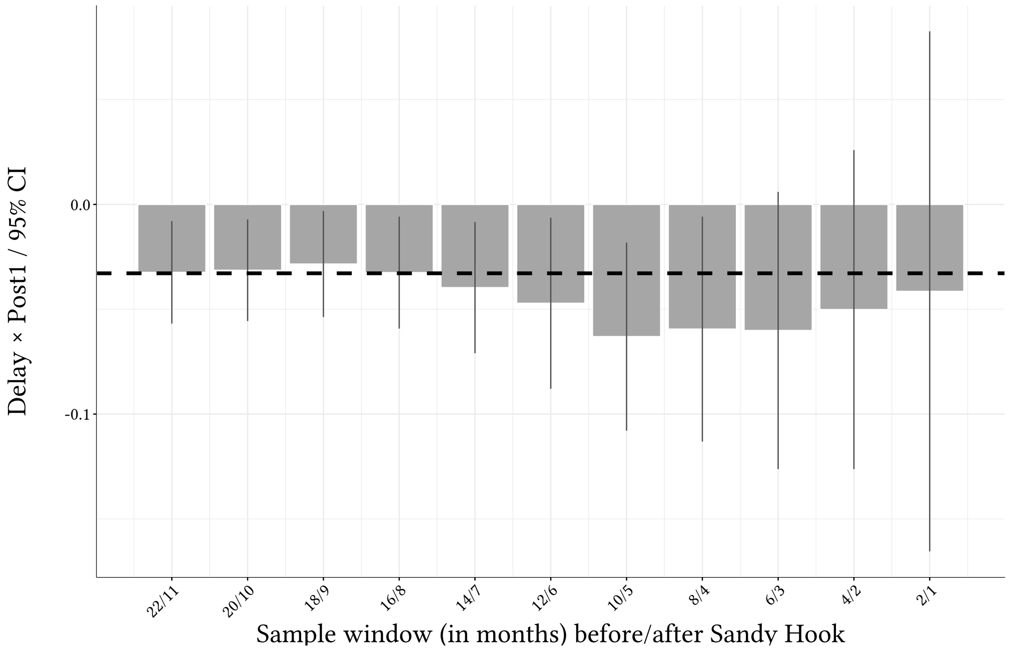

Figure 8: Time window on homicide coefficient

Coefficients for log handgun homicide rate including a decreasing number of months before and after the 2012 election and shooting at Sandy Hook in the regression and corresponding 95% confidence intervals. 22/11 means that 22 months prior to and 11 months after the demand shock are included (=33 months in total), etc. The dashed line indicates our baseline, i.e. the magnitude when including 24 months prior and 12 months after the demand shock.

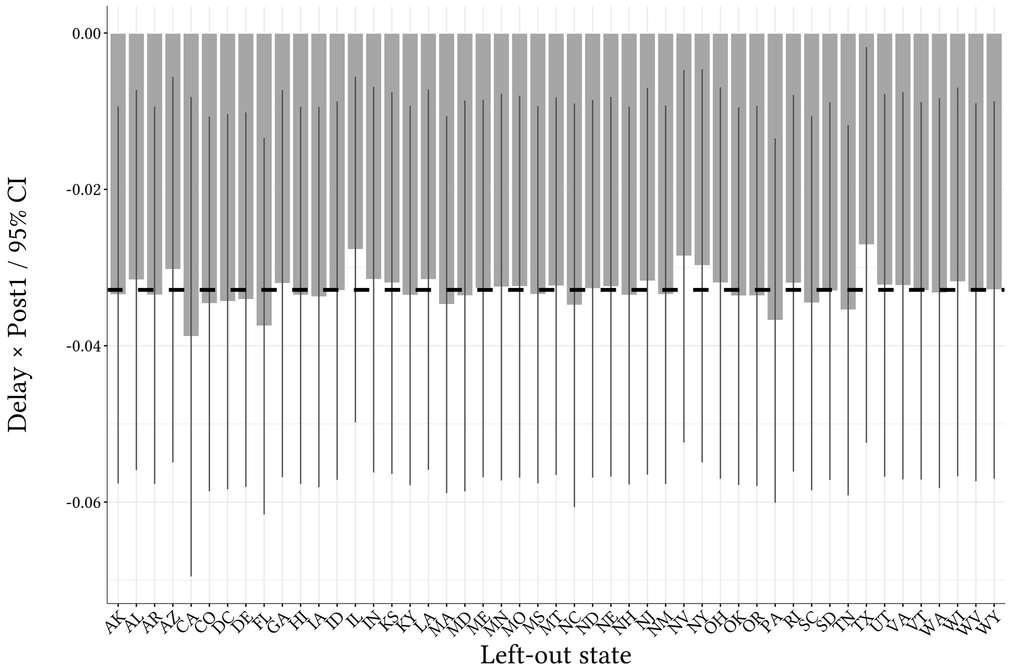

Figure 9: Post1 coefficients on homicide leaving out states

Coefficients for log handgun homicide rate removing a single state (denoted on the x-axis) from the sample and corresponding 95% confidence intervals. The dashed line indicates our baseline, i.e. the magnitude when excluding no states.

Table 8: Effect on homicide rates: victim sex & age

	Log of handgun homicides per 100,000 inhabitants										
Victim sex	Any	Male					Female				
Victim age		Any	< 20	20–39	40–59	≥ 60	Any	< 20	20–39	40–59	≥ 60
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Delay × Post1	−0.033***	−0.024**	0.000	−0.016	−0.009*	−0.001	−0.008*	0.003	−0.005	−0.005*	−0.002
	(0.012)	(0.012)	(0.005)	(0.010)	(0.005)	(0.002)	(0.005)	(0.002)	(0.003)	(0.003)	(0.002)
Delay × Post2	−0.013	−0.015	−0.005	−0.002	−0.008	−0.000	0.004	0.004	0.004	−0.003	−0.001
	(0.019)	(0.016)	(0.005)	(0.012)	(0.008)	(0.003)	(0.007)	(0.003)	(0.004)	(0.005)	(0.002)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	3050	3050	3050	3050	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800
Mean DV	0.25	0.21	0.03	0.14	0.04	0.01	0.04	0.01	0.02	0.01	0
R^2	0.498	0.521	0.263	0.489	0.157	0.069	0.109	0.077	0.102	0.068	0.062

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

Table 9: Effect on homicide rates: victim race

	Log of handgun homicides per 100,000 inhabitants					
Victim race	Any	White	Black	Hispanic	Asian	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post1	−0.033***	−0.006	−0.016	−0.007	−0.001	−0.003**
	(0.012)	(0.005)	(0.012)	(0.004)	(0.002)	(0.001)
Delay × Post2	−0.013	−0.003	−0.004	−0.001	−0.004	−0.004**
	(0.019)	(0.008)	(0.011)	(0.007)	(0.003)	(0.002)
County FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800	109,800	109,800
Mean DV	0.25	0.06	0.14	0.05	0	0
R^2	0.498	0.103	0.626	0.368	0.115	0.121

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

6.3 Prevented Homicides: Demographics

Our previous analyses have not yet been able to identify the exact channel through which the comparative decrease in handgun sales led to fewer homicides. In this and the next section we try to uncover these channels by taking a closer look at the type of additional handgun homicides in *NoDelay* states (or equivalently which were "prevented" in *Delay* states). In a first step, we make use of the demographic information on victims provided in the NVSS data.

Table 8 starts with our baseline estimate of the effect on handgun homicides in column 5 of Table 6 and then splits incidents up by victims' gender and broad age groups. Columns 2 and 7 show that the baseline effect is mainly driven by homicides with male victims which fell by 2.4%. However, women also saw a significant decrease of about 0.8%. Upon looking at individual age categories, the age groups below 20 and above 60 do not seem to respond at all while large, yet insignificant, effects come from the 20 to 39-year olds in specifications 4 and 9. The only significant (and also sizable) estimates come from victims aged 40 to 59 for both women and men in columns 5 and 10.

Next, we split up the homicide rates by race of victim with results displayed in Table 9. The drops in the *"White"* and *"Black"* categories are sizable but lack precision. So does the coefficient for *"Hispanics"* with a p−value of 11%, while *"Asian"* has a response close to zero. The only category yielding a significant coefficient is *"Other"* in column 6 which essentially comprises of American Indians and Pacific Islanders. This group, however, seems to follow a somewhat different pattern since the effect persists into the *Post2* period and even slightly increases. Overall, the "prevented" murders do not appear to be concentrated within a specific race but appear to be more or less evenly distributed across several ethnicities.

6.4 Prevented Homicides: Circumstances

As the previous section has shown, the victims of the additional (in *NoDelay* states) or "prevented" (in *Delay* states) homicides can be either male or female, fall within an age range of 20 to 59 and are not concentrated within one or two specific racial groups. Many victims being between 40 to 59 years old points towards circumstances outside the typical nexus of professional or organized crime. As the additional gun buyers in *NoDelay* states are furthermore likely to behave time-inconsistently (or impulsively), we therefore investigate the possibility that domestic violence may play a role. We split the handgun homicide victims into those who were shot in their home and those who were assaulted elsewhere. Table 10 reports the corresponding results. For the male victims we find that the entire effect is driven by attacks outside their home. Female victims, on the other hand, are predominantly assaulted in their place of living.

In Table 11, we present the results from the UCR SHR data on the particular circumstances of a homicide.[37] Columns 1 to 3 show the baseline specification for all homicides by gender and then split up the male and female victims into specific murder circumstances. As the mean values for the *"Other"* category indicate, a large part of homicides cannot be accurately categorized. Nevertheless, the results in columns 4 and 5 indicate that assaults related to arguments account for almost half of the additional homicides in *NoDelay* states, for both males and females, although only the coefficient for males is statistically significant. Some male victims also die from gang- and felony-related circumstances, but this does not hold for female victims. This suggests that

[37] As outlined in Sections 3.2 and D.2, this data exhibits a more restricted coverage. Appendix Table 23 shows that the UCR SHR data yield qualitatively very similar estimates compared to the NVSS data.

Table 10: Effect on homicide rates: place of assault

	Log of handgun homicides per 100,000 inhabitants						
Victim sex	Any	Male			Female		
Place of assault	Any	Any	Home	Not Home	Any	Home	Not Home
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Delay × Post1	−0.033***	−0.024**	−0.001	−0.023*	−0.008*	−0.005*	−0.003
	(0.012)	(0.012)	(0.006)	(0.014)	(0.005)	(0.003)	(0.003)
Delay × Post2	−0.013	−0.015	−0.012	−0.005	0.004	0.003	0.001
	(0.019)	(0.016)	(0.007)	(0.014)	(0.007)	(0.006)	(0.003)
County FE	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800	109,800	109,800	109,800
Mean DV	0.25	0.21	0.08	0.14	0.04	0.02	0.02
R^2	0.498	0.521	0.254	0.523	0.109	0.085	0.102

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

the effect on female victims assaulted at home is probably unrelated to violence from outsiders such as armed burglars. Almost 20% of the overall male and 50% of the overall female effect come from homicides with undetermined backgrounds. Brawls and defense-related homicides in columns 6 to 7 and 12 to 13 do not seem to contribute.

Summarizing the findings from this and the previous section, we observe that the additional homicides of females in *NoDelay* states primarily happened inside their home, predominantly to women between 20 and 59, and often as a result of arguments. Homicides of men, instead, happened primarily outside their home, largely because of arguments or gang-related crime, and to some extent in conjunction with other felonies. Similar to women, male victims are typically 20-59 years old. These findings suggest domestic violence as a possible explanation for many of the female homicides, and a mixture of criminal and *heat of the moment* murders for the male homicides. While they do not constitute definitive proof, these interpretations are well in line with insights from psychology. According to Tangney, Baumeister, and Boone (2004), impulsiveness is correlated across domains. As we have shown time-inconsistent behavior as a likely driver for firearm purchases in the wake of the demand shock, it would be conceivable

Table 11: Murder Reports: circumstances

	Log of handgun murders per 100,000 inhabitants														
	Any			Arguments		Brawls		Gang-related		Felony		Defense		All Other	
	Any	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Delay × Post1	−0.030**	−0.022*	−0.008*	−0.010**	−0.003	0.002	−0.000	−0.008	−0.000	−0.004	−0.000	−0.001	0.000	−0.004	−0.004
	(0.013)	(0.013)	(0.005)	(0.005)	(0.003)	(0.001)	(0.000)	(0.010)	(0.001)	(0.005)	(0.002)	(0.004)	(0.000)	(0.008)	(0.003)
Delay × Post2	−0.027	−0.025*	−0.001	−0.012*	−0.002	0.000	−0.000	−0.009	−0.000	−0.015	−0.001	−0.000	0.000	0.008	0.003
	(0.017)	(0.015)	(0.008)	(0.007)	(0.004)	(0.001)	(0.000)	(0.008)	(0.001)	(0.010)	(0.003)	(0.006)	(0.001)	(0.014)	(0.003)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2232	2232	2232	2232	2232	2232	2232	2232	2232	2232	2232	2232	2232	2232	2232
Observations	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352
Mean DV	0.17	0.15	0.03	0.03	0.01	0	0	0.02	0	0.03	0	0.01	0	0.07	0.01
R^2	0.535	0.558	0.118	0.237	0.082	0.099	0.064	0.642	0.176	0.342	0.098	0.172	0.069	0.481	0.106

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

that a new gun owner's impulsiveness carries over to possibly committing impulsive acts of violence.

7 Conclusion

In light of the persistently high rate of firearm homicides in the United States, understanding the consequences of legislation limiting access to guns is imperative. One of the main arguments used by proponents of *gun rights* are that gun laws do not substantially affect violent crime but impose excessive burdens on law-abiding gun owners. In this study we focus on the effects of a specific type of policy measure, handgun purchase delay laws, and provide evidence that, while not infringing with Second Amendment rights, these laws can reduce homicides substantially.

We present empirical evidence that states with delay laws saw comparatively smaller changes in gun ownership during a demand shock after the re-election of President Obama in 2012 and the shooting at Sandy Hook Elementary School. Applying our findings to a simple model provides evidence against an entirely rational explanation of consumer behavior. Further results show that purchase delays did not affect intentions to buy a firearm but only reduced the likelihood of consumers making an actual handgun purchase. This insight guides our second part of the analysis, where we investigate delay laws' effect on homicide rates during the period of the demand shock.

Using detailed micro-data on mortality, we find a significant effect of delay laws on handgun-related homicides during the period of the demand shock. The effect size is about 3.3% which in turn implies that about 280 homicides could have been "prevented" during the six-month period if all U.S. states had had some sort of purchase delay law in place. The effect is robust to the inclusion of controls and a variety of alternative specifications, and does not seem to be caused by pre-existing time trends. Additional data sources allow us to look into the types of homicides that occurred in states without delay laws. We find that these additional homicides encompass both genders, and that arguments as well as domestic violence constitute some of the main channels through which handgun ownership may affect homicide rates.

We see our study as a good starting point for further investigations into issues concerning gun ownership and crime. First, additional *direct* evidence on the circumstances under which gun ownership leads to relatively increased violent crime is needed. While our results were able to point in the direction of arguments and domestic violence, the results are far from clear-cut. With increasing coverage of the FBI's National Incident-Based Reporting System (*NIBRS*), more detailed information on particular crime incidents could be utilized to study similar future firearm demand shocks. Second, given the absence of accurate data on how county-level gun ownership evolves over time, our study cannot pin down an exact gun-homicide elasticity. The background check data is furthermore very noisy and makes cross-state comparison impossible at times. We therefore stress the need for a more transparent, county-level version of handgun sales than what is currently available. Third, we believe that more research is needed to evaluate costs and benefits of specific gun laws. As shown in this study, the positive effects of certain purchase delays in specific states may be understated. Rigorous analyses of gun laws may therefore help foster a more informed debate on gun policy. Finally, we would like to stress the importance of incorporating behavioral biases and cognitive limitations when studying the behavior of gun owners. Future research should explicitly take deviations from perfectly rational agents into account when modeling the purchase, storage and use of firearms, be it by criminals or law-abiding citizens.

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A Theoretical Derivations

The purchasing probability of a behavioral agent in period 0 as stated in equation 7 can be rewritten as:

$$\begin{aligned} P[Buy_{i0s}] &= P\Big[U_{i0s} - p_s - c_s > 0 \cap \tilde{U}_{i0s} - p_s - c_s > \tilde{U}_{i1s} - \beta_i\delta_i p_s - \beta_i\delta_i c_s\Big] \\ &= P\Big[\gamma_i > \tilde{\tilde{\gamma}}_{i0s} \cap \gamma_i > \tilde{\gamma}_{i0s}\Big] \\ &= \Big(1 - P\Big[\tilde{\gamma}_{i0s} > \tilde{\tilde{\gamma}}_{i0s}\Big]\Big) \times P\Big[\gamma_i > \tilde{\tilde{\gamma}}_{i0s}\Big] + P\Big[\tilde{\gamma}_{i0s} > \tilde{\tilde{\gamma}}_{i0s}\Big] \times P\Big[\gamma_i > \tilde{\gamma}_{i0s}\Big] \end{aligned} \tag{12}$$

Both threshold levels $\tilde{\gamma}_{i0s}$ and $\tilde{\tilde{\gamma}}_{i0s}$ are determined by parameters of the model as well as expectations and current realisations of the preference shifter ω_t. In order to check the plausibility of $\tilde{\gamma}_{i0s} > \tilde{\tilde{\gamma}}_{i0s}$ using specific parameter values, we first substitute in the components of both thresholds, using the assumption that $\Delta\tilde{\Psi}_{i0s} > 0$, and simplify:

$$\begin{aligned} \tilde{\gamma}_{i0s} &> \tilde{\tilde{\gamma}}_{i0s} \\ \frac{(1-\beta_i\delta_i)(p_s + c_s)}{\Delta\tilde{\Psi}_{i0s}} - \mu(\boldsymbol{x_i}) - \frac{p_s + c_s}{\tilde{\Psi}_{i0s}} + \mu(\boldsymbol{x_i}) &> 0 \\ (1-\beta_i\delta_i)\tilde{\Psi}_{i0s} - \Delta\tilde{\Psi}_{i0s} &> 0 \\ (1-\alpha_i)\Big[(1-\beta_i\delta_i)\bar{\Psi}_{i0s} - \Delta\bar{\Psi}_{i0s}\Big] + \alpha_i\Big[(1-\beta_i\delta_i)\tilde{m}_{i0s} - \Delta\tilde{m}_{i0s}\Big] &> 0 \end{aligned} \tag{13}$$

The inequality can thus be expressed as the average of two components weighted by the degree of projection bias α_i. In essence, each unweighted component is measuring whether the loss from postponing the current temporal utility exceeds the actual difference in temporal utility from consuming in the next period. The two unweighted components represent the extreme cases of no or complete projection bias. Assuming $0 < \alpha_i < 1$, the above inequality can be proven true by showing that both components of the weighted sum are positive, which we show in the following.

For the first requirement, one can substitute in from equation 7 and simplify as follows:

$$
\begin{aligned}
(1-\beta_i\delta_i)\bar{\Psi}_{i0s} &> \Delta\bar{\Psi}_{i0s} \qquad (14)\\
(1-\beta_i\delta_i)\sum_{t=D_s}^{D_s+T}\beta_i^{\mathbb{1}(t>0)}\delta_i^t\mathbb{E}_{H_t}[\omega_t] &> \delta_i^{D_s}\Big[\beta_i^{\mathbb{1}(D_s>0)}\mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \beta_i\delta_i^{T+1}\mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}]\Big]\\
(1-\beta_i\delta_i)\sum_{t=D_s+1}^{D_s+T}\beta_i^{\mathbb{1}(t>0)}\delta_i^t\mathbb{E}_{H_t}[\omega_t] &> \beta_i\beta_i^{\mathbb{1}(D_s>0)}\delta_i^{D_s+1}\mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \beta_i\delta_i^{D_s+T+1}\mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}]\\
(1-\beta_i\delta_i)\sum_{t=0}^{T-1}\beta_i^{\mathbb{1}(t>0)}\delta_i^t\mathbb{E}_{H_{t+D_s+1}}[\omega_{t+D_s+1}] &> \beta_i^{\mathbb{1}(D_s>0)}\mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \delta_i^T\mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}]\\
\mathbb{E}_{H_{D_s}}[\omega_{D_s}] &< \frac{(1-\beta_i\delta_i)\sum_{t=0}^{T-1}\beta_i^{\mathbb{1}(t>0)}\delta_i^t\mathbb{E}_{H_{t+D_s+1}}[\omega_{t+D_s+1}] + \delta_i^T\mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}]}{\beta_i^{\mathbb{1}(D_s>0)}}
\end{aligned}
$$

Whether the above equation is satisfied cannot be evaluated without making further assumptions. Aside from the values for β, δ and T, the main challenge is that the distribution and expected values of ω_t are unknown. In order to illustrate that the inequality is likely to hold, we make the following assumptions: First, we set $\beta_i = 0.9$ and $\delta_i = 0.9997$ such that annual discounting amounts to 0.9 and finally assume $T = 3650$, i.e. an expected gun lifetime of 10 years. For the expectations regarding the demand shifter, we focus on how abnormal $\mathbb{E}_{H_{D_s}}[\omega_{D_s}]$ (or ω_0 for $D_s = 0$) needs to be in order to negate the inequality. We therefore make the simplifying assumption that $\mathbb{E}_{H_t}[\omega_t] = \kappa$ for all $t > D_s$. Using κ, re-arranging and inserting the values for the remaining parameters yields results as follows:

$$
\begin{aligned}
\frac{\mathbb{E}_{H_{D_s}}[\omega_{D_s}]}{\kappa} &< \frac{(1-\beta_i\delta_i)\frac{1-\delta^T}{1-\delta} + \delta_i^T}{\beta_i^{\mathbb{1}(D_s>0)}} \qquad (15)\\
\frac{\omega_0}{\kappa} &< 222.77 \qquad \textit{for } D_s = 0\\
\frac{\mathbb{E}_{H_{D_s}}[\omega_{D_s}]}{\kappa} &< 247.52 \qquad \textit{for } D_s > 0
\end{aligned}
$$

Under the assumptions made above, the lifetime utility constraint is only binding if the (expected) temporal utility at the point of receiving the handgun is more than 200 times larger than its baseline level. Despite the severe shock to gun demand during our treatment period, we think that such a scenario is highly unlikely. We proceed in the same fashion to evaluate the second requirement:

$$
\begin{aligned}
(1-\beta_i\delta_i)\tilde{m}_{i0s} &> \Delta\tilde{m}_{i0s} \qquad (16)\\
(1-\beta_i\delta_i)\delta_i^{D_s}\Big[\beta_i^{\mathbb{1}(D_s>0)}+\beta_i\delta_i\frac{1-\delta_i^T}{1-\delta_i}\Big]\omega_0 &> \delta_i^{D_s}\Big[\beta_i^{\mathbb{1}(D_s>0)}-\beta_i\delta_i^{T+1}\Big]\omega_0\\
(1-\beta_i\delta_i)\Big[\beta_i^{\mathbb{1}(D_s>0)}+\beta_i\delta_i\frac{1-\delta_i^T}{1-\delta_i}\Big] &> \beta_i^{\mathbb{1}(D_s>0)}-\beta_i\delta_i^{T+1}\\
(1-\beta_i\delta_i)\beta_i\delta_i\frac{1-\delta_i^T}{1-\delta_i} &> \beta_i\delta_i\beta_i^{\mathbb{1}(D_s>0)}-\beta_i\delta_i^{T+1}\\
(1-\beta_i\delta_i)\frac{1-\delta_i^T}{1-\delta_i}+\delta_i^T-\beta_i^{\mathbb{1}(D_s>0)} &> 0
\end{aligned}
$$

Under full projection bias, future expectations are fully substituted by current experiences which also cancel out. One therefore does not need to make assumptions about ω_t and can just insert the values for β_i, δ_i and T assumed above:

$$
\begin{aligned}
(1-\beta_i\delta_i)\frac{1-\delta_i^T}{1-\delta_i}+\delta_i^T-\beta_i^{\mathbb{1}(D_s>0)} &> 0 \qquad (17)\\
221.77 &> 0 \qquad \textit{for } D_s=0\\
221.87 &> 0 \qquad \textit{for } D_s>0
\end{aligned}
$$

The evidence for the case of full projection bias lends even stronger support to the inequality in equation 13 being true. We therefore conclude that $P\Big[\tilde{\gamma}_{i0s}>\tilde{\tilde{\gamma}}_{i0s}\Big]\approx 1$ is a reasonable assumption in our context.

B Figures

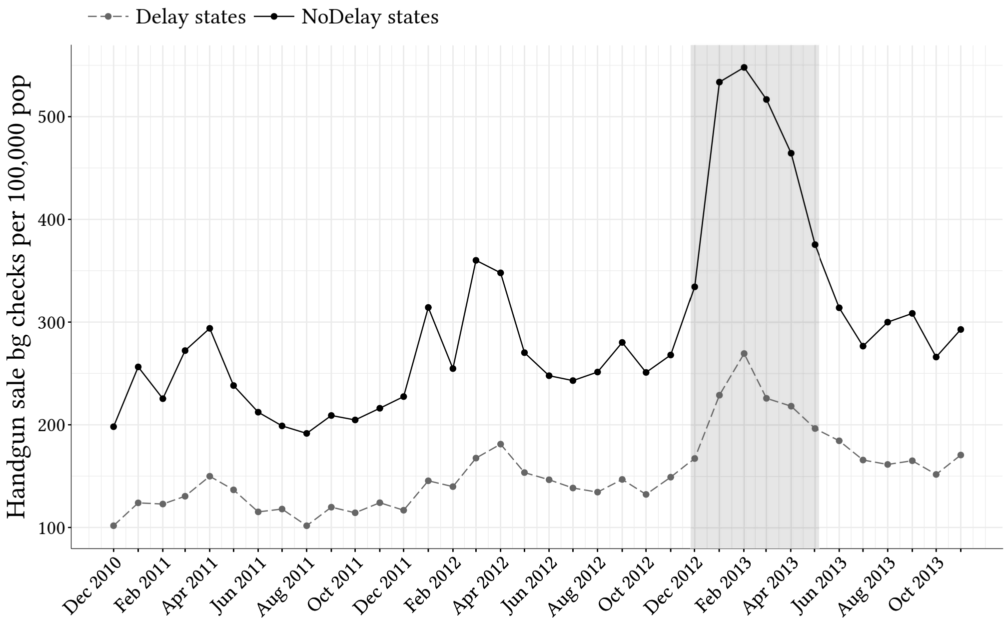

Figure 10: Background checks for handguns in *Delay* vs *NoDelay* states (levels)

Monthly NICS background checks per 100,000 inhabitants for handguns in *Delay* states and *NoDelay* states between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

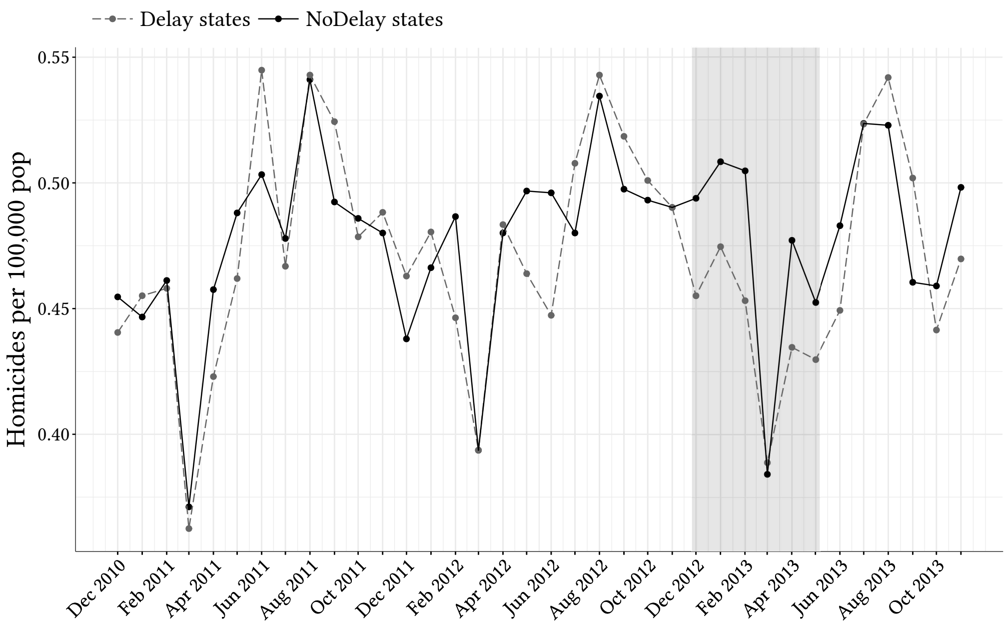

Figure 11: Homicide rate in *Delay* vs *NoDelay* states (levels)

Monthly homicides per 100,000 inhabitants in *Delay* states and *NoDelay* states between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

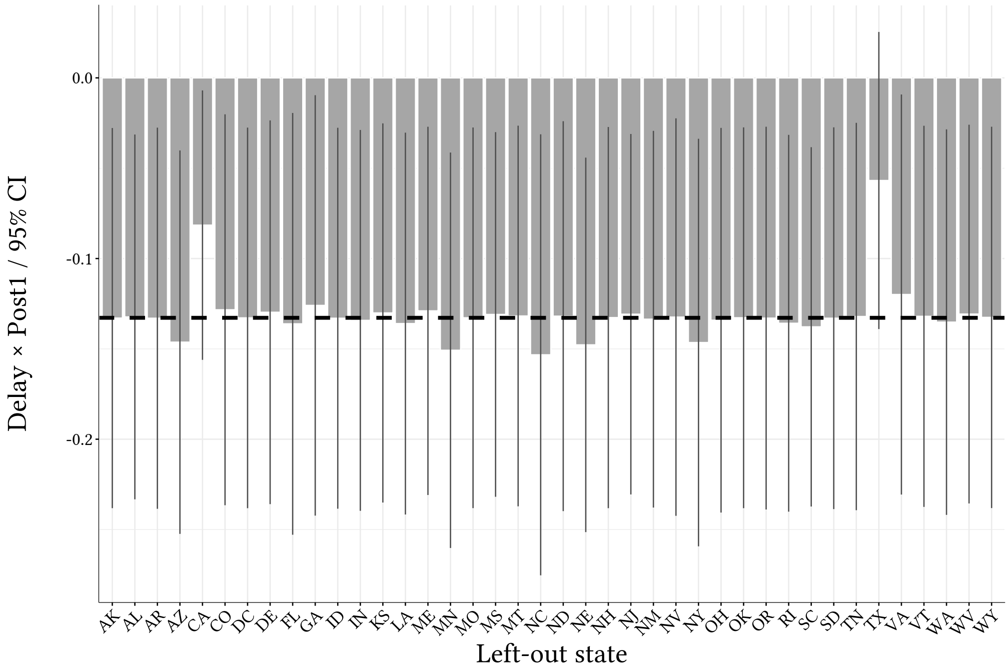

FIGURE 12: POST1 COEFFICIENTS FOR BACKGROUND CHECKS LEAVING OUT STATES

Coefficients for log handgun sale background check rate removing a single state (denoted on the x-axis) from the sample and corresponding 95% confidence intervals. The dashed line indicates our baseline, i.e. the magnitude when excluding no states.

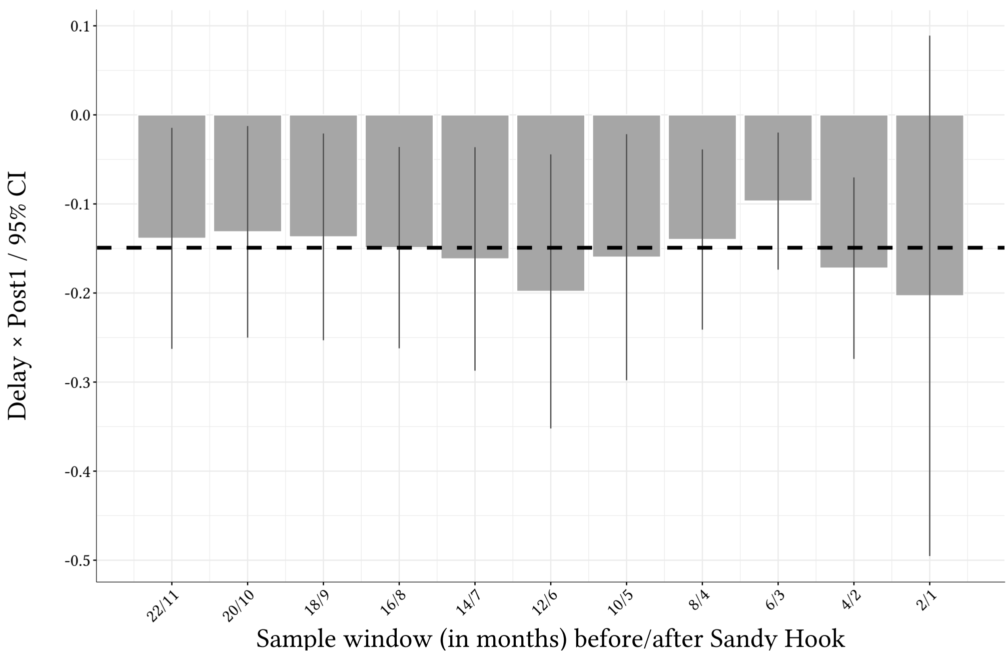

FIGURE 13: TIME WINDOW FOR BACKGROUND CHECK COEFFICIENT

Coefficients for log handgun sale background check rate including a decreasing number of months before and after the 2012 election and shooting at Sandy Hook in the regression and corresponding 95% confidence intervals. 22/11 means that 22 months prior to and 11 months after the demand shock are included (=33 months in total), etc. The dashed line indicates our baseline, i.e. the magnitude when including 24 months prior and 12 months after the demand shock.

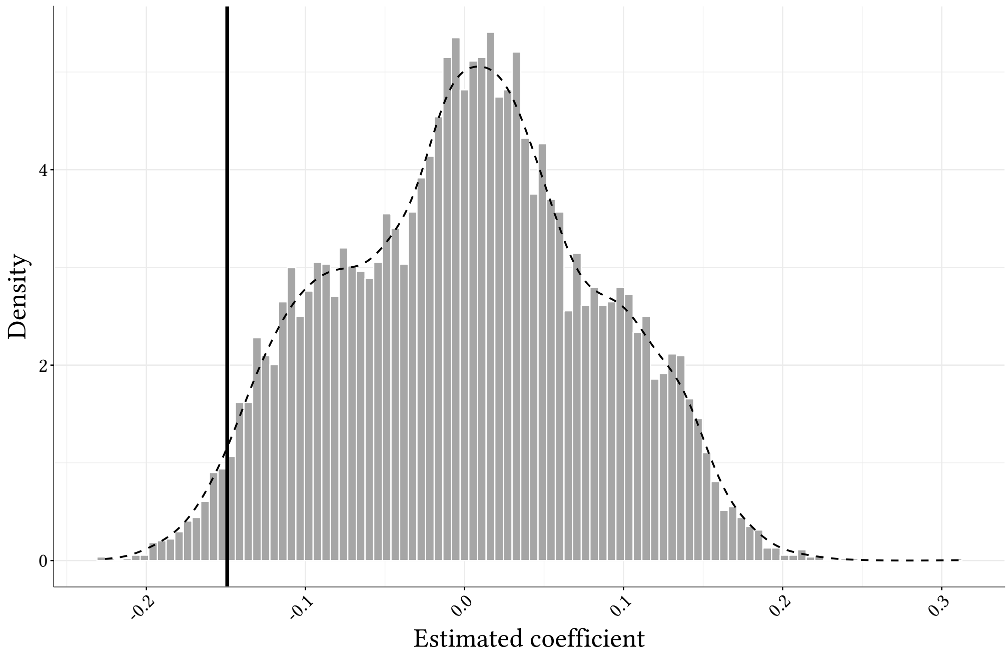

Figure 14: Permutation test for background checks

Density of coefficients of 10,000 regressions as in Table 2, column 4, randomly assigning each state to *Delay* or *NoDelay* in each iteration, while keeping the overall number of *Delay* states constant. The solid vertical line indicates our baseline estimate. The dashed line depicts a kernel density estimate of the coefficients. Only 2.4% of the coefficients are more extreme than our baseline estimates.

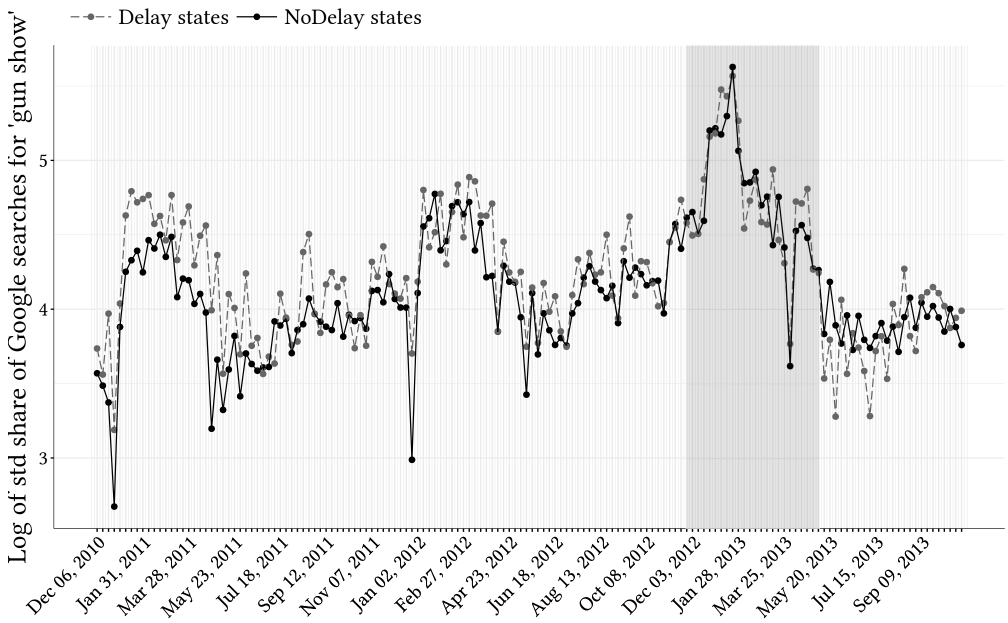

Figure 15: Log of Google searches for "gun show" in *Delay* vs *NoDelay* states

Log of weekly averages of daily normalized Google searches for the expression "gun show" in *Delay* states and *NoDelay* states between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

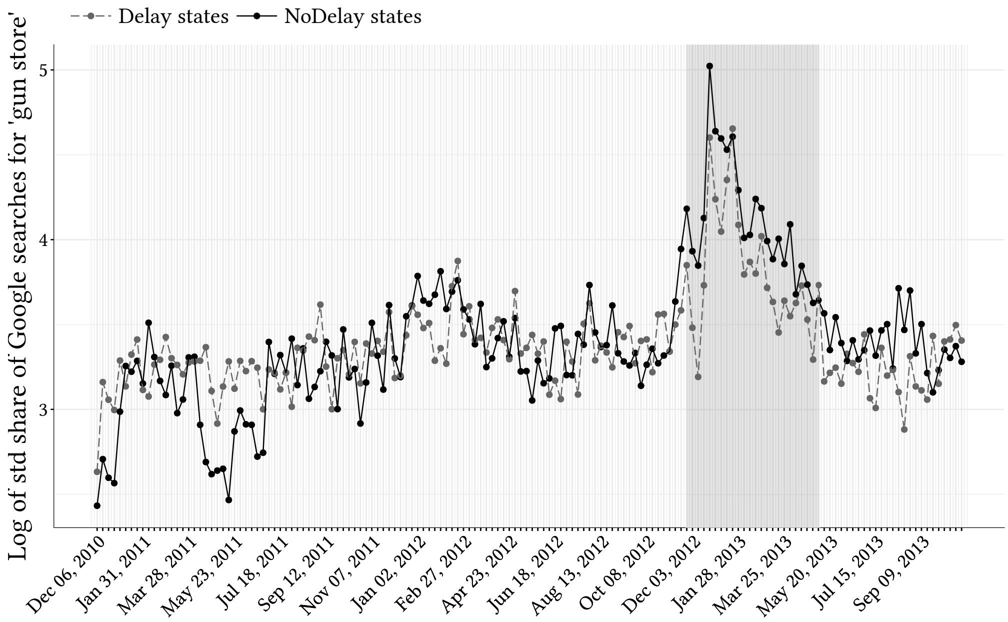

Figure 16: Log of Google searches for "gun store" in *Delay* vs *NoDelay* states

Log of weekly averages of daily normalized Google searches for the expression "gun store" in *Delay* states and *NoDelay* states between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

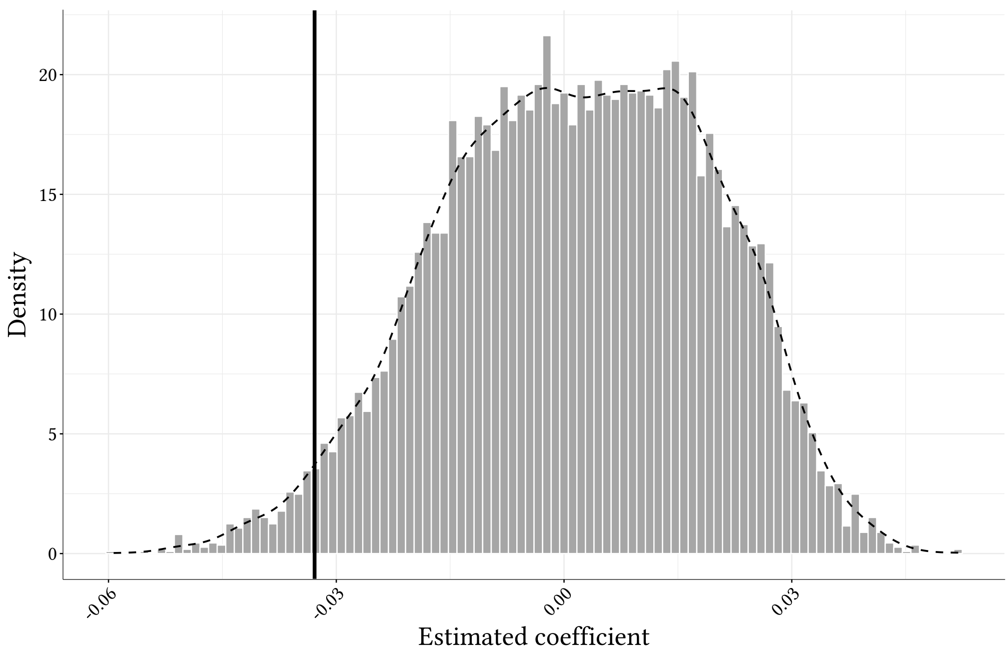

Figure 17: Permutation test for handgun homicides

Density of coefficients of 10,000 regressions as in Table 6, column 5, randomly assigning each state to *Delay* or *NoDelay* in each iteration, while keeping the overall number of *Delay* states constant. The solid vertical line indicates our baseline estimate. The dashed line depicts a kernel density estimate of the coefficients. Only 2.44% of the coefficients are more extreme than our baseline estimates.

C Tables

Table 12: Handgun background checks (levels)

	Handgun sale background checks per 100,000 inhabitants					
	Logs (Baseline)			Levels		
	Total	HandgunSale	Other	Total	HandgunSale	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post1	−0.149**	−0.082*	−0.000	−77.512***	−111.951***	−34.439*
	(0.063)	(0.042)	(0.066)	(26.542)	(32.955)	(19.754)
Delay × Post2	0.018	0.056	0.115	1.538	20.544	19.006
	(0.065)	(0.064)	(0.113)	(18.595)	(25.156)	(18.343)
State FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
States	40	40	40	40	40	40
Observations	1,440	1,440	1,440	1,440	1,440	1,440
Mean DV	5.96	6.58	5.76	233.56	429.64	196.08
R^2	0.981	0.986	0.984	0.943	0.966	0.962

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 13: Handgun background checks (varying the sample)

	Log of handgun sale background checks per 100,000 inhabitants											
	Baseline			Incl. CT & MI			Incl NICS outlier data			Incl all		
	Any	Handgun	Other	Any	Handgun	Other	Any	Handgun	Other	Any	Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Delay × Post1	−0.149**	−0.082*	−0.000	−0.127**	−0.070*	0.002	−0.123**	−0.087**	−0.028	−0.528*	−0.091	0.027
	(0.063)	(0.042)	(0.066)	(0.058)	(0.037)	(0.055)	(0.061)	(0.043)	(0.060)	(0.321)	(0.056)	(0.060)
Delay × Post2	0.018	0.056	0.115	0.043	0.062	0.084	−0.065	−0.018	0.085	−0.385	−0.020	0.111
	(0.065)	(0.064)	(0.113)	(0.065)	(0.054)	(0.093)	(0.094)	(0.075)	(0.092)	(0.289)	(0.090)	(0.082)
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
States	40	40	40	42	42	42	49	49	49	51	51	51
Observations	1,440	1,440	1,440	1,512	1,512	1,512	1,715	1,715	1,715	1,836	1,836	1,836
Mean DV	5.96	6.58	5.76	5.97	6.58	5.75	5.92	6.63	5.84	5.84	6.65	5.85
R^2	0.981	0.986	0.984	0.981	0.985	0.983	0.980	0.980	0.982	0.947	0.966	0.980

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 14: Handgun background checks (different weights)

	Log of handgun sale background checks per 100,000 inhabitants								
Weights	Population			Adult population			None		
	Handgun	All	Other	Handgun	All	Other	Handgun	All	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Delay × Post1	−0.149**	−0.082*	−0.000	−0.147**	−0.081*	−0.002	−0.070	−0.060	−0.062
	(0.063)	(0.042)	(0.066)	(0.063)	(0.043)	(0.067)	(0.079)	(0.071)	(0.072)
Delay × Post2	0.018	0.056	0.115	0.016	0.055	0.115	−0.057	−0.033	0.039
	(0.065)	(0.064)	(0.113)	(0.065)	(0.065)	(0.115)	(0.064)	(0.056)	(0.076)
State FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
States	40	40	40	40	40	40	40	40	40
Observations	1,440	1,440	1,440	1,440	1,440	1,440	1,440	1,440	1,440
Mean DV	5.96	6.58	5.76	5.95	6.58	5.76	6.1	6.76	5.98
R^2	0.981	0.986	0.984	0.982	0.986	0.984	0.984	0.990	0.992

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Weights change according to the specification.

Table 15: Handgun background checks (trend specifications)

	Log of handgun sale background checks per 100,000 inhabitants					
Trend model	Trends from full sample		Trends from pre-period		Seasonal patterns by location	
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post1	−0.149**	−0.143**	−0.147**	−0.114*	−0.092	−0.076
	(0.063)	(0.058)	(0.063)	(0.066)	(0.057)	(0.072)
Delay × Post2	0.018	0.032	0.021	0.093	0.065	0.049
	(0.065)	(0.071)	(0.065)	(0.105)	(0.063)	(0.077)
State FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y	N	N
State FE×t^2	N	Y	N	Y	N	N
Region×Month FE	N	N	N	N	Y	N
State×Month FE	N	N	N	N	N	Y
Controls	Y	Y	Y	Y	Y	Y
States	40	40	40	40	40	40
Observations	1,440	1,440	1,440	1,440	1,440	1,440
Mean DV	5.96	5.96	5.96	5.96	5.96	5.96
R^2	0.981	0.984	0.814	0.796	0.982	0.987

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 16: Google searches for “gun show” (levels)

	Standardized share of Google searches for “gun show”							
	Logs (Baseline)				Levels			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay × Post	−0.167**				−7.516***			
	(0.066)				(1.696)			
Delay × Post1		−0.093	−0.010	−0.012		−13.048***	−11.183***	−12.197***
		(0.094)	(0.107)	(0.142)		(3.476)	(3.334)	(4.353)
Delay × Post2		−0.236***	−0.119	−0.280*		−2.394	0.217	−2.069
		(0.058)	(0.106)	(0.156)		(2.833)	(5.276)	(6.399)
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y	Y
State FE×t	N	N	Y	Y	N	N	Y	Y
Controls	N	N	N	Y	N	N	N	Y
States	49	49	49	49	49	49	49	49
Observations	7,693	7,693	7,693	7,693	7,693	7,693	7,693	7,693
Mean DV	3.9	3.9	3.9	3.9	36.22	36.22	36.22	36.22
R^2	0.660	0.660	0.671	0.724	0.677	0.680	0.687	0.745

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, % males aged 18-24 and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 17: Google searches for “gun store” (levels)

	Standardized share of Google searches for “gun store”							
	Logs (Baseline)				Levels			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay × Post	−0.227***				−0.750			
	(0.087)				(1.898)			
Delay × Post1		−0.278***	−0.235**	0.038		−0.450	−1.409	−0.266
		(0.105)	(0.099)	(0.097)		(3.657)	(2.363)	(2.603)
Delay × Post2		−0.181**	−0.121	−0.038		−1.029	−2.371	2.633
		(0.083)	(0.111)	(0.113)		(1.635)	(3.507)	(2.843)
State FE	Y	Y	Y	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y	Y	Y	Y
State FE×t	N	N	Y	Y	N	N	Y	Y
Controls	N	N	N	Y	N	N	N	Y
States	49	49	49	49	49	49	49	49
Observations	7,693	7,693	7,693	7,693	7,693	7,693	7,693	7,693
Mean DV	3.58	3.58	3.58	3.58	26.43	26.43	26.43	26.43
R^2	0.607	0.607	0.625	0.677	0.738	0.738	0.748	0.815

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, % males aged 18-24 and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 18: Homicide rates (levels)

	Homicides per 100,000 inhabitants					
	Logs (Baseline)			Levels		
	Any	Handgun	Other	Any	Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post1	−0.030*	−0.033***	0.004	−0.029	−0.034**	0.006
	(0.018)	(0.012)	(0.014)	(0.024)	(0.015)	(0.016)
Delay × Post2	0.001	−0.013	0.020	0.013	−0.012	0.025*
	(0.024)	(0.019)	(0.013)	(0.032)	(0.022)	(0.015)
County FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800	109,800	109,800
Mean DV	0.36	0.25	0.14	0.44	0.29	0.15
R^2	0.473	0.498	0.177	0.332	0.356	0.107

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

Table 19: Homicide rates (varying the sample)

	Log of homicides per 100,000 inhabitants											
	Baseline			Restrict to NICS			Excl NICS outlier data			Incl CT & MI		
	Any	Handgun	Other	Any	Handgun	Other	Any	Handgun	Other	Any	Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Delay × Post1	−0.030*	−0.033***	0.004	−0.039**	−0.036***	−0.004	−0.035*	−0.037***	0.003	−0.030*	−0.031***	0.002
	(0.018)	(0.012)	(0.014)	(0.019)	(0.012)	(0.017)	(0.018)	(0.012)	(0.014)	(0.017)	(0.012)	(0.013)
Delay × Post2	0.001	−0.013	0.020	−0.018	−0.023	0.008	−0.004	−0.018	0.020	−0.003	−0.015	0.017
	(0.024)	(0.019)	(0.013)	(0.023)	(0.019)	(0.016)	(0.024)	(0.019)	(0.013)	(0.023)	(0.018)	(0.012)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	2519	2519	2519	3050	3050	3050	3141	3141	3141
Observations	109,800	109,800	109,800	90,684	90,684	90,684	106,692	106,692	106,692	113,076	113,076	113,076
Mean DV	0.36	0.25	0.14	0.37	0.25	0.14	0.37	0.25	0.14	0.37	0.25	0.14
R^2	0.473	0.498	0.177	0.434	0.453	0.163	0.466	0.489	0.176	0.491	0.517	0.182

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

Table 20: Handgun homicide rates (trend specifications)

	Log of homicides per 100,000 inhabitants			
Trend model	Trends from full sample		Trends from pre-period	
	(1)	(2)	(3)	(4)
Delay × Post1	−0.033***	−0.028*	−0.034***	−0.040**
	(0.012)	(0.015)	(0.013)	(0.020)
Delay × Post2	−0.013	−0.003	−0.014	−0.028
	(0.019)	(0.034)	(0.018)	(0.039)
County FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y
County FE×t^2	N	Y	N	Y
Controls	Y	Y	Y	Y
Counties	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800
Mean DV	0.25	0.25	0.25	0.25
R^2	0.498	0.504	0.067	0.151

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

Table 21: Handgun homicide rates (remove border counties)

	Homicides per 100,000 inhabitants					
	Baseline			Excluding border counties		
	Any	Handgun	Other	Any	Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post1	−0.030*	−0.033***	0.004	−0.024	−0.028**	0.008
	(0.018)	(0.012)	(0.014)	(0.020)	(0.012)	(0.016)
Delay × Post2	0.001	−0.013	0.020	0.003	−0.012	0.021
	(0.024)	(0.019)	(0.013)	(0.026)	(0.019)	(0.015)
County FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	2848	2848	2848
Observations	109,800	109,800	109,800	102,528	102,528	102,528
Mean DV	0.36	0.25	0.14	0.36	0.24	0.14
R^2	0.473	0.498	0.177	0.464	0.487	0.175

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

Table 22: Homicide rates (state level)

	Log of homicides per 100,000 inhabitants					
	Any				Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post	−0.009					
	(0.011)					
Delay × Post1		−0.028**	−0.029	−0.030	−0.031*	−0.002
		(0.013)	(0.020)	(0.019)	(0.016)	(0.014)
Delay × Post2		0.010	0.008	−0.000	−0.010	0.010
		(0.015)	(0.022)	(0.026)	(0.019)	(0.014)
State FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
State FE×t	N	N	Y	Y	Y	Y
Controls	N	N	N	Y	Y	Y
States	49	49	49	49	49	49
Observations	1,764	1,764	1,764	1,764	1,764	1,764
Mean DV	0.42	0.42	0.42	0.42	0.28	0.15
R^2	0.751	0.752	0.761	0.787	0.802	0.456

Notes: Observations are at the state-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 23: UCR/FBI Homicide Reports: comparability

	Log of murders per 100,000 inhabitants								
	UCR			NVSS (UCR sample)			NVSS (full sample)		
	Any	Handgun	Other	Any	Handgun	Other	Any	Handgun	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Delay × Post1	−0.026	−0.030**	0.007	−0.043**	−0.038***	−0.005	−0.030*	−0.033***	0.004
	(0.018)	(0.013)	(0.018)	(0.018)	(0.013)	(0.017)	(0.018)	(0.012)	(0.014)
Delay × Post2	0.022	−0.027	0.057**	−0.001	−0.016	0.020	0.001	−0.013	0.020
	(0.031)	(0.017)	(0.025)	(0.027)	(0.020)	(0.018)	(0.024)	(0.019)	(0.013)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2232	2232	2232	2232	2232	2232	3050	3050	3050
Observations	80,352	80,352	80,352	80,352	80,352	80,352	109,800	109,800	109,800
Mean DV	0.33	0.17	0.17	0.36	0.24	0.13	0.36	0.25	0.14
R^2	0.482	0.535	0.254	0.487	0.513	0.185	0.473	0.498	0.177

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population (NVSS) or by the population within the jurisdiction of all reporting law enforcement agencies within a county (UCR).

D Additional Analyses

D.1 Supply of Gun Shows

One could be concerned that lower demand for firearms in *Delay* states arises because buyers flock to unregulated gun shows to circumvent the tedious and time-consuming process of purchasing through a federally licensed dealer. As previously noted, the majority of transactions at gun shows is presumably represented in our sample, since many exhibitors are federally licensed (and therefore mandated to perform background checks). Additionally, we have demonstrated that the demand for gun shows did not tilt towards *Delay* states.

We now show that also the supply of gun shows did not increase comparatively stronger in *Delay* states. Figure 18 displays their locations. Figure 19 shows the evolution of the log of gun shows graphically. Table 24 reports regression results using the log of monthly gun shows per 100,000 inhabitants as dependent variable. Overall, the results match our earlier findings regarding the demand for gun shows. We conclude that the supply of gun shows in *Delay* states very likely did not increase over *NoDelay* states.

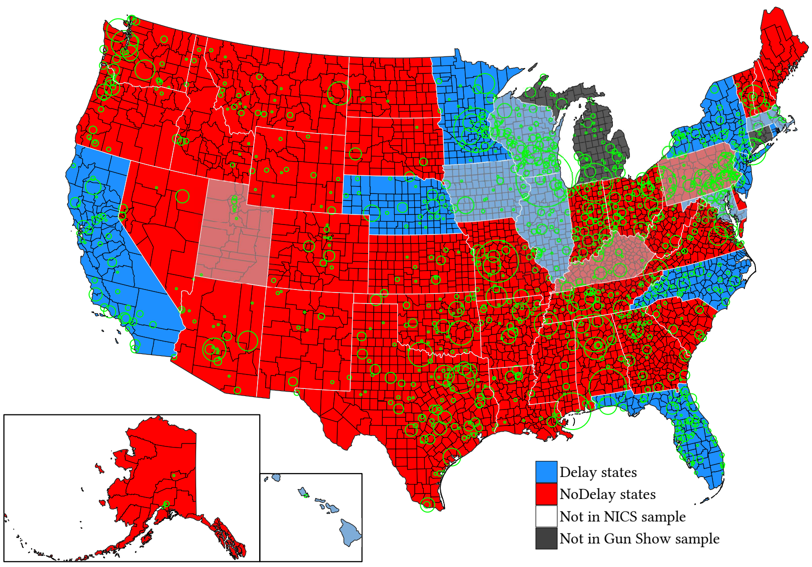

FIGURE 18: LOCATIONS OF GUN SHOWS

Map of the United States showing the distribution of gun shows in 2012 and 2013. Red states denote *NoDelay* states. Blue states denote *Delay* states. Connecticut and Michigan are shown in gray as we exclude the states from the sample. Each location with a gun show is represented by a green circle, the size of the green circle indicates the number of gun shows held at this location.

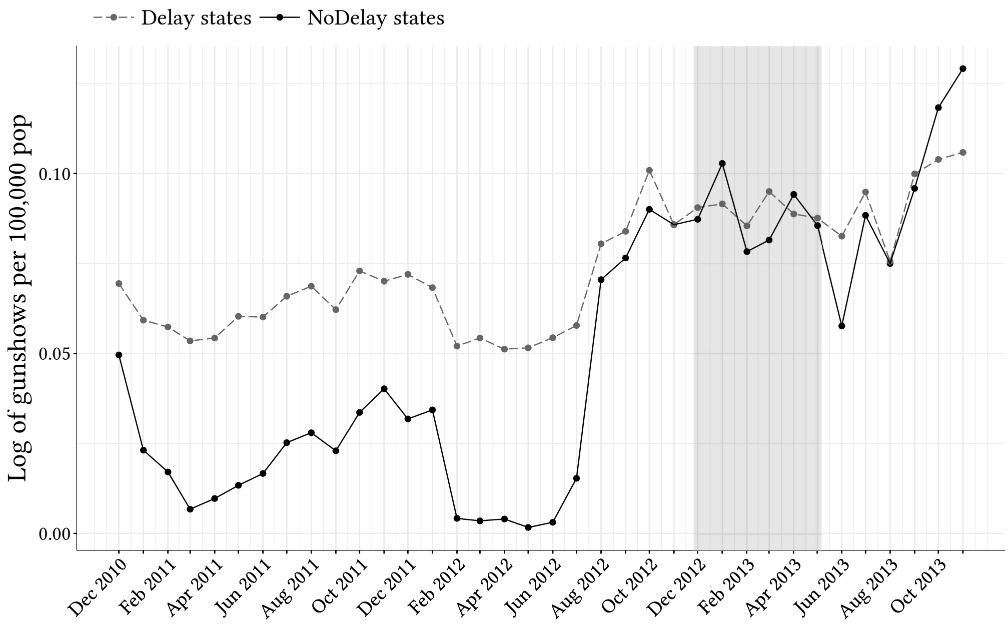

Figure 19: Gun shows in *Delay* vs *NoDelay* states

Log of monthly number of gun shows per 100,000 inhabitants in *Delay* states and *NoDelay* states between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

Table 24: Gun shows

	Log of gun shows per 100,000 inhabitants			
	(1)	(2)	(3)	(4)
Delay × Post	−0.032***			
	(0.008)			
Delay × Post1		−0.031***	−0.017**	−0.014**
		(0.008)	(0.007)	(0.006)
Delay × Post2		−0.033***	−0.013*	−0.012
		(0.010)	(0.008)	(0.008)
County FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
County FE×t	N	N	Y	Y
Controls	N	N	N	Y
Counties	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800
Mean DV	0.04	0.04	0.04	0.04
R^2	0.180	0.180	0.255	0.261

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

D.2 The Effect of Delay Laws on Crime

Section 6 established a significant reduction in gun-related homicide rates in *Delay* states as a result of a firearm demand shock. While we found some evidence indicating impulsive crime as a possible source of this effect, an alternative explanation, in which handgun-related homicides are simply a by-product of an increase in overall crime levels, cannot yet be ruled out. In this section, we use our empirical setup to probe the validity of the *"More Guns, Less Crime"* hypothesis and investigate the effects of having more handgun owners on crime rates other than homicide.

The data we use in this part is the UCR *Offenses Known and Clearances by Arrest* series which consists of detailed data from approximately 18,000 federal, state, tribal, county and local law enforcement agencies voluntarily submitted through the state UCR program or directly to the FBI. The monthly counts of index crimes for each law enforcement agency covers murder, manslaughter, rape, assault, robbery, burglary, larceny and vehicle theft. The data also allows distinguishing between the type of weapon used (e.g. firearm, knife, strong arm) in robberies and assaults as well as the severity of assault (simple vs aggravated) and rape crimes (forcible vs non-forcible).[38]

Results for violent crime are reported in Table 25. The outcome variables are crime rates constructed in the same way as the homicide outcomes in Section 6, and the regression specification is identical to that used in our baseline results. The sample equals that used in our analysis of the UCR SHR data in Table 11. Column 1 shows that violent crime rates decreased by 4.6% on average during the demand shock in *Delay* states. The largest, although mostly statistically insignificant, coefficients are the ones on rape, robbery and aggravated assault. Out of these, however, only robbery is significant at the 10% level while aggravated assault is statistically insignificant at a $p-$value of 11%. The effect on manslaughter is highly significant but comparatively small in size.

The UCR data allow us to further split incidents of murder, robbery and aggravated assault by the main type of weapon used. Table 26 shows the results for each of these three crime categories when dividing them into whether a firearm was used. Interestingly, only murder sees a significant relative decrease in firearm-related offenses while the effects of both robbery and aggravated assault seem to be mainly driven by

[38]See also Appendix G for the applied data cleaning procedure.

incidents where no firearms were used. Corresponding event-studies in Figures 20 and **??** cast some doubt on whether the observed effects for both types of non-firearm crime are unaffected by pre-trends. Importantly, none of the violent crime categories experiences a significant relative *increase*, which provides evidence against the existence of a strong deterrence effect.

Table 27 reports our findings for the categories of non-violent crime. As shown in column 1, also the non-violent crime rate saw a significant relative decrease in *Delay* states during the demand shock and also in its aftermath. The estimate for simple assault is positive but insignificant. Burglary, larceny and vehicle theft yield significant coefficients for at least one of the two (*Post1* and *Post2*) periods. Looking at the event-study graphs for each of these in Figures 22, 23 and 24 shows that only the effect on burglaries coincides with the start of the *Post1* period. The event-study on larceny indicates a pre-trend, and the one for vehicle theft starts rather abruptly almost at the end of the treatment period. We therefore do not necessarily interpret these two as outcomes of relatively increased gun ownership. For burglary, one could think of this as a deterrence effect from the *perceived* increase in gun ownership. Since *Delay* states presumably started from a comparatively smaller ex-ante *level* of gun ownership, a jump in gun sales might have been particularly deterring even though it was smaller than in *NoDelay* states. However, none of the other non-violent crime categories points in the direction of a strong deterrence effect from higher firearm ownership.

D.3 The Effect of Delay Laws on Suicides and Accidents

In addition to homicides, the comparatively smaller increase in handgun ownership in *Delay* states may also have affect suicides and accidents involving a handgun. In Table 28 we show our baseline results for homicides split by weapon type in columns 1 to 3 and then compare these with corresponding estimates for suicides and accidents. For accidents we find a significant relative *increase* in non-handgun incidents while those related to a handgun show no response. This is in contrast with the findings of Levine and McKnight (2017) who report that gun-related fatal accidents strongly increased in relative terms after the shooting at Sandy Hook Elementary School. While our study differs along a few dimensions, including the fact that we use county-level data and a slightly different treatment period, we do not think that these are the primary drivers

Table 25: Violent crimes

	Log of incidents per 100,000 inhabitants					
	Any Violent	Murder	Mansl'ter	Rape	Robbery	Agg. Assault
	(1)	(2)	(3)	(4)	(5)	(6)
Delay × Post1	−0.046**	−0.031**	−0.005*	−0.041	−0.041*	−0.040
	(0.023)	(0.015)	(0.003)	(0.038)	(0.023)	(0.025)
Delay × Post2	−0.040	0.009	−0.007**	−0.078	−0.048	−0.030
	(0.028)	(0.022)	(0.003)	(0.058)	(0.033)	(0.028)
County FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Counties	2232	2232	2232	2232	2232	2232
Observations	80,352	80,352	80,352	80,352	80,352	80,352
Mean DV	3.84	0.32	0.01	1.34	2.38	3.33
R^2	0.805	0.471	0.079	0.524	0.865	0.772

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

Table 26: Crimes by type of weapon

	Log of incidents per 100,000 inhabitants								
	Murder			Robbery			Aggr. Assault		
	All	Gun	Other	All	Gun	Other	All	Gun	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Delay × Post1	−0.026	−0.030**	0.007	−0.041*	0.005	−0.043*	−0.040	0.013	−0.045*
	(0.018)	(0.013)	(0.018)	(0.023)	(0.027)	(0.022)	(0.025)	(0.034)	(0.023)
Delay × Post2	0.022	−0.027	0.057**	−0.048	−0.033	−0.049*	−0.030	−0.026	−0.031
	(0.031)	(0.017)	(0.025)	(0.033)	(0.043)	(0.029)	(0.028)	(0.040)	(0.028)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2232	2232	2232	2232	2232	2232	2232	2232	2232
Observations	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352	80,352
Mean DV	0.33	0.17	0.17	2.38	1.4	1.98	3.33	1.52	3.12
R^2	0.482	0.535	0.254	0.865	0.864	0.842	0.772	0.799	0.743

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

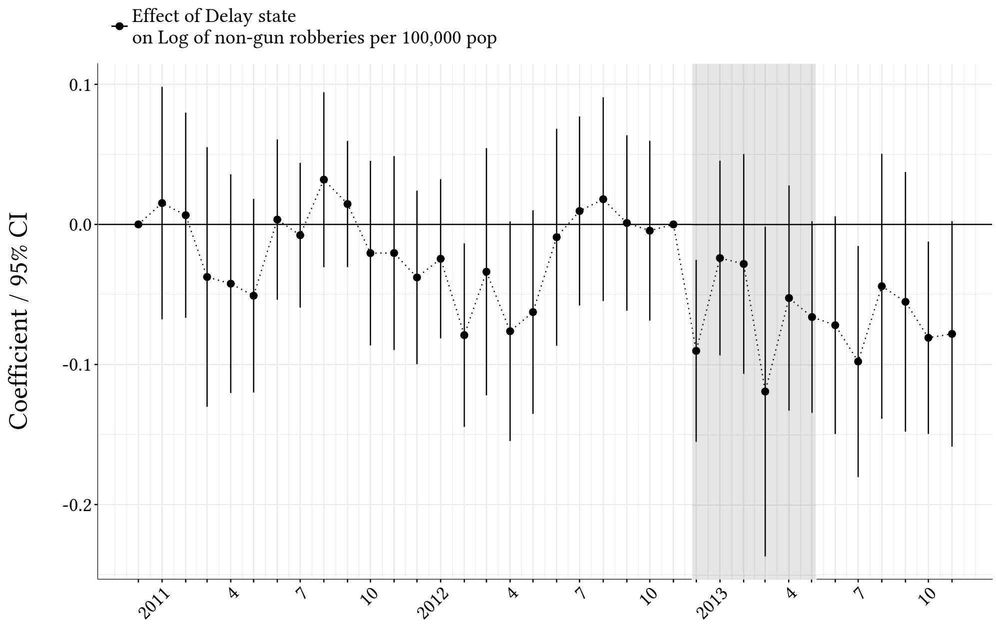

Figure 20: Event study graph for Non-Firearm Robbery Rate

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on log robbery per 100,000 inhabitants committed without a gun for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

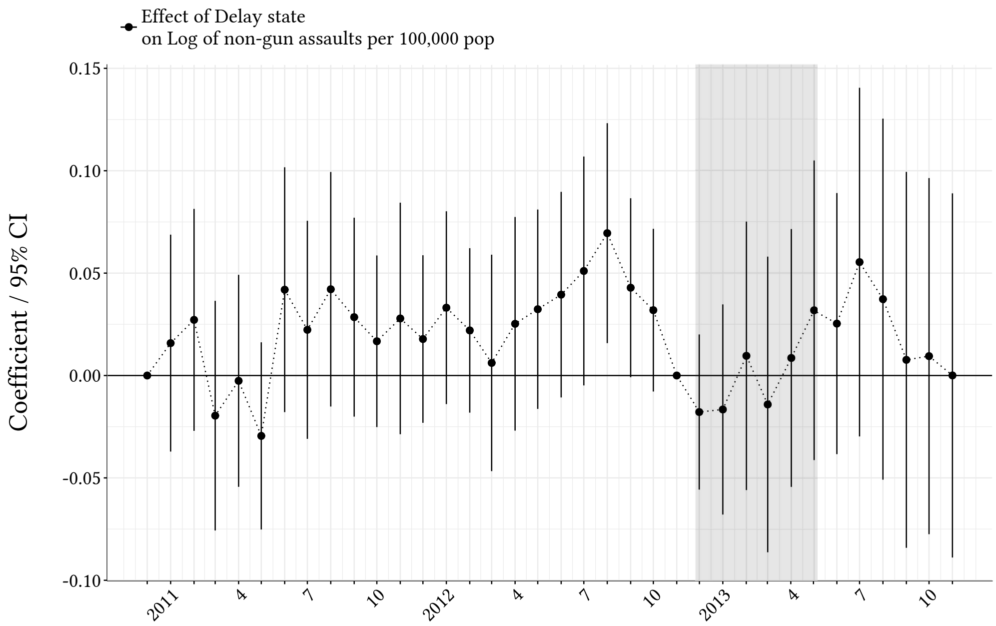

Figure 21: Event study graph for Non-Firearm Assault Rate

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on log assault per 100,000 inhabitants committed without a gun for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

Table 27: Non-violent crimes

	Log of incidents per 100,000 inhabitants				
	Any Non-Violent	Simple Assault	Burglary	Larceny	Veh.Theft
	(1)	(2)	(3)	(4)	(5)
Delay × Post1	−0.054**	0.019	−0.051	−0.083***	−0.021
	(0.023)	(0.021)	(0.035)	(0.025)	(0.035)
Delay × Post2	−0.052***	0.012	−0.063***	−0.075***	−0.057*
	(0.012)	(0.018)	(0.024)	(0.016)	(0.030)
County FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
Counties	2232	2232	2232	2232	2232
Observations	80,352	80,352	80,352	80,352	80,352
Mean DV	6.33	4.62	4.53	5.68	3.27
R^2	0.909	0.956	0.813	0.848	0.814

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

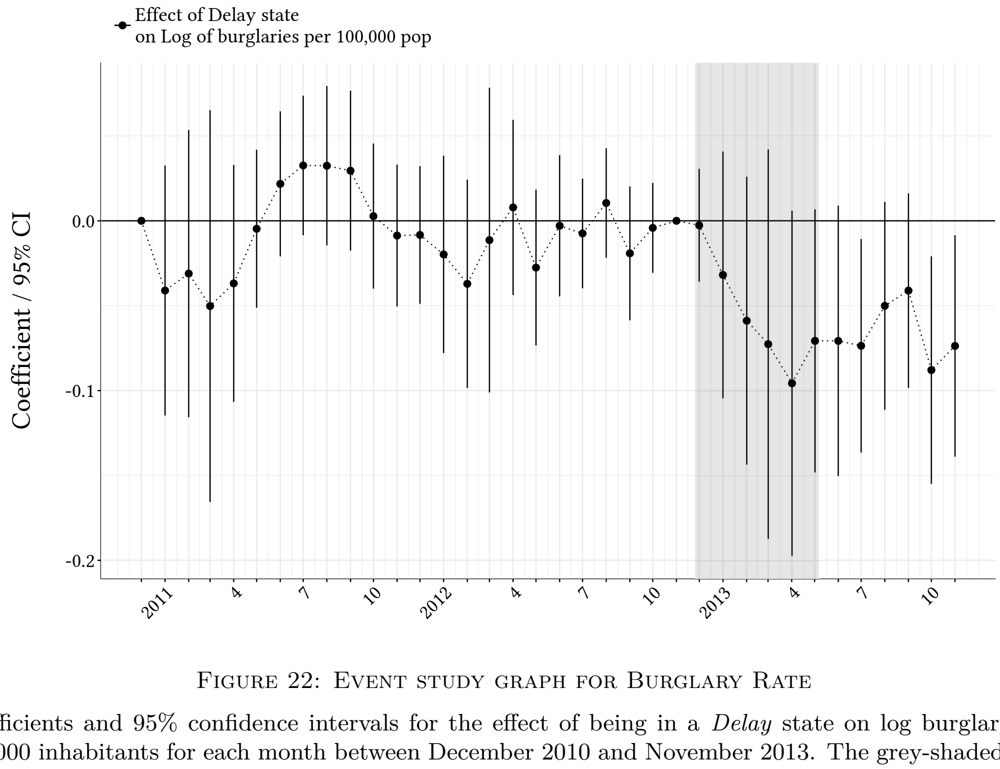

Figure 22: Event study graph for Burglary Rate

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on log burglary per 100,000 inhabitants for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

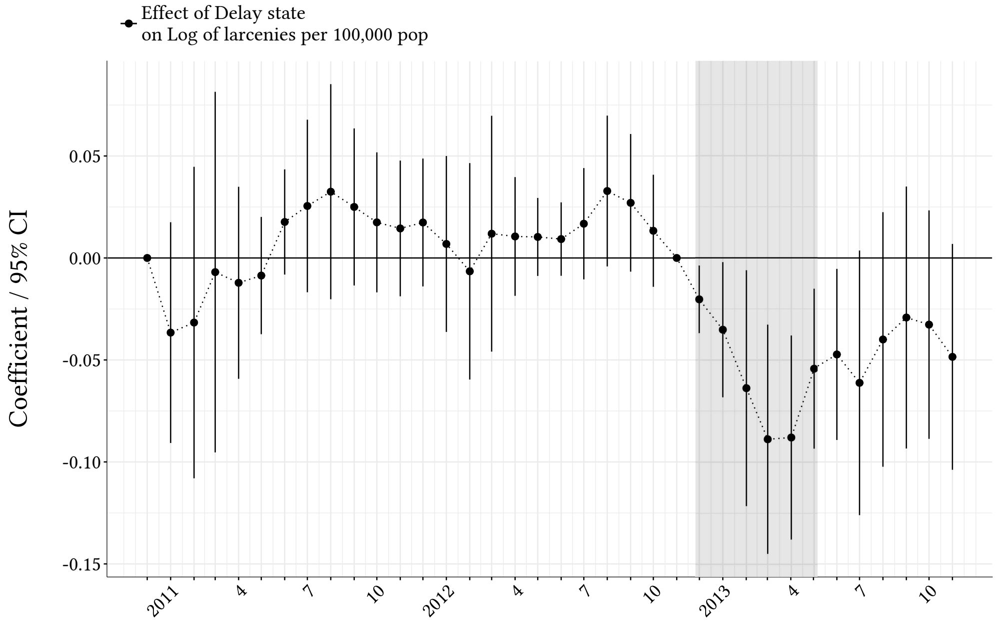

Figure 23: Event study graph for Larceny Rate

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on log larceny per 100,000 inhabitants for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

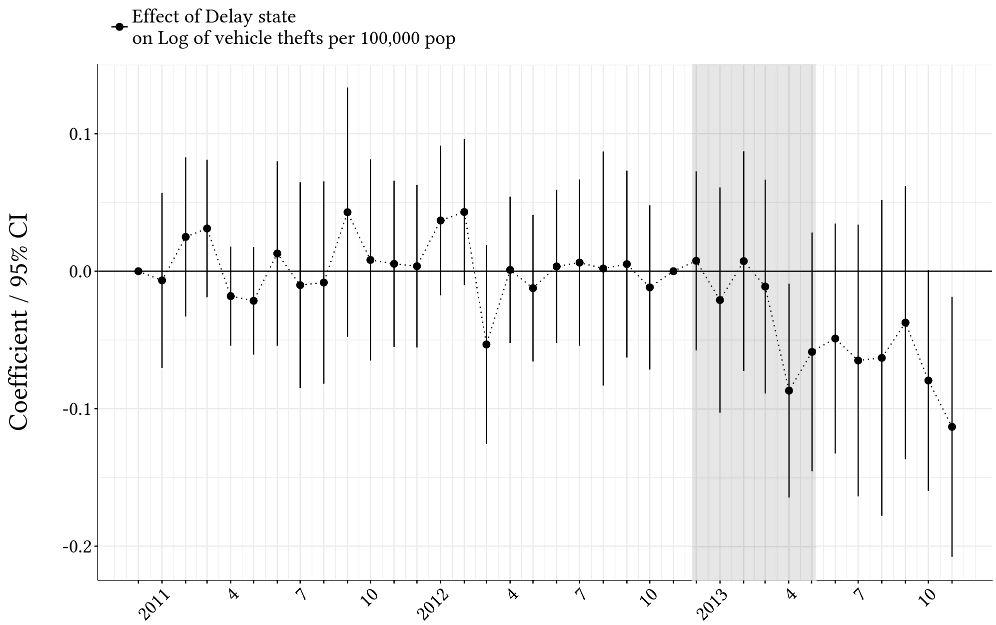

Figure 24: Event study graph for Vehicle Theft Rate

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on log vehicle theft per 100,000 inhabitants for each month between December 2010 and November 2013. The grey-shaded area shows the first six months after the 2012 election and the shooting at Sandy Hook.

TABLE 28: HOMICIDES, ACCIDENTS, AND SUICIDES

	Log of mortality rate per 100,000 inhabitants								
	Homicide			Accidents			Suicide		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Delay × Post1	−0.030*	−0.033***	0.004	0.031*	−0.002	0.032*	−0.001	0.011	−0.004
	(0.018)	(0.012)	(0.014)	(0.018)	(0.002)	(0.018)	(0.015)	(0.011)	(0.016)
Delay × Post2	0.001	−0.013	0.020	0.011	0.001	0.011	0.018	0.021	0.005
	(0.024)	(0.019)	(0.013)	(0.020)	(0.003)	(0.020)	(0.019)	(0.018)	(0.018)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	3050	3050	3050	3050	3050	3050	3050	3050	3050
Observations	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800	109,800
Mean DV	0.36	0.25	0.14	1.75	0.01	1.75	0.81	0.38	0.51
R^2	0.473	0.498	0.177	0.416	0.078	0.416	0.223	0.216	0.188

Notes: Observations are at the county-level. The sample period is a 24-month window centered around the Sandy-Hook shooting, i.e. December 2011 until November 2013. Reported standard errors are clustered at the state-level in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are % rural, % blacks, % hispanics, and % males aged 18-24. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

for the observed differences across the two studies. Instead, a more likely explanation could be our use of handgun purchase delay laws instead of the 2012 Obama vote share as a shifter for the reaction in firearm sales. Our results thus indicate that the effects are not robust to this different, and in our view more credible, identification strategy.[39]

Columns 7 to 9 show the reaction of suicide rates. Importantly, those related to handguns do not show any significant reaction. As prior research has argued that having a gun in the home is positively associated with suicide by firearm (Anglemyer, Horvath, and Rutherford, 2014), this finding may be surprising. However, our time window used is relatively small and only if a person is both suicidal and in the possession of a gun would a firearm-related suicide occur. Having said that, it seems plausible that additional suicides may materialize after a longer time period. It seems unlikely, though, that a person with suicidal thoughts would purchase a firearm due to the gun demand shock where the primary motive was an increased perception of needing firearms for self-defense and expected limitations to future firearm access.

[39]Section 5.2. shows that differences across *Delay* and *NoDelay* states only arise in handgun sales, not in the intention to purchase a firearm. We deem it unlikely to be the case for the 2012 Obama vote share to a similar extent.

E Firearm Purchase Delays

As already stated in the main text, there is substantial heterogeneity in firearm purchasing and sales restrictions imposed by the states. For example, many states invoke restrictions on the prerequisites and responsibilities of gun dealers, such as whether they require an additional state license to operate their business or whether they are supposed to keep centrally stored electronic records of transactions. Other legal restrictions concern buyers, as states can for instance decide if they want buyers to be able to purchase guns in bulk, if buyers need a permit prior to purchase, if they have to undergo background checks (for transactions exempted from federal background check requirements), or if buyers are required to wait a certain amount of time between purchasing and receiving their gun. Finally, there exists legislation concerned with restrictions on carrying firearms in public places, including schools and the workplace.

For this study, we are primarily interested in restrictions that delay the purchase of a handgun. These are mandatory waiting periods and firearm purchasing (or ownership) permits. Between December 2010 and November 2013, the period of our study, nine states and the District of Columbia had imposed mandatory waiting periods. California and D.C. require 10 days, Hawaii 14 days, Rhode Island 7 days and Illinois between 24 hours (long guns) to 72 hours (handguns) on all firearm purchases. Minnesota is the only state to require 7 days wait between purchase and pickup of handguns and assault rifles only. Maryland and New Jersey impose 7 days for handguns, while Florida and Iowa impose a 3 day waiting period for handguns. Wisconsin repealed its 48 hour waiting time on handguns in 2015.

Furthermore, some states require a license to possess or buy a firearm prior to the actual purchase, which due to bureaucratic hurdles can also impose a waiting time. In Connecticut, a handgun eligibility certificate may take up to 90 days before being issued. Before buying a gun in Hawaii, prospective gun owners have to obtain a permit to purchase which can take up to 20 days to be issued. Buyers in Illinois have to obtain a Firearm Owner's Identification card (FOID) before being allowed to purchase an unlimited number of firearms in the following ten years. Obtaining an FOID can take up to 30 days. The state of Maryland requires buyers to hold a Handgun Qualification License which will be issued or denied within 30 days of application. In Massachusetts, authorities may take up to 30 days to process a request for a license to carry or a

Firearm Identification Card (FID), where the former allows unlimited purchases of any firearms without additional paperwork and the latter is restricted to rifles and shotguns. Nebraska requires potential buyers of handguns to be in possession of a handgun certificate or a concealed carry permit, which may take up to 3 days to be issued. The permit allows unlimited purchase of handguns in a 3 year period. Residents of New Jersey in turn must obtain a permit to purchase a handgun for each purchase separately, while they can purchase unlimited shotguns and rifles with a Firearms Purchaser Identification Card (FPIC). Authorities may take up to 30 days to issue such a permit. In New York, a license to possess or carry a handgun is necessary for each gun and obtaining one can take up to six months. In North Carolina, a license to purchase a handgun can take up to 14 days to be issued, and it is valid for one gun only. Residents of Rhode Island need to wait up to 14 days to receive their pistol safety certificate (blue card).

F NICS Background Checks per State

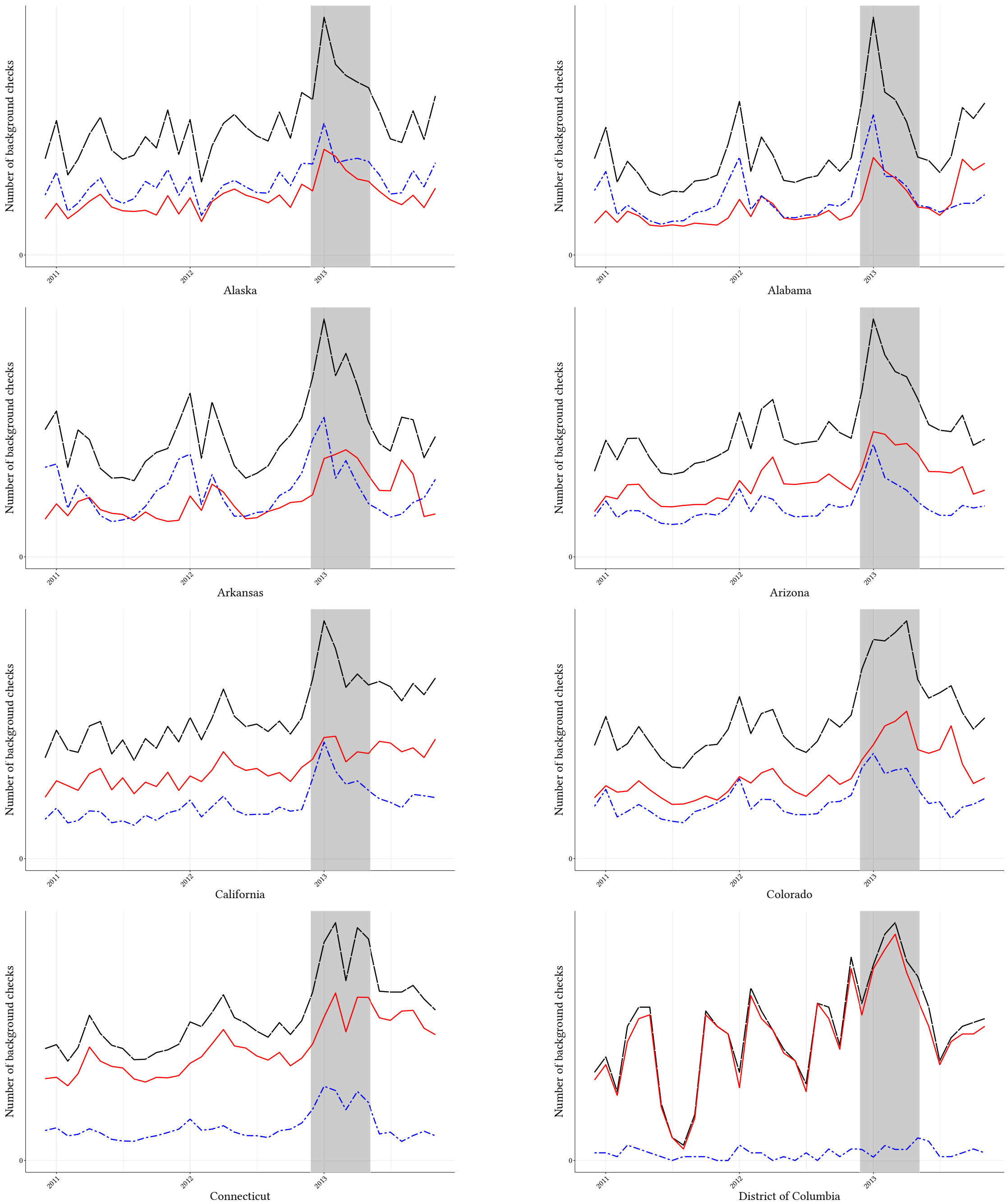

Figure 25: Monthly NICS background checks, AL to DC

Monthly federal NICS gun sale background checks plotted over time between December 2010 and November 2013 in absolute numbers broken up by state. The gray area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line for permits, and the black line displays the sum of the two.

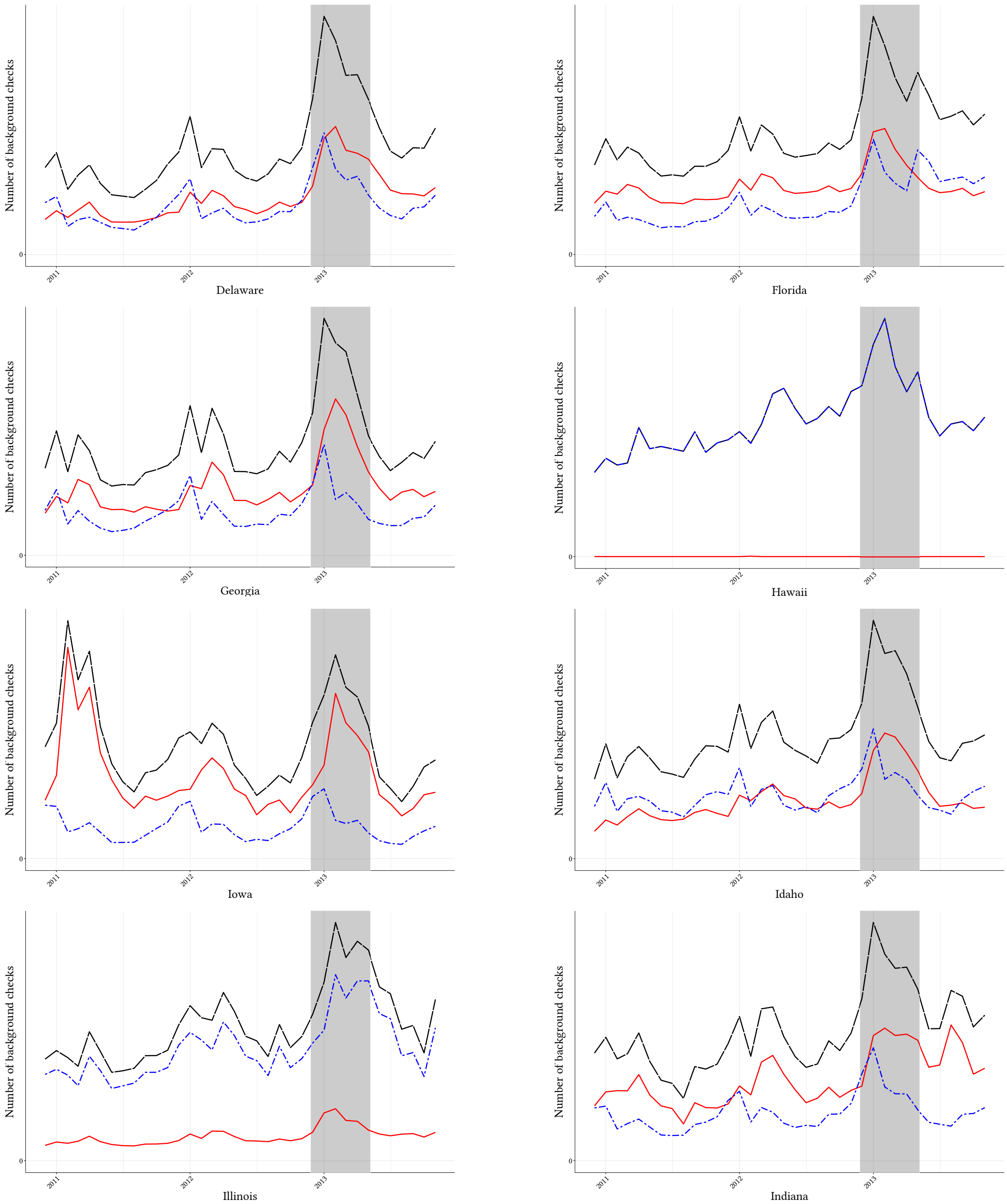

FIGURE 26: MONTHLY NICS BACKGROUND CHECKS, DE TO IN

Monthly federal NICS gun sale background checks plotted over time between December 2010 and November 2013 in absolute numbers broken up by state. The gray area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line for permits, and the black line displays the sum of the two.

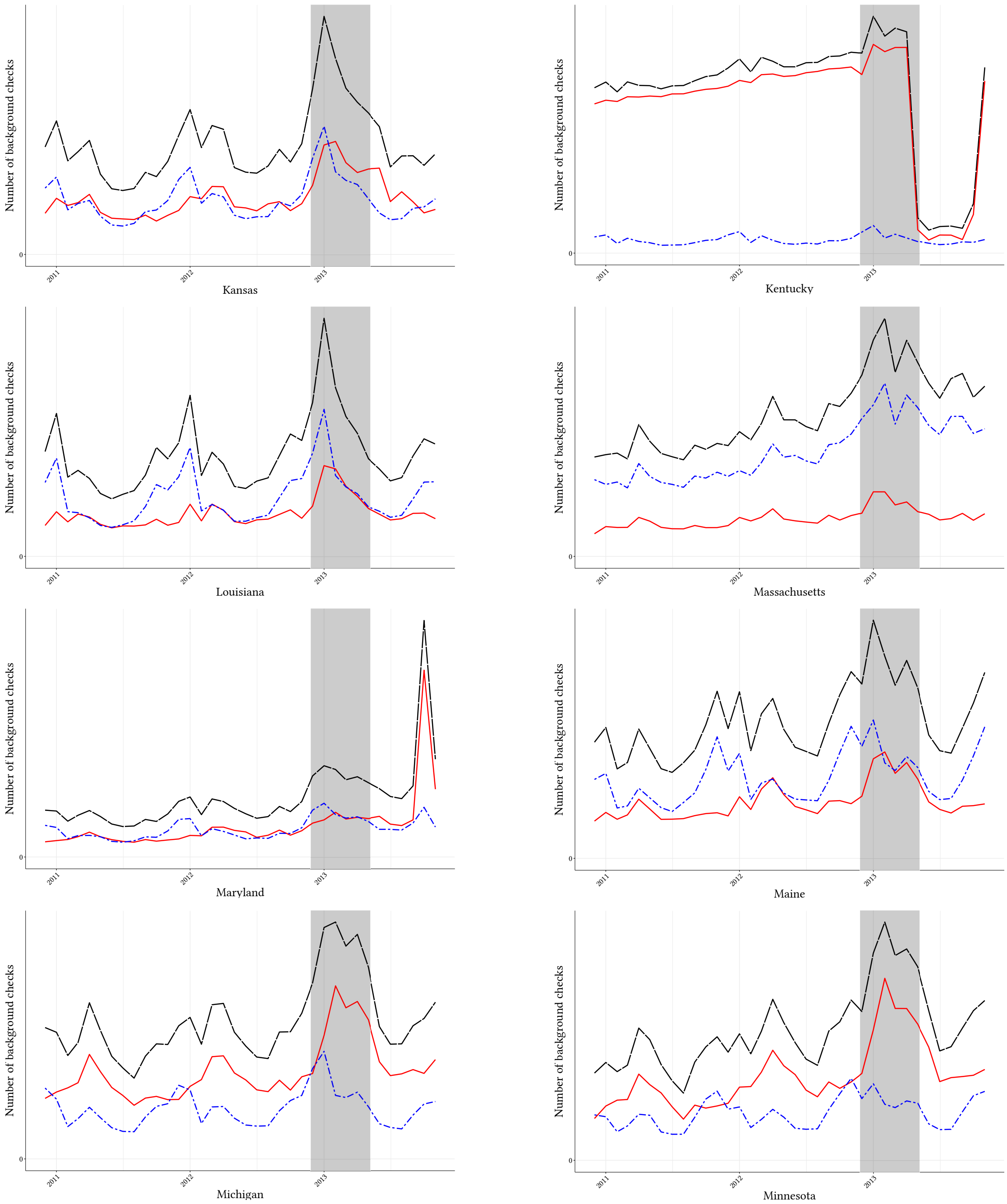

Figure 27: Monthly NICS background checks, KS to MN

Monthly federal NICS gun sale background checks plotted over time between December 2010 and November 2013 in absolute numbers broken up by state. The gray area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line for permits, and the black line displays the sum of the two.

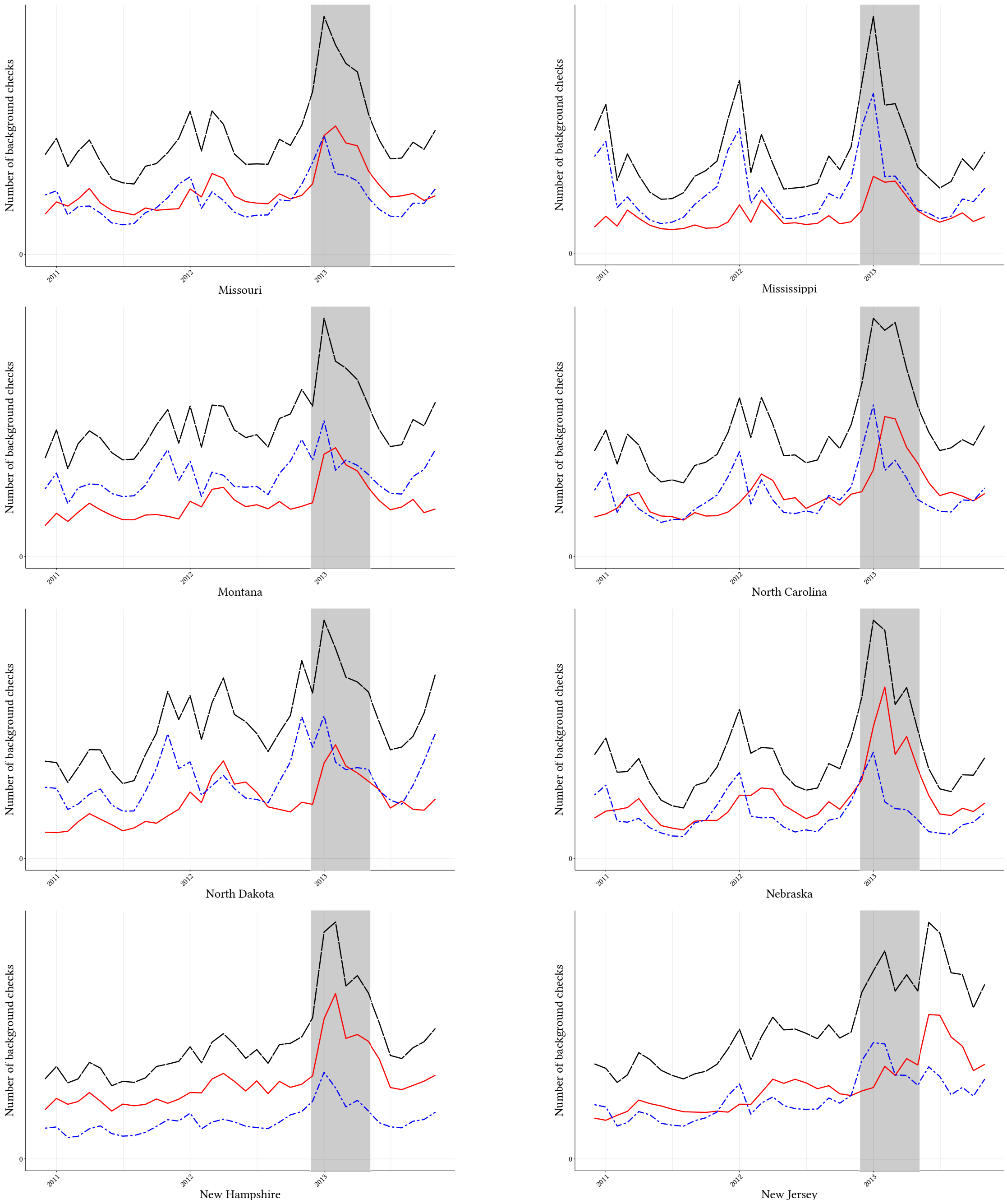

Figure 28: Monthly NICS background checks, MO to NJ

Monthly federal NICS gun sale background checks plotted over time between December 2010 and November 2013 in absolute numbers broken up by state. The gray area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line for permits, and the black line displays the sum of the two.

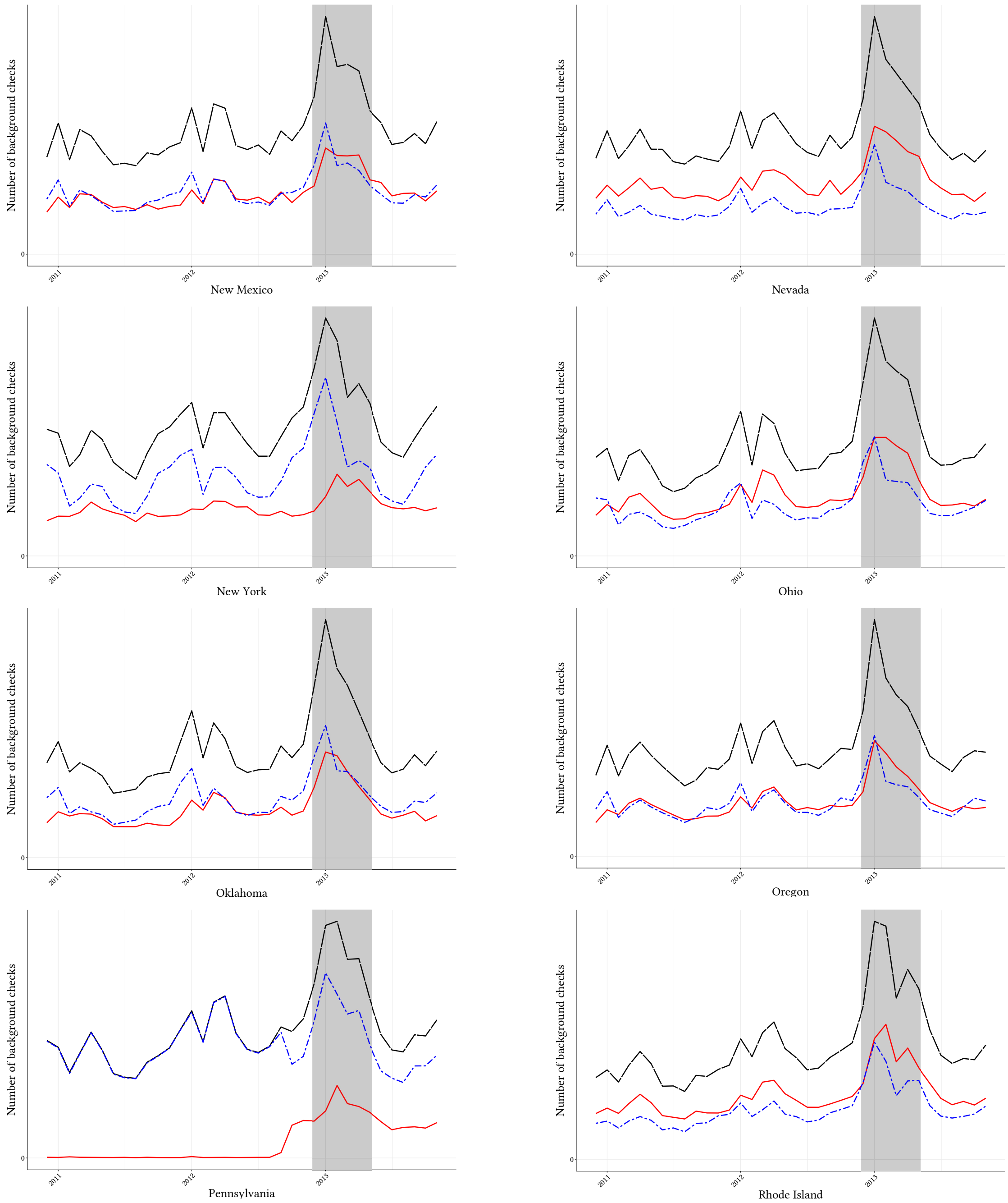

Figure 29: Monthly NICS background checks, NM to RI

Monthly federal NICS gun sale background checks plotted over time between December 2010 and November 2013 in absolute numbers broken up by state. The gray area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line for permits, and the black line displays the sum of the two.

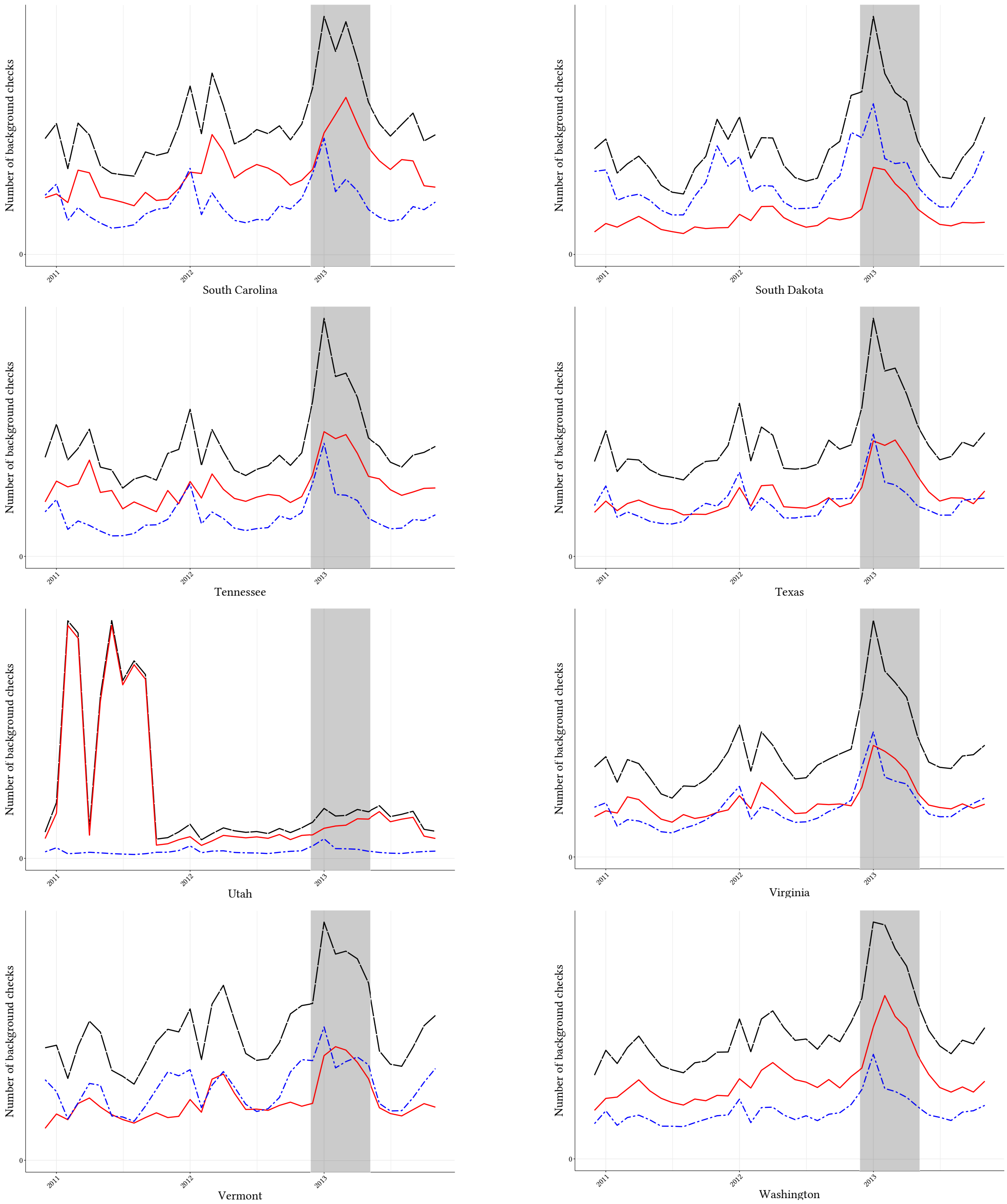

Figure 30: Monthly NICS background checks, SC to WA

Monthly federal NICS gun sale background checks plotted over time between December 2010 and November 2013 in absolute numbers broken up by state. The gray area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line for permits, and the black line displays the sum of the two.

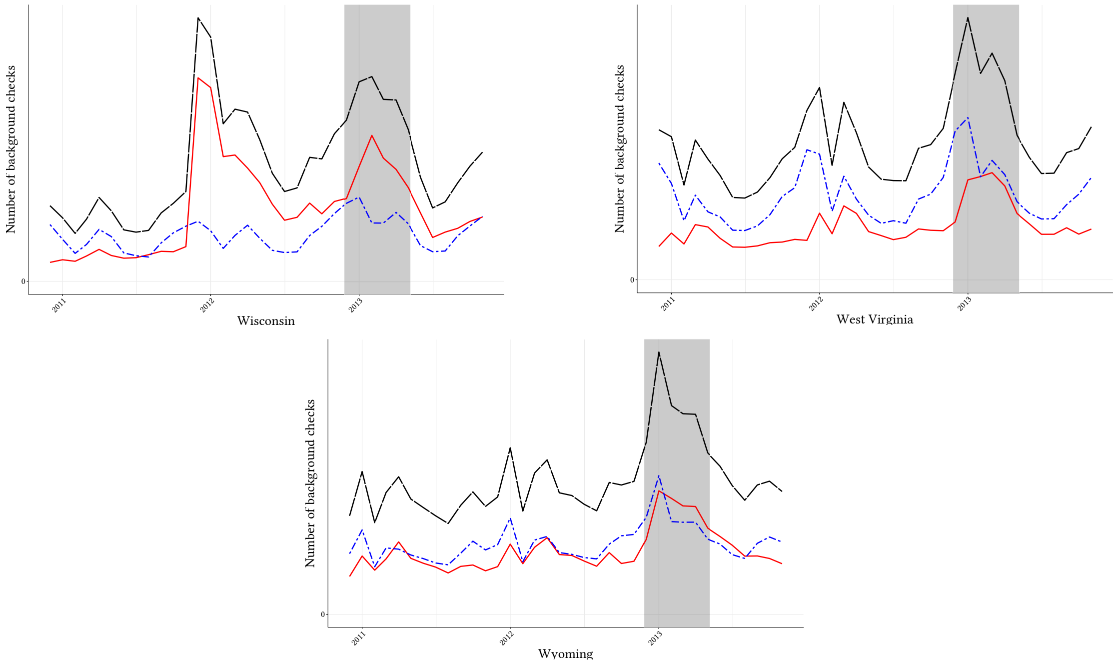

FIGURE 31: MONTHLY NICS BACKGROUND CHECKS, WI TO WY

Monthly federal NICS gun sale background checks plotted over time between December 2010 and November 2013 in absolute numbers broken up by state. The gray area depicts the six months after the 2012 election and the shooting at Sandy Hook. The red line shows background check for handguns, the blue line for permits, and the black line displays the sum of the two.

G Cleaning Procedure for UCR Data

The UCR crime data suffers from inconsistent reporting by some participating agencies. Common reporting mistakes include large negative absolute values for crimes, or continuously reporting zero crimes. These obvious problems of the UCR data have led some scholars to conclude that the data should not be used in empirical analysis (Maltz and Targonski, 2002). We take a more pragmatic approach and use the UCR data only in supplementary analyses after applying the following data cleaning guidelines set out in Targonski (2011).

First, we determine truly missing data points. An entry of zero could either mean that no crimes occurred, or that the agency was not reporting any crimes. An additional reporting variable however indirectly indicates, whether data was submitted. If no data was submitted, this reporting variable will have missing values for that specific date. We thus exclude all observations showing zero crimes, where the additional reporting variable contains missing values. Second, there are some obvious cases of data bunching, as there exist agencies that report their data only quarterly or (semi)annually, but no data in the months between. We identify those observations using an algorithm designed by Targonski and we also exclude them from the analysis.[40] Third, some smaller agencies choose to not report crimes themselves, but through another agency. In that case, they show up as reporting zeroes, although their counts are reflected in the data of the reporting agency. We drop those observations. Fourth, we apply the rule of 20 to identify wrongly reported zero crimes. Whenever an agency reports on average 20 or more crimes per month, it seems unlikely they experienced zero crime in any given month. Such data points are also excluded from our analysis. Fifth, we delete all observations with outlier values 999, 9999 and 99999 from the sample. Sixth, we remove all data containing negative values smaller than -3.[41]

In addition to the cleaning procedure above, we drop data from all counties which do not report consistently over the full sample period and report zero crimes throughout.

[40]The algorithm is not part of Targonski (2011) but we received instructions and rules for the algorithm from Joe Targonski in a personal email exchange. The algorithm basically identifies any county (with absolute annual crime reports above 10) that report crimes only in March, June, September and December (or a subset of those for (semi-)annually reporters), and zero crimes in all other months.

[41]In line with Targonski (2011) we ignore small negative values of at least -3. Those are usually corrections for misreporting in previous months.

In order to ensure sufficient coverage and representativeness we also drop counties if the consistently reporting agencies cover less than 50% of the county's population in 2010.