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

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

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 2% 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: K420, H760, H100, K140.

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 15 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 seasonally differenced 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

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

delay laws witnessed a relative 8% *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 elevated demand 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 during our treatment period.

We also present evidence suggesting time inconsistency, rather than fully rational behavior, as the more likely mechanism behind our findings. While certain forms of time-inconsistent behavior predict that even very short delays may have a substantial impact, mere exponential discounting implies that longer waits *monotonously* lead to stronger reactions in firearm purchases. Using variation in the length of delays across states, we however fail to observe a relationship between delay length and effect size. Furthermore, Google search results show that intentions and actions of gun buyers generally do not coincide, therefore pointing towards time-inconsistent decision-making. These findings in sum strongly suggest a behavioral channel to be driving gun buyers' actions.

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 2% *decrease* in overall homicide rates during the demand spike, which is entirely driven by homicides involving handguns. Our baseline estimate implies that about 200 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 also expect to observe more impulsive homicides if time inconsistency was linked to impulsive behavior in general. We find that, 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 four important streams of research. First, we add to the literature investigating the impact of firearm legislation, and in particular purchase delays, on crime rates. [Ludwig and Cook \(2000\)](#) 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. \(2018\)](#) 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 and law changes can be anticipated by prospective gun buyers, our paper substantially advances this literature by providing novel and credible identification through exploiting a sudden and unanticipated demand shock in conjunction with pre-existing delay laws.

²All 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.

Several studies 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 \(2017\)](#) 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.

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. Another recent paper similar to ours are [Levine and McKnight \(2017\)](#) who study how elevated gun exposure after the Sandy Hook shooting translated into higher rates of firearm-related accidents.⁴ We add

³Due 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 \(2015\)](#).

⁴While gun-related accidents are not at the heart of our paper, supplementary results reported in the appendix cannot replicate those findings. Rather, our findings suggest that the primary detrimental effect of increased gun ownership after the Sandy Hook shooting was an increase in gun-related *homicides*. We suspect this discrepancy to arise from Levine and McKnight’s identification strategy, which 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

to this literature by confirming the positive link between guns and homicides found in earlier studies using improved identification, and by providing the first study to look specifically into homicide characteristics.

Third, we relate to studies at the intersection between behavioral economics and economics of crime linking impulsiveness 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. [Heller et al. \(2017\)](#) report the results from three large RCTs aimed at changing the way disadvantaged youth make decisions (from more automatic to more deliberative decisions). They find reduced arrests and increased graduation rates following the interventions. We advance this literature by providing the first study to establish a link between firearm availability and fatal consequences of impulsive behavior.

Fourth, 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](#)). To the best of our knowledge, we are the first study to investigate behavioral motives in the context of gun ownership.

This paper is organized as follows: Section 2 provides relevant background information regarding U.S. gun laws and the gun demand shock we consider. 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

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

education, are orthogonal to accidental firearm deaths. Our study, instead, relies on frictions in the purchasing process and comprises a careful assessment of the identifying assumptions.

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.⁵ 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 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. 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

⁵Overviews of all current restrictions in the respective states can be found in [NRA \(2018\)](#) and [Giffords Law Center to Prevent Gun Violence \(2018\)](#).

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).

In order to accurately determine states' delay laws, and to furthermore minimize chances of mis-classification, we utilize several sources by applying the following procedure: Our starting point is the classification of the 50 U.S. states as having waiting periods or permit requirements for handgun sales between 2009 to 2013 according to Siegel et al. (2017). We then complement this data with information on the District of Columbia and actual delay length using Cherney, Moral, and Schell (2018). Finally, we cross-check the data with classifications provided by Law Center to Prevent Gun Violence (2012a,b). Any conflicting categorizations across the three sources are then resolved by looking up the precise wording of the respective law.⁷ The results of this procedure are reported in Table 1, which shows that during the period of our study, from November 2009 to October 2013, 15 states and the District of Columbia had adopted some form of delay laws throughout. 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.⁸ 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.⁹

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.¹⁰ We refer to all other states as *NoDelay* states.

⁶Fees can range from \$1 plus notary fee in Michigan to \$340 in New York City (\$100 in the state of New York). See <https://www.cga.ct.gov/2013/rpt/2013-R-0048.htm>.

⁷Sources used for our all of our classifications are reported in Table 37 in the Appendix.

⁸Wisconsin repealed its 48 hour waiting time on handguns in 2015.

⁹We provide more details on the specifics of all delay laws in Appendix F.

¹⁰For 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, averaging would be inconsequential for our analysis.

TABLE 1: HANDGUN WAITING PERIODS AND HANDGUN PURCHASING LICENSE DELAY BY STATE AS OF NOVEMBER 2012

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	60*	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†	40	5†§	0	0	0	0	2
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	30	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	0*	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 legal minimum 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 legal maximum time (in days) which 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. * Also requires a safety certificate through completing a safety course lasting only a few hours. † No legally defined maximum time, inferred from [U.S. Department of Justice \(2005\)](#). § Abolished in December 2012. ¶ Repealed in 2015, i.e. after the end of the sample period. Sources for each state are reported in Table 37.

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 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

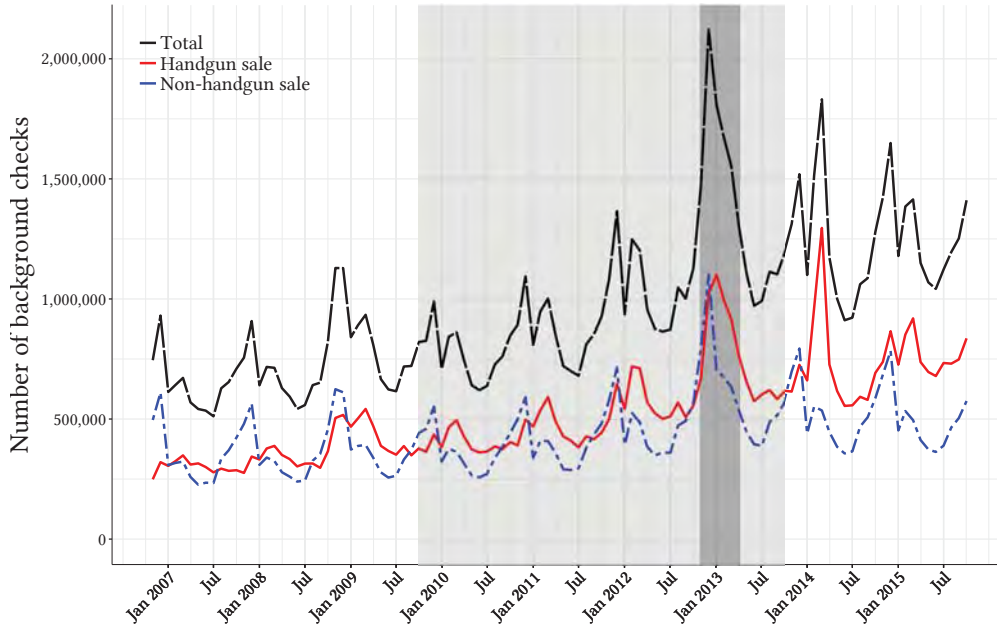


FIGURE 1: NICS BACKGROUND CHECKS

Monthly federal NICS background checks plotted over time between November 2007 and October 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 checks for handguns, the blue line all other firearm-related background checks, and the black line displays the sum of the two.

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 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.

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.¹¹ In our analysis, we use monthly NICS handgun sale background checks in a given state between November 2010 and October 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).¹²

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 the purchase of multiple weapons, as well as an exchange of an old for a new firearm. 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 or produce outliers.

¹¹The data is available for download at https://www.fbi.gov/file-repository/nics_firearm_checks.-_month_year_by_state.type.pdf.

¹²For 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. Regarding private sales, in later sections we test for the presence of a similar reaction on secondary markets (outside the NICS system), which is also an important challenge to our identification strategy, by looking at the demand for and timing of gun shows. We regard the issue of multiple purchases to be less important, as we are interested in the extensive margin and would only underestimate a potential effect. High numbers of exchanges of old guns for new guns in *NoDelay* states, instead, could potentially distort our results, as they do not lead to a net increase in available firearms. These differences in handgun exchanges, however, should be correlated with overall gun ownership levels, a channel that we test by including interactions with other correlates of gun popularity. Finally, we add background checks for permits to our measure of handgun sales to capture cases in which buyers obtain a permit in order to purchase a handgun.¹³

To remove as many inaccuracies and problematic observations from the sample as possible, we apply the following approach. First, we discard Hawaii, Illinois and Massachusetts, where permit checks also include those for long guns. Next, we remove Connecticut and Michigan. Connecticut is 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. Michigan, as already mentioned earlier, changed from requiring a permit to not requiring a permit in the period of observation. In order to remove outliers, we drop Kentucky, Pennsylvania and Utah, as well as parts of the series for Iowa, Maryland and Wisconsin from the sample. Kentucky performs monthly rechecks of existing permit holders, artificially inflating the data ([Lang, 2013, 2016](#)). Pennsylvania did not record a single handgun sale background check from November 2009 until August 2012. Utah performed quarterly rechecks of existing permit holders in 2011 leading to strong spikes in background checks from January 2010 to August

¹³This procedure could not be applied for Hawaii, Illinois and Massachusetts as permit checks in these states may also include permits for long guns. Permits were also *not* added to handgun sale checks for Florida where, for no apparent reason, almost all months report 0 permit checks (and single digits for non-zero months) until April 2013, when they suddenly jump to 15,000-30,000 per month for the remainder of the sample period. Any further reference to handgun background checks implicitly includes background checks made for permits, unless otherwise noted.

2011.¹⁴ 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 between November 2010 and June 2011. Maryland changed its gun laws with respect to licensing in 2013, leading to a massive background check increase in September and October 2013 ([New York Times, 2015](#)). Finally, Wisconsin passed a concealed-carry bill in 2011 leading to a jump in background checks from November 2011 to May 2012.¹⁵

Performing the steps above yields our baseline sample for investigating the effect of delay laws on background checks (*BL1*). While we prefer this restricted sample for our analysis of the NICS data, robustness checks for our main results will show that alternative (and less restrictive) sample definitions generate qualitatively similar results.

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 November 2010 and October 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.¹⁶ We aggregate this data at the county-month level to obtain a balanced panel of 3,047 counties and normalize by the county’s 2010 population in

¹⁴Our implicit rule was to drop an entire state series if outliers covered at least 12 consecutive months between November 2009 and October 2013.

¹⁵In Appendix G we plot the temporal variation in handgun sale and permit NICS background checks for each state separately and specifically highlight the data irregularities for these states.

¹⁶Our 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.

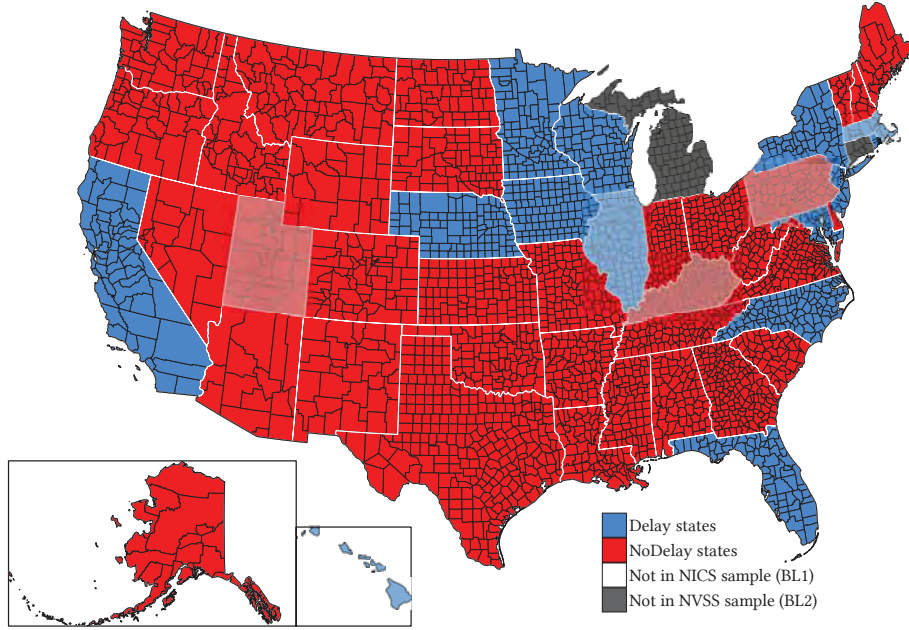


FIGURE 2: STATES AND COUNTIES REPRESENTED IN THE NICS AND NVSS SAMPLES

Map of the United States showing the states contained in the NICS background check data and counties contained in the NVSS homicide data. Red counties are located in *NoDelay* states. Blue counties are located in *Delay* states. Shaded states are dropped in their entirety from the NICS sample. Dark grey counties are not present in the NVSS sample.

100,000. This second baseline sample, denoted as *BL2*, covers every U.S. state apart from Connecticut and Michigan and we use it in all analyses based on non-NICS data.¹⁷

Figure 2 shows the counties represented in our NVSS sample BL2 and highlights states which were removed in the NICS sample BL1. When we present our results, we will also present robustness checks that apply more or less stringent sample restrictions and deliver very similar results.

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 the cleansed UCR homicide data to the county-month level to obtain a balanced panel

¹⁷As 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.

with data from 2,229 counties.¹⁸ Homicide counts from the UCR series are normalized using the aggregate population in 100,000 covered by the reporting agencies within a specific county in 2010.

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 whether consumers may behave time-inconsistently, we would like to separate intentions from actions.¹⁹ 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.

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

¹⁸The cleaning procedure applied to the UCR data sets on homicide and other crimes is discussed in Appendix H.

¹⁹In Appendix Section A we present formal predictions derived from a theoretical model contrasting time consistency and time inconsistency.

show” in the same way as we did for “gun store” searches. In Appendix E.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.²⁰ 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 Further variables used only for robustness checks are introduced and explained where appropriate.

²⁰The website is currently unavailable but can still be accessed in the version we used to compile our data using <https://web.archive.org/web/20160426010710/http://www.gunshowmonster.com/>.

²¹In order to avoid concerns about bad controls, we use time-invariant and pre-determined control variables interacted with year-month fixed effects which means 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.bristol.ac.uk/efm/media/workingpapers/working_papers/pdf/18694.pdf.

4 Empirical Strategy

4.1 Difference-in-Differences Approach

To estimate the effect of delay laws on handgun purchases and mortality during a demand shock, a fairly straightforward approach is to use a *Difference-in-Differences* (DiD) regression model, which exploits time-series variation from the six-month surge in firearm demand across the United States:

$$\begin{aligned} \log(\text{HandgunSales}_{st}) = & \alpha + \beta_1(\text{Delay}_s \times \text{Post1}_t) + \beta_2(\text{Delay}_s \times \text{Post2}_t) \\ & + \delta_t \mathbf{X}_s + \gamma_s + \lambda_t + \phi_s \times t + \epsilon_{st} \end{aligned} \quad (1)$$

Our shifter for new gun owners is the interaction term $\text{Delay}_s \times \text{Post1}_t$, which captures the specific effect of these laws during the demand shock. It consists of Delay_s which is a dummy variable for states requiring handgun buyers to observe a waiting period or to possess a permit/license according to Table 1.²² This state indicator is then interacted with Post1_t , a dummy for time periods starting after the Obama re-election on November 6th, 2012 and ending after April 17th, 2013 when the proposals for a renewed assault weapons ban and universal background checks were defeated in the U.S. Senate. Our primary coefficient of interest would thus be β_1 and capture the average proportionate difference in *HandgunSales* between *Delay* and *NoDelay* states during the demand shock. We also include a second interaction using the time dummy Post2_t which equals one for time periods starting after April 17th 2013 to investigate effects beyond the initial six months.

Apart from state and time fixed-effects γ_s and λ_t , the DiD regression also allows for state-specific linear trends $\phi_s \times t$ to account for the possibility that some states may deviate from the general upward trend in handgun background checks shown in Figure 1. Furthermore, our regression models each also feature a set of control variables \mathbf{X}_s . 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. ϵ_{st} denotes the residual term.

²²These 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.

A potential major shortcoming of the equation above is that it only accounts for location-specific, time-invariant components through state-fixed effects γ_s using within-state variation to estimate the effect of any time-varying factors such as $Delay_s \times Post1_t$. This approach, however, does not take into account potential seasonality that varies across locations. In order to remove any state-specific seasonal components, our main specification for estimating the effect on gun sales uses a seasonally differenced version of equation 1, where we subtract a 12 month lag from the dependent variable (denoted by Δ_{12}). Alternative specifications using levels and within-estimation (as in Equation 1) are reported in the Appendix.

$$\begin{aligned} \Delta_{12} \log(HandgunSales_{st}) = & \tilde{\alpha} + \tilde{\beta}_1(Delay_s \times Post1_t) + \tilde{\beta}_2(Delay_s \times Post2_t) \\ & + \tilde{\delta}_t \mathbf{X}_s + \tilde{\lambda}_t + \tilde{\phi}_s + \tilde{\epsilon}_{st} \end{aligned} \quad (2)$$

$$\begin{aligned} \Delta_{12} \log(Homicides_{ct}) = & \tilde{\alpha} + \tilde{\beta}_1(Delay_s \times Post1_t) + \tilde{\beta}_2(Delay_s \times Post2_t) \\ & + \tilde{\delta}_t \mathbf{X}_c + \tilde{\lambda}_t + \tilde{\phi}_c + \tilde{\epsilon}_{ct} \end{aligned} \quad (3)$$

Equation 3 is effectively the county-level analogue of Equation 2 and measures the effect of gun purchases in *Delay* states on homicide rates. 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.²³

A potential alternative to estimating equation 3 directly would be to use equation 2 as a first-stage in an instrumental variables regression with $\Delta_{12} \log(Homicides_{ct})$ 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 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 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

²³Each of these estimation decisions is reassessed in sections 5.1 and 6.2 and we provide supplementary results in the Appendix.

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.²⁴ 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 November 2009 and October 2013, exactly 36 months before and 12 months after the 2012 election.²⁵ Our 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. We address this issue in two ways: First, we test for differential changes in gun purchasing intentions proxied by Google searches for “gun store” to check the

²⁴Appendix Figures 8 and 9 depict the evolution in levels.

²⁵Note that after applying seasonal differencing, the nominal sample period starts in November 2010 and covers 24 months before and 12 months after treatment onset.

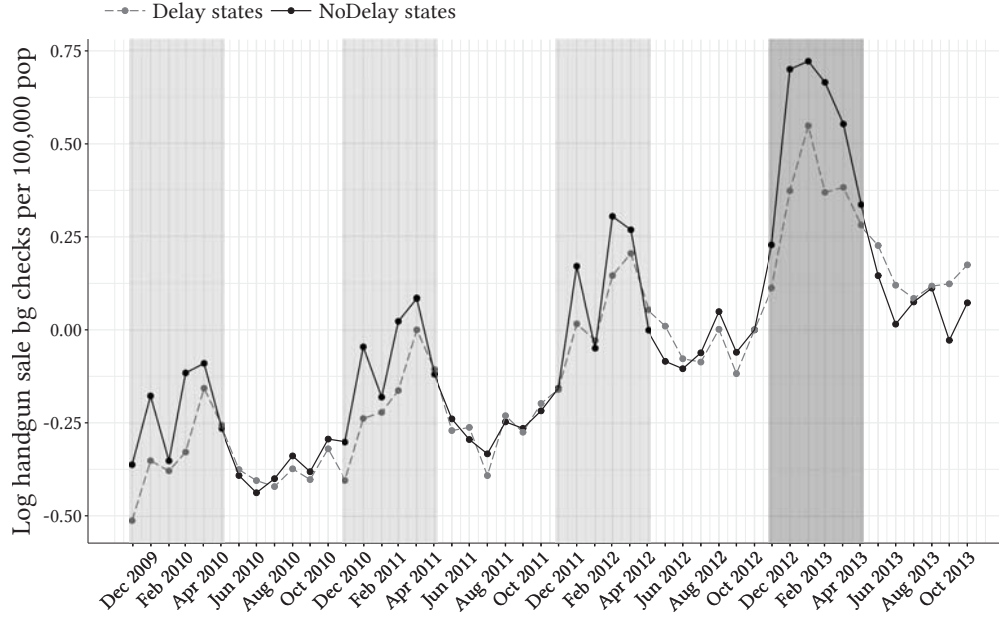


FIGURE 3: LOG BACKGROUND CHECK RATE FOR HANDGUNS IN *Delay* vs *NoDelay* STATES

Log of monthly NICS handgun background checks per 100,000 inhabitants in *Delay* states and *NoDelay* states between November 2009 and October 2013. The sample encompasses data for all states consistently included in our main specification. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. For better visibility, each series has been re-scaled to 0 on the last observation before the treatment.

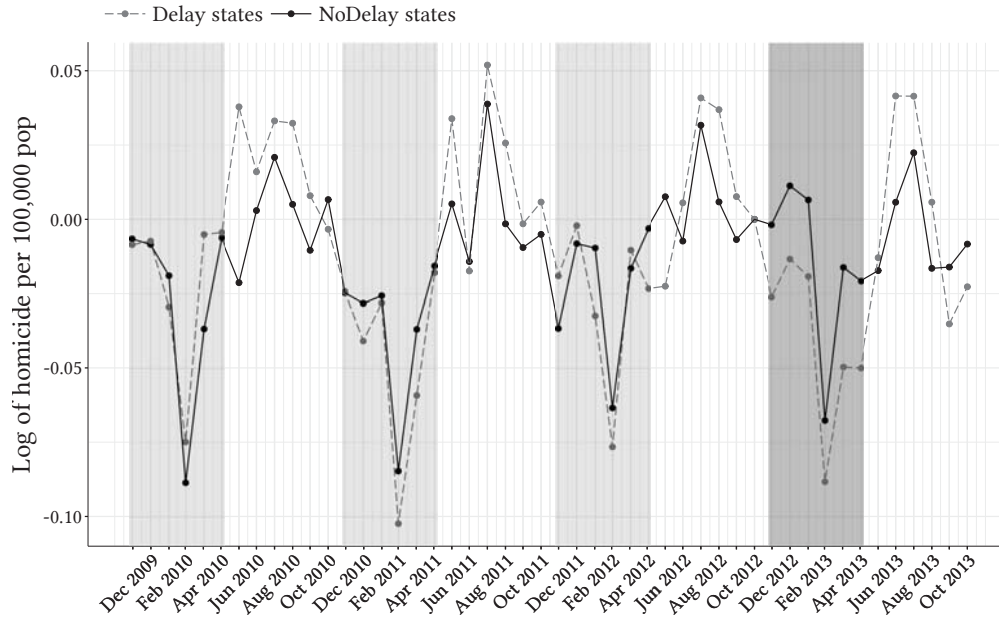


FIGURE 4: LOG HOMICIDE RATE IN *Delay* vs *NoDelay* STATES

Log of monthly homicides per 100,000 inhabitants in *Delay* states and *NoDelay* states between November 2009 and October 2013. The sample encompasses data from all counties consistently included in our main specification. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. For better visibility, each series has been re-scaled to 0 on the last observation before the treatment.

comparability of *Delay* and *NoDelay* states. Second, we corroborate the role of delay laws by running a series of horserace regressions where we add interactions of *Post1* and *Post2* with potential confounders such as other types of gun laws and correlates of gun popularity.

Another potential objection would be that our reduced-form effect on homicides in Equation 3 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 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 behind our results, 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. 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. The inclusion of $Delay_s \times Post2_t$ in the regression equation allows testing whether *Delay* states actually experienced comparatively fewer handgun purchases over the entire time period or if this is compensated by higher sales rates later.

TABLE 2: HANDGUN SALE BACKGROUND CHECKS

	Δ_{12} Log of background checks per 100,000 inhabitants							
	Handgun Sale				Total		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay \times Post	-0.027 (0.028)							
Delay \times Post1		-0.112*** (0.041)	-0.081* (0.044)	-0.073** (0.033)	-0.039 (0.027)	-0.030 (0.023)	0.009 (0.052)	0.021 (0.048)
Delay \times Post2		0.057 (0.062)	0.010 (0.064)	0.007 (0.084)	0.043 (0.055)	0.048 (0.062)	0.102 (0.096)	0.116 (0.094)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y	Y	Y
State Trends	N	N	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	1,516	1,516	1,516	1,516
R ²	0.429	0.446	0.539	0.594	0.689	0.724	0.678	0.757
$p(\beta_1 = -\beta_2)$		0.34	0.31	0.46	0.95	0.81	0.44	0.32

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

Table 2 reports the results from regressing seasonally differenced 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.²⁶ 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 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.

Adding controls in column 3 decreases both coefficients in size and yields now a marginally significant negative coefficient for β_2 . When accounting for potentially diverging pre-trends by adding state-specific linear time trends in column 4, the estimate

²⁶Appendix Table 15 compares the outcomes when reporting the dependent variable in logs and levels and without applying seasonal differencing. Appendix Table 14 shows regression results when including Connecticut and Michigan, when including Michigan with the mid-sample change in treatment assignment, when excluding all states where the NICS data contains outliers, and when using the same sample as for the NVSS data, BL2. The qualitative results remain the same.

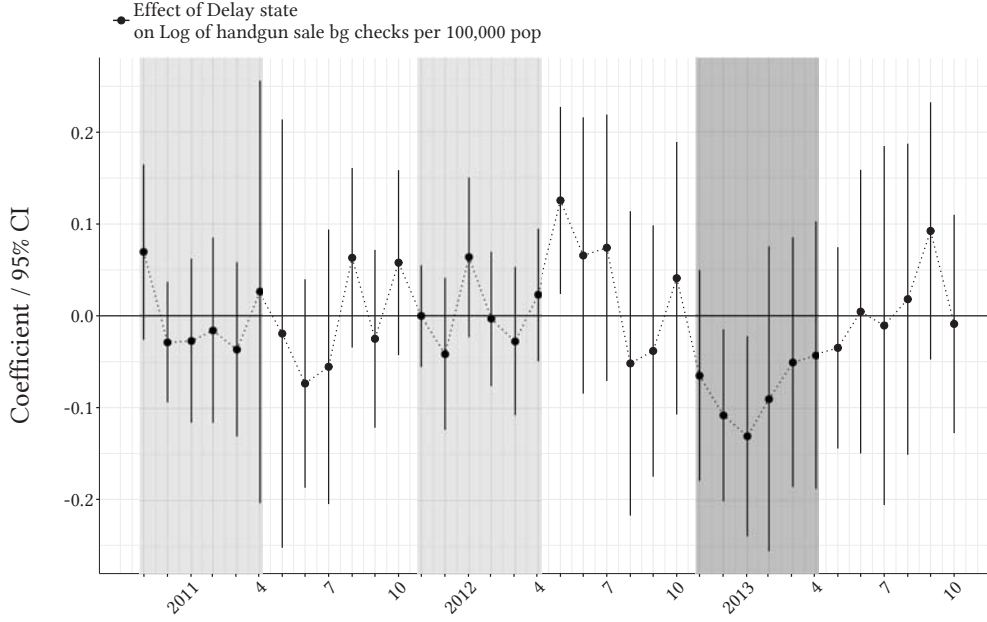


FIGURE 5: EVENT STUDY GRAPH FOR NICS BACKGROUND CHECKS

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log of NICS handgun background checks per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

for β_1 gains precision but stays virtually the same in size while β_2 decreases further. A very likely explanation for this result would be that specification 4 reduces the influence of diverging trends in smaller states without a large influence on the overall (weighted) coefficient. Our preferred estimate is the more conservative specification in columns 3 and 4. The results imply that sales rates were 7-8% lower in *Delay* states during the first six months than in *NoDelay* states. The p -values from the post-estimation Wald tests in the bottom row can never reject the hypothesis of a pure postponement effect due to the relatively large standard errors, despite the addition of controls in columns 3 and 4 eliminating most of the positive effect in the *Post2* period. We interpret this as tentative evidence that delay laws actually prevented consumers in *NoDelay* states from *buying* handguns rather than keeping them from merely *anticipating purchases*. Columns 5 to 8 show that firearm purchase delay laws only significantly affected background checks for handgun sales but not those for any other transactions.²⁷

One particular 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

²⁷Note that while the coefficients of *Handgun Sale* and *Other* would need to sum up to the coefficient of *Total* in the levels case, this does not need to be the case when using logs or log differences as in Table 2.

TABLE 3: HANDGUN BACKGROUND CHECKS (PLACEBO REGRESSIONS)

	Δ_{12} Log of handgun background checks per 100,000 inhabitants							
	Baseline (2010-2013)		-1 Year (2009-2012)		-2 Years (2008-2011)		Obama I (2006-2009)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.081*	-0.073**	-0.001	0.025	-0.007	0.052	-0.174	-0.164
	(0.044)	(0.033)	(0.029)	(0.046)	(0.020)	(0.038)	(0.119)	(0.129)
Delay×Post2	0.010	0.007	0.033	0.050	-0.003	0.054	-0.030	-0.020
	(0.064)	(0.084)	(0.054)	(0.067)	(0.055)	(0.062)	(0.057)	(0.071)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43	43
Observations	1,516	1,516	1,525	1,525	1,540	1,540	1,548	1,548
R ²	0.539	0.594	0.545	0.582	0.565	0.626	0.449	0.497

Notes: Observations are at the state-level. The sample period is November $x - 2$ until October $x + 1$, i.e. an asymmetric 36-month window 2 years before and 1 year after November of the year used as the event (x). Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

trends. We investigate this possibility using an event-study design based on specification 3 in Table 2 in which we allow for monthly treatment effects. 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 starts becoming increasingly negative and peaks in size in January 2013. After that, the coefficients gradually move back to the pre-period level and remain insignificant for the entire *Post2* period. Table 3 reports placebo tests where we shift both sample period and treatment date several years backward and demonstrate that this does not produce any significant results. There appears to be a similar effect of even higher magnitude for the period after the first Obama election in 2008 which is, however, not statistically significant at conventional levels.

We report additional robustness checks in the Appendix. In Table 14 we show that other plausible sample definitions yield qualitatively similar results. Figure 10 explores robustness with respect to the exclusion of specific states from the sample and Figures 11 and 12 with respect to the sample length. Figure 13 reports results from a permutation test with 10,000 iterations, where we randomly shuffle the assignment of *Delay* and *NoDelay* states and find that only 4% of the iterations yield more extreme

results than our baseline estimate. Table 15 shows results when using levels or a within-transformation instead of seasonal differencing. In Table 16 we explore different weighting options and show that our effect seems to predominantly arise from more populated states. Table 17 looks at different time trend specifications, such as quadratic trends, linear and quadratic trends estimated from the pre-event period only as suggested by Wolfers (2006). Finally, Table 18 demonstrates that using wild-cluster bootstrap standard errors as suggested by Cameron, Gelbach, and Miller (2008) for cases of only few clusters does not invalidate our results.

5.2 Alternative Channels

While the previous regression results appear fairly robust, there could be alternative explanations for why we observe a less pronounced spike in *Delay* states during the demand shock. A first legitimate question to ask is whether handgun purchases were actually *prevented* or simply *displaced*. While our regression results have not delivered a strong indication for 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. This variable is seasonally differenced by subtracting its 52nd lag.

The results reported in columns 1 through 4 of Table 4 reveal that, if anything, relative demand for gun shows was *falling* in *Delay* states during the gun demand shock.²⁸ 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.²⁹

²⁸Appendix Table 21 provides qualitatively similar findings in levels instead of logs.

²⁹Figure 14 in the appendix graphically depicts the evolution of Google searches. Section E.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.

TABLE 4: GOOGLE SEARCHES

	Δ_{52} Log of standardized share of Google searches							
	“gun show”				“gun store”			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post	-0.133** (0.065)				-0.193*** (0.067)			
Delay×Post1		-0.066 (0.078)	-0.026 (0.086)	-0.094 (0.128)		-0.205** (0.084)	0.037 (0.082)	-0.017 (0.090)
Delay×Post2		-0.195** (0.079)	-0.222** (0.097)	-0.290** (0.142)		-0.181** (0.092)	-0.027 (0.097)	-0.081 (0.133)
Year-Week FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	N	N	Y	Y
State Trends	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
R ²	0.140	0.140	0.257	0.276	0.112	0.112	0.230	0.247

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks, % hispanics and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

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, giving rise to omitted-variable bias. We test this possibility in two ways: the first uses a proxy for purchasing intentions to show that these were not different across the two groups of states. The second one checks if our results can be explained away by controlling for other gun laws and proxies for gun popularity.

We start by looking at Google searches for the term “gun store” which serves as a proxy for public interest in buying a gun. By doing so we rely on previous research by [Scott and Varian \(2014\)](#) that has identified this variable as a strong predictor for firearm purchasing intentions. Columns 5 to 8 in Table 4 repeat regression specifications from columns 1 through 4, but instead using Google searches for “gun store” as the dependent variable. Columns 5 and 6 seem to confirm the notion that consumers in *Delay* states reacted differently to those in *NoDelay* states in the aftermath of the

shock. Upon inclusion of our controls in columns 7 and 8, however, these significant differences completely disappear.^{30,31,32}

Our second set of robustness checks investigates whether the inclusion of alternative treatment variables can eliminate our baseline results. In particular, if other types of firearm legislation or proxies of pre-existing preferences for guns can entirely explain away our results, this would indicate that our estimates were in fact picking up the effect of such underlying heterogeneity. In Table 19 in the appendix we explore the impact of three different types of restrictive gun legislation: 1) prohibiting domestic violence offenders from possessing firearms, 2) background checks requiring to search state mental health records and 3) the *absence* of stand-your-ground laws which give immunity for shooting an intruder in one’s home.³³ While each of the three different types of firearm legislations reduces the coefficient for β_1 , it remains significant at least at the 10% level in all specifications including state trends.

In order to test whether our baseline results are driven by different levels of preferences for owning firearms, we run a similar series of horse-race regressions using different indicators of gun popularity: 1) gun stores per capita as of December 2010 calculated using data from the [Bureau of Alcohol, Tobacco, Firearms and Explosives \(2010\)](#), 2) estimates for the 2013 state-level rate of gun ownership from YouGov published by [Kalesan et al. \(2015\)](#) and 3) gunshows per capita in 2011³⁴. The results in Appendix Table 20 show that the inclusion of these variables only slightly weakens the baseline result but cannot wipe out the effect. We interpret this as additional evidence that our results are unlikely to be driven by unobserved heterogeneity across states in terms of gun legislation and preferences.

³⁰Figure 15 in the appendix shows the development of Google searches between November 2009 and October 2013 graphically. A regression using levels and producing similar results can be found in Appendix Table 22. Figure 16 also shows that these results do not depend on the chosen time window.

³¹To phrase this in the language of the model described in Appendix A, preferences for firearms ($\mu(\mathbf{x}_i) + \gamma_i$) differ across consumers from different states. The results in Table 4 imply, however, that we can assume the distribution of unobserved gun preferences $G(\gamma)$ to be the same across states after conditioning on observables.

³²The estimates in columns 7 and 8 are not driven by the inclusion of any specific variable in our set of controls. Results are reported in Table 30. Since we do not observe that the inclusion of these controls diminishes the effect on handgun sales in Table 2, but only of 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.

³³This data is also obtained from [Siegel et al. \(2017\)](#).

³⁴In light of our findings in Section E.1, we did not want to include gunshow data from 2012 in this exercise.

TABLE 5: HANDGUN BACKGROUND CHECKS DEPENDING ON DELAY LENGTH

Maximum delay length	Δ_{12} Log of background checks per 100,000 inhabitants									
	Baseline (=12 delay states)		$D \leq 30$ Drop NY (=11)		$D \leq 14$ Drop MD, NC, NJ (=8)		$D \leq 7$ Drop CA, DC (=6)		$D \leq 3$ Drop MN, RI (=4)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	-0.081* (0.044)	-0.073** (0.033)	-0.072 (0.049)	-0.073** (0.035)	-0.103* (0.053)	-0.087** (0.042)	-0.038 (0.046)	-0.048 (0.038)	-0.071** (0.036)	-0.075* (0.038)
Delay×Post2	0.010 (0.064)	0.007 (0.084)	0.013 (0.074)	-0.000 (0.094)	-0.001 (0.080)	-0.006 (0.116)	-0.091 (0.064)	-0.129 (0.105)	-0.131** (0.059)	-0.174 (0.120)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y	N	Y
States	43	43	42	42	39	39	37	37	35	35
Observations	1,516	1,516	1,480	1,480	1,374	1,374	1,302	1,302	1,230	1,230
R ²	0.539	0.594	0.546	0.600	0.561	0.605	0.608	0.659	0.613	0.663
$p(\beta_1 = -0.081)$			0.87	0.82	0.68	0.88	0.35	0.4	0.79	0.88

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Included control variables are log(population), % rural, % below poverty line, % blacks, % hispanics and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

5.3 Proposed Mechanism: Time Inconsistency

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.³⁵ 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, if consumers discount exponentially and do not suffer from time inconsistency, we would expect the demand shock to induce strong differences in gun sales between states with long and short delays and little to no differences between states with short delays and no delays. The rationale is that, under exponential discounting, longer waits for the receipt of a firearm come with diminished utility from using the gun. With time inconsistency (e.g. due to present bias) the presence or absence of any delay should matter more than the delay's actual length. Table 5 uses the two main

³⁵We formally derive predictions with a model in Appendix Section A.

specifications from Table 2, with each new column pair excluding states with a certain delay length. Columns 1 through 6 show mostly significant coefficients between -0.072 and -0.103 as we gradually reduce the maximum delay length to 14 days, which arguably constitutes a short delay already. Reducing the maximum delay length further to 7 days makes the coefficient decrease to an insignificant -0.038 and -0.048 , depending on the inclusion of state trends. Given the results in Figure 10, this seems to be mainly driven by removing the largest *Delay* state, California, and thus assigning more weight to other states in the regressions. Excluding Minnesota and Rhode Island, which restricts the maximum delay length to 3 days, increases the estimates again to a significant -0.071 and -0.075 , respectively.³⁶

In sum, the findings above suggest that time inconsistency is more likely to drive gun buyers during this gun demand shock than fully rational deliberations. Particularly noteworthy is that the most restrictive version in columns 9 and 10 yields results very close to the full sample ones in 1 and 2. In addition, one cannot reject the hypothesis that β_1 is identical to -0.081 of specification 1 for any of the restricted-delay samples.

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.³⁷ Table 6 shows the results from regression equation 3.³⁸ Observations are now at the county level, and the sample includes all states which were previously omitted due to measurement error in the background check data.³⁹

³⁶Minnesota witnessed an intense gun control debate in early 2013 which may have triggered the second demand shock captured by the raw data in Appendix Section G (NRA, 2013). Attaching more weight to this state as in columns 7 and 8 should therefore attenuate the overall effect.

³⁷Appendix Section E.2 investigates the effect on crime other than murder, providing a test of the “more guns, less crime” hypothesis. We do not find evidence for a strong deterrence effect of guns on any type of crime. Appendix Section E.3 investigates the effect on suicides and accidents. While we do not detect any impact on accidents, suicides seem to evolve similarly to homicides, although to a lesser extent and not with sufficient statistical precision. This is not surprising as we focus on short-run effects and the fraction of consumers responding to the demand shock *and* becoming suicidal within 6 months may be quite small.

³⁸Appendix Table 23 displays regression results in levels instead of logs and without seasonal differencing.

³⁹We still exclude Connecticut because of identification concerns, and Michigan because the state switched treatment assignment in the period of observation. Appendix Table 25 shows that results are very similar when expanding the sample to include Connecticut and Michigan, when only including

Column 1 shows that *Delay* states experienced a significant relative drop in gun homicide rates by 1.9% after the start of the firearm demand shock. Column 2 reveals that this effect is mostly concentrated and statistically significant only during our treatment period *Post1*. The estimate for this time period implies a 2.4% relative reduction and an insignificant effect of 1.4% for the *Post2* period. Controlling for observables in column 3 implies a significant 2.2% relative drop in *Delay* states' handgun homicide rates during the treatment period *Post1* and an insignificant relative decline of 1.8% in *Post2*. The inclusion of county trends in column 4 mainly leads to a loss in precision but only slightly diminishes β_1 to -0.019 , which is still significant at 5%.

Columns 5 and 6 show that the *Post1* effect for handgun homicides is also reflected in decreased aggregate homicide rates of about the same magnitude. This effect is significant at the 5% level in our preferred setup and without county trends. Notably, there is virtually no impact of delay laws on overall homicides in the *Post2* period. The reason for this becomes apparent when looking at the results for non-handgun homicides in specifications 7 and 8. Here we see a significant *increase* which explains the zero-effect in the aggregate homicide rates during *Post2*. A straightforward explanation could be that the reaction of *NoDelay* states reflects two different channels through which increased handgun ownership can affect homicides. One would be a *lethality effect* by which random acts of aggression or anger turn into the shooting and killing of another person. The other effect would be a *substitution effect* whereby homicides are simply carried out by handguns instead of other weapons with no aggregate effect. Our results are indicative of both effects as non-handgun homicides significantly increase in *Post2*, but we also observe a significant increase in aggregate homicides in *Post1*.

In line with our research question, the remainder of this paper focusses on the lethality effect and the impact of delay laws on handgun-related homicides during the *Post1* period. As firearm purchase delay legislation is often intended 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.

Michigan with a mid-sample change of treatment assignment, when removing those states with NICS outliers, and when using sample BL1.

TABLE 6: BASELINE: HOMICIDE RATES

	Δ_{12} Log of homicides per 100,000 inhabitants							
	Handgun				Any		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post	-0.019** (0.008)							
Delay×Post1		-0.024*** (0.009)	-0.022*** (0.008)	-0.019** (0.010)	-0.024** (0.012)	-0.021 (0.016)	-0.002 (0.010)	-0.001 (0.013)
Delay×Post2		-0.014 (0.012)	-0.018 (0.015)	-0.015 (0.018)	0.002 (0.017)	0.005 (0.023)	0.022*** (0.008)	0.024** (0.011)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y	Y	Y
County Trends	N	N	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,047	3,047	3,047	3,047	3,047	3,047
Observations	109,692	109,692	109,692	109,692	109,692	109,692	109,692	109,692
R ²	0.002	0.002	0.008	0.019	0.006	0.016	0.005	0.014

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 7: PLACEBO REGRESSIONS OF HOMICIDE RATES

	Δ_{12} Log of homicides per 100,000 inhabitants							
	Baseline (2010-2013)		-1 Year (2009-2012)		-2 Years (2008-2011)		Obama I (2006-2009)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.022*** (0.008)	-0.019** (0.010)	-0.001 (0.014)	-0.007 (0.016)	-0.008 (0.011)	-0.014 (0.014)	-0.004 (0.011)	-0.002 (0.013)
Delay×Post2	-0.018 (0.015)	-0.015 (0.018)	-0.008 (0.007)	-0.014 (0.010)	0.006 (0.011)	0.001 (0.015)	0.001 (0.012)	0.003 (0.014)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y
R ²	0.008	0.019	0.008	0.019	0.009	0.021	0.044	0.056

Notes: All regressions use 109,692 observations from 3,047 counties. The sample period is November $x - 2$ until October $x + 1$, i.e. an asymmetric 36-month window 2 years before and 1 year after November of the year used as the event (x). Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

6.2 Robustness and Sensitivity Checks

In order to ensure that our findings of lower handgun homicide rates in *Delay* states during the period of high firearm demand are not a statistical artifact, we examine their robustness and sensitivity in a similar way as we did for the results on handgun background checks. 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. During our treatment period *Post1*, however, there is a clear negative impact for November 2012, December 2012 and March 2013. Figure 7 instead shows no systematic effect on non-handgun homicides before or during our treatment. The positive effect observed during *Post2* in the baseline regressions applies to all months during this period, but is only statistically significant for July 2013.

Second, we run placebo regressions similar to those in Section 5.1. In columns 3 to 6 of Table 7, we repeat our main regressions but pre-date our sample and treatment periods by 1 and 2 years. The results turn out insignificant and much closer to zero, suggesting that the previously uncovered effect of delay laws on handgun-related homicides can only be observed during this specific treatment period. Relatedly, we consider whether our estimates may reflect divergent 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 and 8 of Table 7 do not yield any significant or sizable coefficients which would indicate that violent reactions or racial tensions were driving our homicide results. In sum, both tests provide evidence against correlated shocks being responsible for our findings.

We provide numerous further robustness checks in the Appendix. Table 25 shows that our results hold for different sample definitions, including the one used for the NICS data. The exclusion of single states at a time reported in Figure 17 also does not seem to affect our estimates much. 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. Next, we narrow the chosen time window sequentially to check whether we picked a period that is particularly favorable to generate

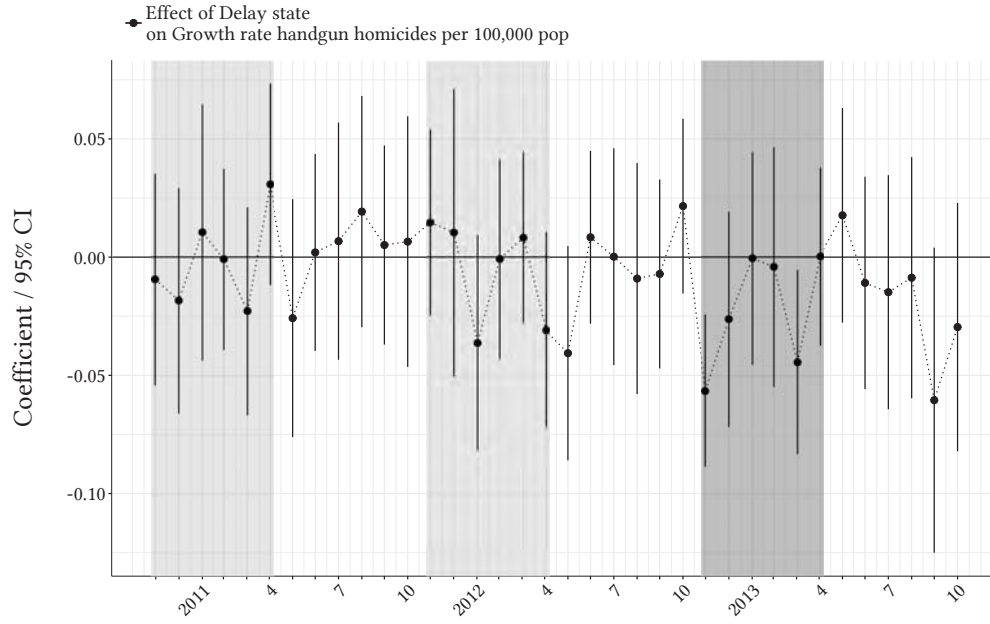


FIGURE 6: EVENT STUDY GRAPH FOR HANDGUN HOMICIDE RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log handgun homicide per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

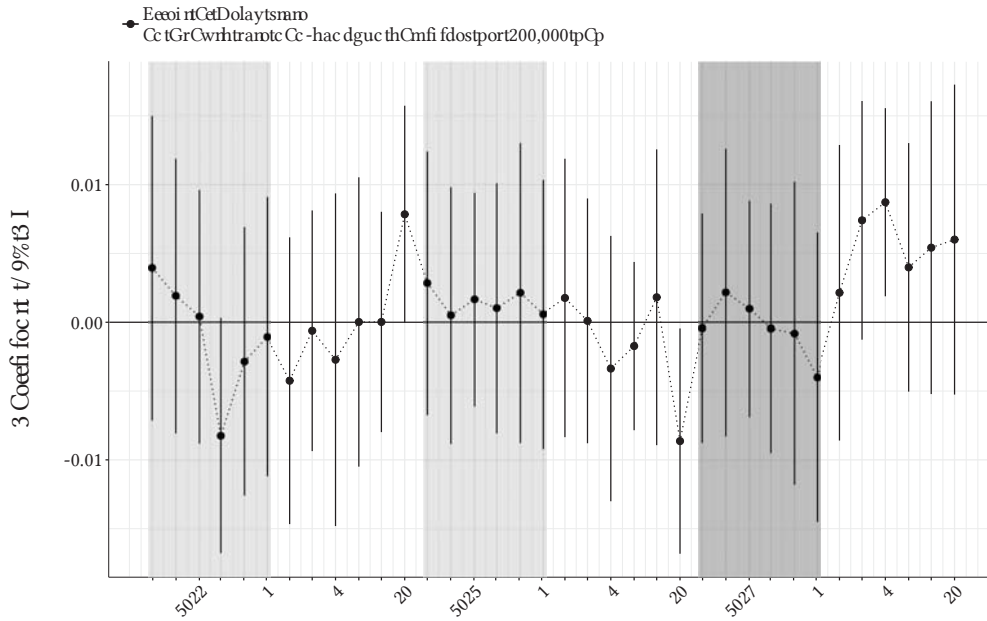


FIGURE 7: EVENT STUDY GRAPH FOR NON-HANDGUN HOMICIDE RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log non-handgun homicide per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

TABLE 8: EFFECT ON HOMICIDE RATES: VICTIM AGE

Victim age	Δ_{12} Log of handgun homicides per 100,000 inhabitants															
	Any		< 10		10-19		20-29		30-39		40-49		50-59		≥ 60	
Delay \times Post1	(1) -0.022*** (0.008)	(2) -0.019** (0.010)	(3) -0.000 (0.001)	(4) 0.000 (0.001)	(5) 0.003 (0.004)	(6) 0.003 (0.004)	(7) -0.017*** (0.006)	(8) -0.014** (0.007)	(9) 0.001 (0.004)	(10) -0.002 (0.004)	(11) -0.004 (0.003)	(12) -0.005 (0.004)	(13) -0.002 (0.003)	(14) -0.004 (0.004)	(15) -0.002 (0.002)	(16) -0.003 (0.003)
Delay \times Post2	(1) -0.018 (0.015)	(2) -0.015 (0.018)	(3) 0.001 (0.001)	(4) 0.002 (0.001)	(5) -0.009* (0.005)	(6) -0.009 (0.005)	(7) -0.001 (0.008)	(8) 0.002 (0.009)	(9) 0.001 (0.005)	(10) 0.002 (0.004)	(11) -0.005 (0.006)	(12) -0.006 (0.007)	(13) -0.002 (0.004)	(14) -0.003 (0.005)	(15) -0.003 (0.003)	(16) -0.004 (0.003)
Year-Month FE	Y	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	Y
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
R ²	0.008	0.019	0.006	0.015	0.011	0.021	0.012	0.023	0.006	0.016	0.006	0.015	0.005	0.014	0.004	0.013

Notes: All regressions use 109,692 observations from 3,047 counties. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: * p<0.1; ** p<0.05; *** p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

our results. Figures 18 and 19 show how the estimate on handgun homicides for the *Post1* period changes when reducing the sample along the time dimension. Figure 20 shows estimated coefficients from a permutation test with 10,000 iterations, in which we randomly reshuffle each state’s designation as either *Delay* or *NoDelay*. We find that only 1.8% of all generated coefficients are more extreme than our estimates. Concerns about functional form and data transformation are addressed in Table 23, which features results in levels and using within-transformation. Different weighting options are explored in Table 24 revealing a similar pattern as Table 16. Table 26 shows results under the assumption of quadratic trends and trends estimated only from the pre-treatment period and Table 27 reports standard errors using the wild-cluster bootstrap procedure.

The usage of county data allows us to perform two additional checks: First, we remove counties in *Delay* states that border *NoDelay* states in Table 28. We obtain slightly smaller results which provides evidence against spillover effects. Second, Table 29 reports results from a regression at the state level. We obtain a significant effect, despite a drop in sample size from 109,692 to 1,764 observations and reduced statistical power.

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 up incidents in 10-year age bins of victims. Columns 7 and 8 show that only the age group 20 to 29 stands out, accounting for about 3/4 of the baseline effect. Bearing this in mind, Table 9 breaks the results down by victim sex with a particular focus on the 20 to 29 age group. Here we see that men make up about 2/3 of the victims while women account for 1/3. The results for female victims, however, are more precisely estimated. In both categories victims are predominantly aged 20 to 29. Next, we split up the homicide rates by race of victim with results

TABLE 9: EFFECT ON HOMICIDE RATES: VICTIM SEX

Victim sex	Δ_{12} Log of handgun homicides per 100,000 inhabitants									
	Any		Male				Female			
	Any		Any		20-29		Any		20-29	
Victim age	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	-0.022***	-0.019**	-0.013	-0.011	-0.011*	-0.008	-0.009**	-0.008*	-0.006***	-0.006**
	(0.008)	(0.010)	(0.008)	(0.009)	(0.006)	(0.006)	(0.003)	(0.005)	(0.002)	(0.002)
Delay×Post2	-0.018	-0.015	-0.018	-0.016	0.002	0.005	0.002	0.002	-0.002	-0.002
	(0.015)	(0.018)	(0.013)	(0.015)	(0.007)	(0.008)	(0.005)	(0.007)	(0.002)	(0.003)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
R ²	0.008	0.019	0.008	0.020	0.012	0.023	0.005	0.014	0.007	0.016

Notes: All regressions use 109,692 observations from 3,047 counties. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 10: EFFECT ON HOMICIDE RATES: VICTIM RACE

Victim race	Δ_{12} Log of handgun homicides per 100,000 inhabitants									
	Any		White		Black		Hispanic		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	-0.022***	-0.019**	-0.011**	-0.012*	-0.014	-0.009	0.002	0.001	0.001	0.000
	(0.008)	(0.010)	(0.006)	(0.007)	(0.008)	(0.009)	(0.005)	(0.005)	(0.002)	(0.003)
Delay×Post2	-0.018	-0.015	-0.002	-0.003	-0.008	-0.004	-0.006	-0.007	-0.004	-0.004
	(0.015)	(0.018)	(0.007)	(0.008)	(0.009)	(0.010)	(0.006)	(0.007)	(0.003)	(0.003)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
R ²	0.008	0.019	0.003	0.012	0.009	0.021	0.014	0.022	0.007	0.018

Notes: All regressions use 109,692 observations from 3,047 counties. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

displayed in Table 10. One can see that the drop in handgun homicides is about equally shared between the “*White*” and “*Black*” categories. For the former, the results are also statistically significant at the 5 and 10% level depending on the inclusion of county trends. The “*Hispanics*” and “*Other*” categories do not seem to respond to the treatment.

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 29 and are predominantly white or black. As the additional gun buyers in *NoDelay* states may have purchased their firearm based on impulses, we start by investigating whether domestic violence may play a role. To do so, we first split the handgun homicide victims into those who were shot in their home and those who were assaulted elsewhere. Table 11 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 12, we present the results from the UCR SHR data on the particular circumstances of a homicide.⁴⁰ Columns 1 to 2 show the baseline specification for handgun homicides reported in the UCR SHR and then split these into specific murder circumstances. The results for aggregate handgun homicides have the same sign as those using the NVSS data but are only about 2/3 in size and insignificant. The results in columns 3 and 4, however, indicate that deadly assaults related to arguments account for the main part of the additional handgun homicides in *NoDelay* states. Unlike for the aggregate handgun murder rate, this effect is also highly significant. All other types of homicide circumstances such as brawls, (organized) crime and defense as well as other/undetermined, do not seem to be systematically affected during the *Post1* period. In Table 13 we make use of data on the offenders provided in the UCR SHR. The results clearly show that even though the victims can be of both sexes, most of the additional argument-related handgun murders in *NoDelay* are committed by men.

⁴⁰ As outlined in Sections 3.2 and E.2, this data exhibits a more restricted coverage. Appendix Table 31 shows that the UCR SHR data yield qualitatively very similar estimates compared to the NVSS data in our *Post1* period of interest. A map illustrating the exact coverage for the UCR data is shown in Figure 39.

TABLE 11: EFFECT ON HOMICIDE RATES: PLACE OF ASSAULT

Victim sex	Δ_{12} Log of handgun homicides per 100,000 inhabitants							
	Male				Female			
	Home		Not Home		Home		Not Home	
Place of assault	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	0.006 (0.008)	0.004 (0.008)	-0.018* (0.009)	-0.014 (0.011)	-0.007** (0.003)	-0.008** (0.004)	-0.001 (0.002)	0.000 (0.003)
Delay×Post2	-0.012* (0.007)	-0.014* (0.008)	-0.009 (0.011)	-0.004 (0.012)	0.002 (0.005)	0.001 (0.006)	0.000 (0.002)	0.001 (0.003)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y
R ²	0.006	0.017	0.011	0.023	0.004	0.014	0.006	0.016

Notes: All regressions use 109,692 observations from 3,047 counties. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 12: MURDER REPORTS: CIRCUMSTANCES

Circumstances	Δ_{12} Log of handgun murders per 100,000 inhabitants									
	Any		Arguments		Brawls		Gang, Felony or Defense		All Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	-0.014 (0.011)	-0.013 (0.013)	-0.010*** (0.004)	-0.016*** (0.006)	0.002 (0.001)	0.002 (0.001)	-0.000 (0.009)	-0.008 (0.010)	-0.008 (0.011)	0.005 (0.009)
Delay×Post2	0.003 (0.019)	0.004 (0.025)	-0.007 (0.005)	-0.013 (0.008)	-0.000 (0.001)	-0.000 (0.001)	-0.006 (0.008)	-0.014 (0.013)	0.018 (0.013)	0.031* (0.017)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
Counties	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229
Observations	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244
R ²	0.010	0.021	0.009	0.020	0.013	0.023	0.020	0.041	0.010	0.027

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 13: MURDER REPORTS: OFFENDER SEX

Offender sex	Δ_{12} Log of argument-related handgun murders per 100,000 inhabitants					
	Any		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)
Delay×Post1	-0.010*** (0.004)	-0.016*** (0.006)	-0.008** (0.003)	-0.013** (0.005)	-0.002 (0.001)	-0.002 (0.001)
Delay×Post2	-0.007 (0.005)	-0.013 (0.008)	-0.008* (0.005)	-0.013* (0.008)	0.001 (0.001)	0.001 (0.002)
Year-Month FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y
Counties	2,229	2,229	2,229	2,229	2,229	2,229
Observations	80,244	80,244	80,244	80,244	80,244	80,244
R ²	0.009	0.020	0.009	0.020	0.005	0.015

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

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 29, and often as a result of arguments. Homicides of men, instead, happened primarily outside their home, also largely because of arguments. Similar to women, male victims are typically 20-29 years old. Virtually all of the additional homicides are committed by men. In terms of mechanisms, our findings suggest domestic violence as a possible explanation for many of the female homicides and other *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 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. 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, providing evidence against an entirely rational explanation of consumer behavior. 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 2% which in turn implies that about 200 homicides could have been “prevented” during the six-month *Post1* period if all U.S. states had had some sort of purchase delay law in place. This is clearly less than the estimates by [Luca, Malhotra, and Poliquin \(2017\)](#), which imply 465 “prevented” homicides for the same period following the introduction of delay laws. Our estimated 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 may 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 Modeling Firearm Purchases in the Presence of Delay Laws

To understand why 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.⁴¹

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 :

$$\nu(\mu(\mathbf{x}_i), \gamma_i, \omega_t) = [\mu(\mathbf{x}_i) + \gamma_i]\omega_t \quad (4)$$

$$\mathbb{E}_{H_t}[\nu(\mu(\mathbf{x}_i), \gamma_i, \omega_t)] = [\mu(\mathbf{x}_i) + \gamma_i]\mathbb{E}_{H_t}[\omega_t] \quad (5)$$

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 $\mu(\mathbf{x}_i)$ 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

⁴¹The 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 \(2017\)](#).

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 4 then refers to the actual utility, while Equation 5 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 .⁴² 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 $D_s \geq 0$.⁴³ i 's expected net present value then reads:

$$\begin{aligned} U_{i0s} &= \sum_{t=D_s}^{D_s+T} \delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\mathbf{x}_i), \gamma_i, \omega_t)] \\ &= [\mu(\mathbf{x}_i) + \gamma_i] \Psi_{i0s} \\ \text{with} \quad \Psi_{i0s} &\equiv \sum_{t=D_s}^{D_s+T} \delta_i^t \mathbb{E}_{H_t}[\omega_t] \end{aligned} \tag{6}$$

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

⁴²That 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.

⁴³Throughout 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.

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:

$$\begin{aligned}
P[\text{Buy}_{i0s}] &= P[U_{i0s} - p_s - c_s > 0] \\
&= P[\mu(\mathbf{x}_i) + \gamma_i \Psi_{i0s} - p_s - c_s > 0] \\
&= P\left[\gamma_i > \frac{p_s + c_s}{\Psi_{i0s}} - \mu(\mathbf{x}_i)\right] \\
&= P[\gamma_i > \bar{\gamma}_{i0s}]
\end{aligned} \tag{7}$$

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 \mathbf{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.⁴⁴ 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 $P[\text{Buy}_{i0s}]$ 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 $P[\text{Buy}_{i0s}]$ 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 $P[\text{Buy}_{i0s}]$ between states with and without delays.
- R4. There should be no response in $P[\text{Buy}_{i0s}]$ 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

⁴⁴To 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.

$\mathbb{E}_{H_0}[\omega_0]$, 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 $P[\text{Buy}_{i0s}]$ will be amplified by shocks to instrumental utility. Finally, prediction R4 claims that the differences prescribed by R3 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.⁴⁵

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 \(1999\)](#).⁴⁶

⁴⁵This 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.

⁴⁶The 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](#)).

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

$$\tilde{\nu}(\mu(\mathbf{x}_i), \gamma_i, \omega_t | \omega_0) = [\mu(\mathbf{x}_i) + \gamma_i][(1 - \alpha_i)\omega_t + \alpha_i\omega_0] \quad (8)$$

$$\mathbb{E}_{H_t}[\tilde{\nu}(\mu(\mathbf{x}_i), \gamma_i, \omega_t | \omega_0)] = [\mu(\mathbf{x}_i) + \gamma_i][(1 - \alpha_i)\mathbb{E}_{H_t}[\omega_t] + \alpha_i\omega_0] \quad (9)$$

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

$$\begin{aligned} \tilde{U}_{i0s} &= \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\mathbf{x}_i), \gamma_i, \omega_t | \omega_0)] \\ &= [\mu(\mathbf{x}_i) + \gamma_i] \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t [(1 - \alpha_i)\mathbb{E}_{H_t}[\omega_t] + \alpha_i\omega_0] \\ &= [\mu(\mathbf{x}_i) + \gamma_i] \tilde{\Psi}_{i0s} \end{aligned} \quad (10)$$

$$\text{with} \quad \tilde{\Psi}_{i0s} \equiv (1 - \alpha_i)\bar{\Psi}_{i0s} + \alpha_i\tilde{m}_{i0s}$$

$$\text{and} \quad \bar{\Psi}_{i0s} \equiv \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_t}[\omega_t]$$

$$\text{and} \quad \tilde{m}_{i0s} \equiv \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \omega_0 = \delta_i^{D_s} \left[\beta_i^{\mathbb{1}(D_s>0)} + \beta_i \delta_i \frac{1 - \delta_i^T}{1 - \delta_i} \right] \omega_0$$

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

$$\begin{aligned} P[\tilde{U}_{i0s} - p_s - c_s > 0] &= P[\mu(\mathbf{x}_i) + \gamma_i] \tilde{\Psi}_{i0s} - p_s - c_s > 0 \\ &= P\left[\gamma_i > \frac{p_s + c_s}{\tilde{\Psi}_{i0s}} - \mu(\mathbf{x}_i)\right] \\ &= P[\gamma_i > \tilde{\gamma}_{i0s}] \end{aligned} \quad (11)$$

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 disproportionately

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:

$$\begin{aligned}
& P[\tilde{U}_{i0s} - p_s - c_s > \tilde{U}_{i1s} - \beta_i \delta_i p_s - \beta_i \delta_i c_s] \\
&= P \left[\sum_{t=D_s}^{D_s+T} \beta_i^{\mathbf{1}(t>0)} \delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\mathbf{x}_i), \gamma_i, \omega_t | \omega_0)] - p_s - c_s \right. \\
&\quad \left. > \sum_{t=D_s+1}^{D_s+T+1} \beta_i \delta_i^t \mathbb{E}_{H_t}[\tilde{\nu}(\mu(\mathbf{x}_i), \gamma_i, \omega_t | \omega_0)] - \beta_i \delta_i p_s - \beta_i \delta_i c_s \right] \\
&= P \left[[\mu(\mathbf{x}_i) + \gamma_i] \Delta \tilde{\Psi}_{i0s} > (1 - \beta_i \delta_i)(p_s + c_s) \right] \\
&= P \left[\gamma_i > \frac{(1 - \beta_i \delta_i)(p_s + c_s)}{\Delta \tilde{\Psi}_{i0s}} - \mu(\mathbf{x}_i) \right] \\
&= P[\gamma_i > \tilde{\gamma}_{i0s}]
\end{aligned} \tag{12}$$

$$\text{with} \quad \Delta \tilde{\Psi}_{i0s} \equiv (1 - \alpha_i) \Delta \bar{\Psi}_{i0s} + \alpha_i \Delta \tilde{m}_{i0s}$$

$$\text{and} \quad \Delta \bar{\Psi}_{i0s} \equiv \delta_i^{D_s} \left[\beta_i^{\mathbf{1}(D_s>0)} \mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \beta_i \delta_i^{T+1} \mathbb{E}_{H_{T+D_s+1}}[\omega_{T+D_s+1}] \right]$$

$$\text{and} \quad \Delta \tilde{m}_{i0s} \equiv \delta_i^{D_s} \left[\beta_i^{\mathbf{1}(D_s>0)} - \beta_i \delta_i^{T+1} \right] \omega_0$$

Since a behavioral agent needs to have a positive lifetime utility *and* decide buying today, her probability of purchasing is somewhat more complex: $P[\text{Buy}_{i0s}] = P[\tilde{U}_{i0s} - p_s - c_s > 0 \cap \tilde{U}_{i0s} - p_s - c_s > \tilde{U}_{i1s} - \beta_i \delta_i p_s - \beta_i \delta_i c_s]$. As shown in Appendix Section B, the lifetime utility constraint $\tilde{U}_{i0s} - p_s - c_s > 0$ is highly unlikely to be ever binding such that $P[\text{Buy}_{i0s}] \approx P[\gamma_i > \tilde{\gamma}_{i0s}]$. 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 $P[\text{Buy}_{i0s}]$ 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 $P[\text{Buy}_{i0s}]$ 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 ($\alpha_i \rightarrow 1$), this substantial difference should also hold during demand shocks.

- B3. An increase in ω_0 will disproportionately increase the differences in $P[\text{Buy}_{i0s}]$ between states with and without delays (for any value of α_i and β_i).
- B4. There should be no response in $P[\text{Buy}_{i0s}]$ 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 $\beta_i \rightarrow 1$, 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 main text, we will make a case for the patterns in the data being more consistent with many gun buyers behaving time-inconsistently.

B Additional Theoretical Derivations

The purchasing probability of a behavioral agent in period 0 as stated in Equation 10 can be rewritten as:

$$\begin{aligned}
P[Buy_{i0s}] &= P[U_{i0s} - p_s - c_s > 0 \cap \tilde{U}_{i0s} - p_s - c_s > \tilde{U}_{i1s} - \beta_i \delta_i p_s - \beta_i \delta_i c_s] \\
&= P[\gamma_i > \tilde{\gamma}_{i0s} \cap \gamma_i > \tilde{\gamma}_{i0s}] \\
&= \left(1 - P[\tilde{\gamma}_{i0s} > \tilde{\gamma}_{i0s}]\right) \times P[\gamma_i > \tilde{\gamma}_{i0s}] + P[\tilde{\gamma}_{i0s} > \tilde{\gamma}_{i0s}] \times P[\gamma_i > \tilde{\gamma}_{i0s}]
\end{aligned} \tag{13}$$

Both threshold levels $\tilde{\gamma}_{i0s}$ and $\tilde{\gamma}_{i0s}$ 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 $\tilde{\gamma}_{i0s} > \tilde{\gamma}_{i0s}$ using specific parameter values, we first substitute in the components of both thresholds, using the assumption that $\Delta\tilde{\Psi}_{i0s} > 0$, and simplify:

$$\begin{aligned}
&\tilde{\gamma}_{i0s} > \tilde{\gamma}_{i0s} \tag{14} \\
&\frac{(1 - \beta_i \delta_i)(p_s + c_s)}{\Delta\tilde{\Psi}_{i0s}} - \mu(\mathbf{x}_i) - \frac{p_s + c_s}{\tilde{\Psi}_{i0s}} + \mu(\mathbf{x}_i) > 0 \\
&(1 - \beta_i \delta_i)\tilde{\Psi}_{i0s} - \Delta\tilde{\Psi}_{i0s} > 0 \\
&(1 - \alpha_i)\left[(1 - \beta_i \delta_i)\tilde{\Psi}_{i0s} - \Delta\tilde{\Psi}_{i0s}\right] + \alpha_i\left[(1 - \beta_i \delta_i)\tilde{m}_{i0s} - \Delta\tilde{m}_{i0s}\right] > 0
\end{aligned}$$

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 10 and simplify as follows:

$$\begin{aligned}
(1 - \beta_i \delta_i) \bar{\Psi}_{i0s} &> \Delta \bar{\Psi}_{i0s} \tag{15} \\
(1 - \beta_i \delta_i) \sum_{t=D_s}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_t}[\omega_t] &> \delta_i^{D_s} \left[\beta_i^{\mathbb{1}(D_s>0)} \mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \beta_i \delta_i^{T+1} \mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}] \right] \\
(1 - \beta_i \delta_i) \sum_{t=D_s+1}^{D_s+T} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_t}[\omega_t] &> \beta_i \beta_i^{\mathbb{1}(D_s>0)} \delta_i^{D_s+1} \mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \beta_i \delta_i^{D_s+T+1} \mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}] \\
(1 - \beta_i \delta_i) \sum_{t=0}^{T-1} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_{t+D_s+1}}[\omega_{t+D_s+1}] &> \beta_i^{\mathbb{1}(D_s>0)} \mathbb{E}_{H_{D_s}}[\omega_{D_s}] - \delta_i^T \mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}] \\
\mathbb{E}_{H_{D_s}}[\omega_{D_s}] &< \frac{(1 - \beta_i \delta_i) \sum_{t=0}^{T-1} \beta_i^{\mathbb{1}(t>0)} \delta_i^t \mathbb{E}_{H_{t+D_s+1}}[\omega_{t+D_s+1}] + \delta_i^T \mathbb{E}_{H_{D_s+T+1}}[\omega_{D_s+T+1}]}{\beta_i^{\mathbb{1}(D_s>0)}}
\end{aligned}$$

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:

$$\begin{aligned}
\frac{\mathbb{E}_{H_{D_s}}[\omega_{D_s}]}{\kappa} &< \frac{(1 - \beta_i \delta_i) \frac{1 - \delta_i^T}{1 - \delta_i} + \delta_i^T}{\beta_i^{\mathbb{1}(D_s>0)}} \tag{16} \\
\frac{\omega_0}{\kappa} &< 222.77 \quad \text{for } D_s = 0 \\
\frac{\mathbb{E}_{H_{D_s}}[\omega_{D_s}]}{\kappa} &< 247.52 \quad \text{for } D_s > 0
\end{aligned}$$

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:

$$\begin{aligned}
(1 - \beta_i \delta_i) \tilde{m}_{i0s} &> \Delta \tilde{m}_{i0s} \tag{17} \\
(1 - \beta_i \delta_i) \delta_i^{D_s} \left[\beta_i^{\mathbf{1}(D_s > 0)} + \beta_i \delta_i \frac{1 - \delta_i^T}{1 - \delta_i} \right] \omega_0 &> \delta_i^{D_s} \left[\beta_i^{\mathbf{1}(D_s > 0)} - \beta_i \delta_i^{T+1} \right] \omega_0 \\
(1 - \beta_i \delta_i) \left[\beta_i^{\mathbf{1}(D_s > 0)} + \beta_i \delta_i \frac{1 - \delta_i^T}{1 - \delta_i} \right] &> \beta_i^{\mathbf{1}(D_s > 0)} - \beta_i \delta_i^{T+1} \\
(1 - \beta_i \delta_i) \beta_i \delta_i \frac{1 - \delta_i^T}{1 - \delta_i} &> \beta_i \delta_i \beta_i^{\mathbf{1}(D_s > 0)} - \beta_i \delta_i^{T+1} \\
(1 - \beta_i \delta_i) \frac{1 - \delta_i^T}{1 - \delta_i} + \delta_i^T - \beta_i^{\mathbf{1}(D_s > 0)} &> 0
\end{aligned}$$

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:

$$\begin{aligned}
(1 - \beta_i \delta_i) \frac{1 - \delta_i^T}{1 - \delta_i} + \delta_i^T - \beta_i^{\mathbf{1}(D_s > 0)} &> 0 \tag{18} \\
221.77 &> 0 \quad \text{for } D_s = 0 \\
221.87 &> 0 \quad \text{for } D_s > 0
\end{aligned}$$

The evidence for the case of full projection bias lends even stronger support to the inequality in equation 14 being true. We therefore conclude that $P[\tilde{\gamma}_{i0s} > \tilde{\tilde{\gamma}}_{i0s}] \approx 1$ is a reasonable assumption in our context.

C Figures

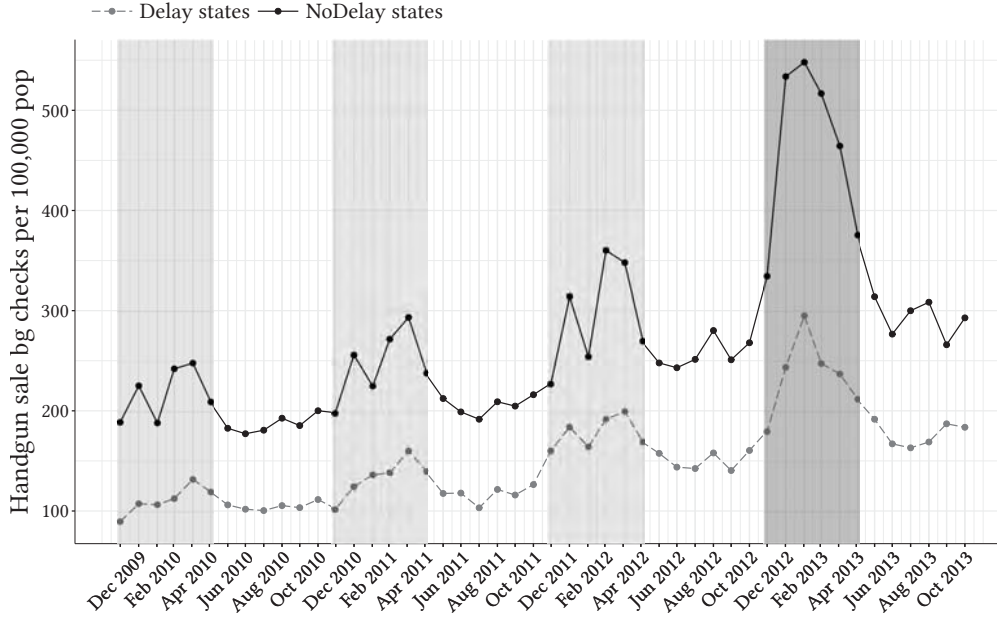


FIGURE 8: BACKGROUND CHECKS FOR HANDGUNS IN *Delay* vs *NoDelay* STATES (LEVELS)

Monthly NICS handgun background checks per 100,000 inhabitants in *Delay* states and *NoDelay* states between November 2009 and October 2013. The sample encompasses data for all states consistently included in our main specification. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

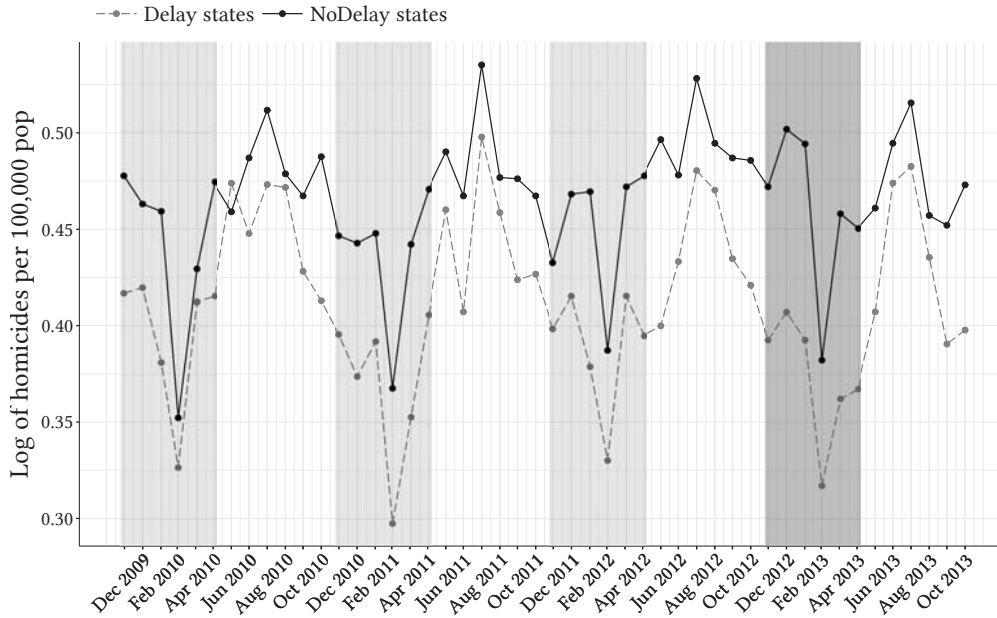


FIGURE 9: HOMICIDE RATE IN *Delay* vs *NoDelay* STATES (LEVELS)

Monthly homicides per 100,000 inhabitants in *Delay* states and *NoDelay* states between November 2009 and October 2013. The sample encompasses data from all counties consistently included in our main specification. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

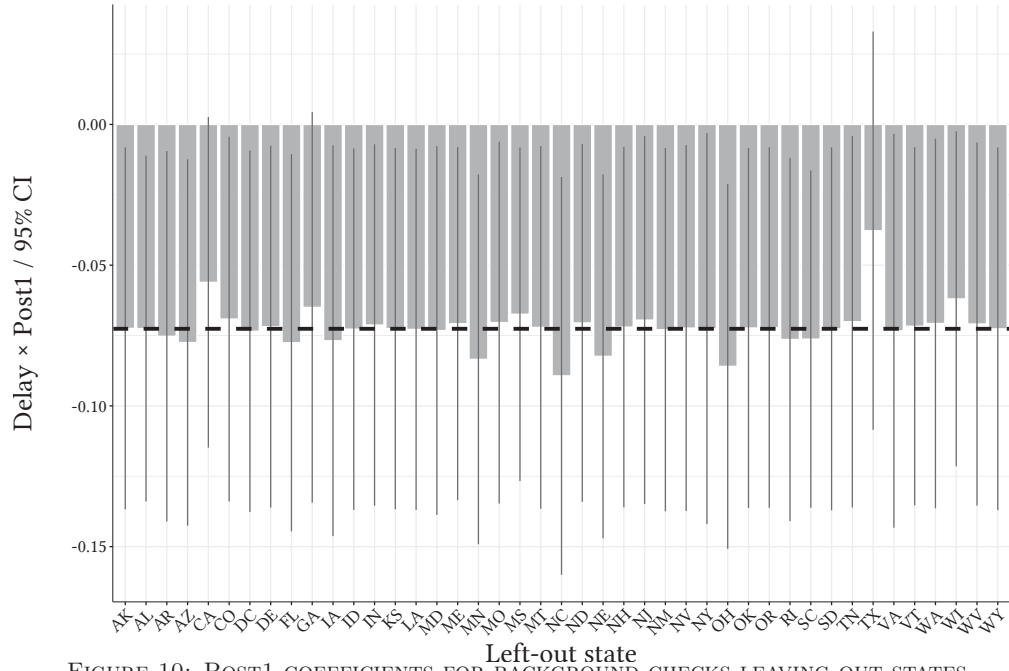


FIGURE 10: POST1 COEFFICIENTS FOR BACKGROUND CHECKS LEAVING OUT STATES

Coefficients for Δ_{12} Log handgun sale background checks after 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 state.

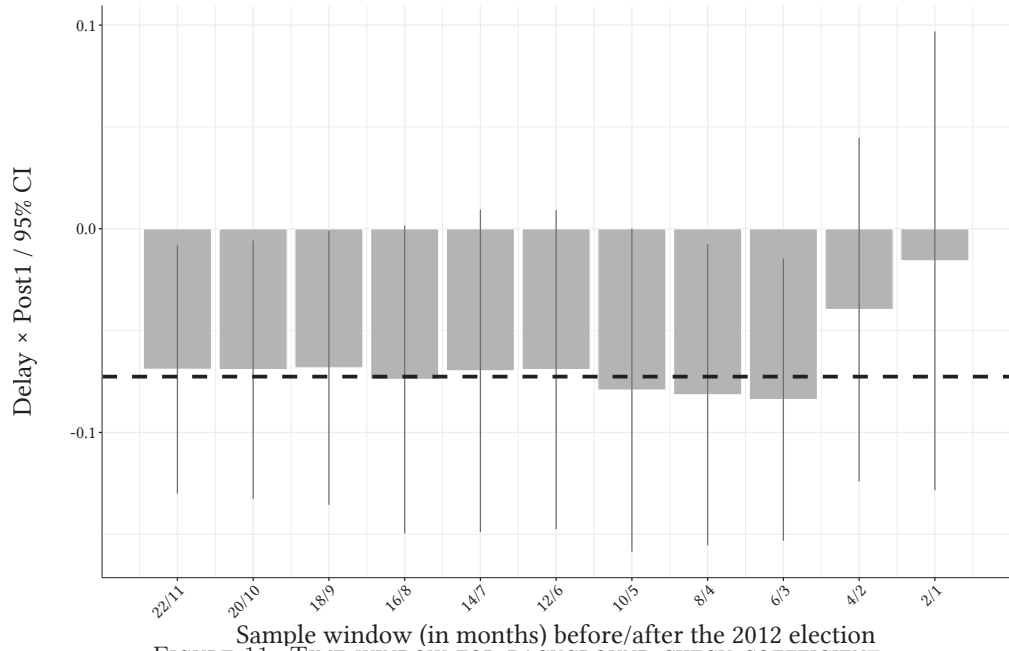


FIGURE 11: TIME WINDOW FOR BACKGROUND CHECK COEFFICIENT

Coefficients for Δ_{12} Log handgun sale background checks per 100,000 inhabitants including a decreasing number of months before and after the 2012 election 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.

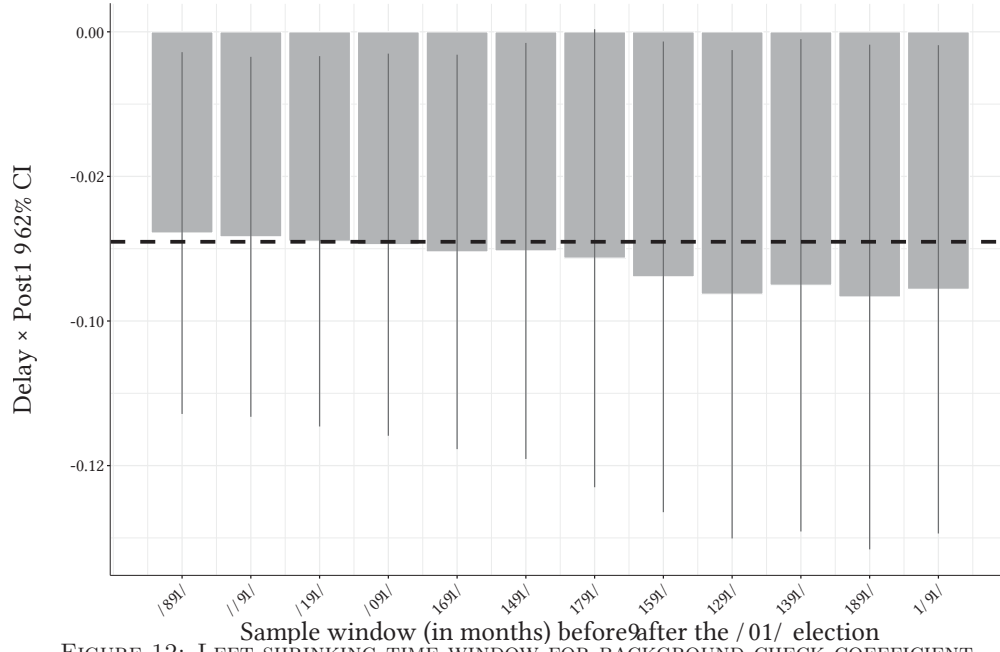


FIGURE 12: LEFT-SHRINKING TIME WINDOW FOR BACKGROUND CHECK COEFFICIENT

Coefficients for Δ_{12} Log handgun sale background checks per 100,000 inhabitants including a decreasing number of months before the 2012 election in the regression and corresponding 95% confidence intervals. 23/12 means that 23 months prior to and 12 months after the demand shock are included (=35 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.

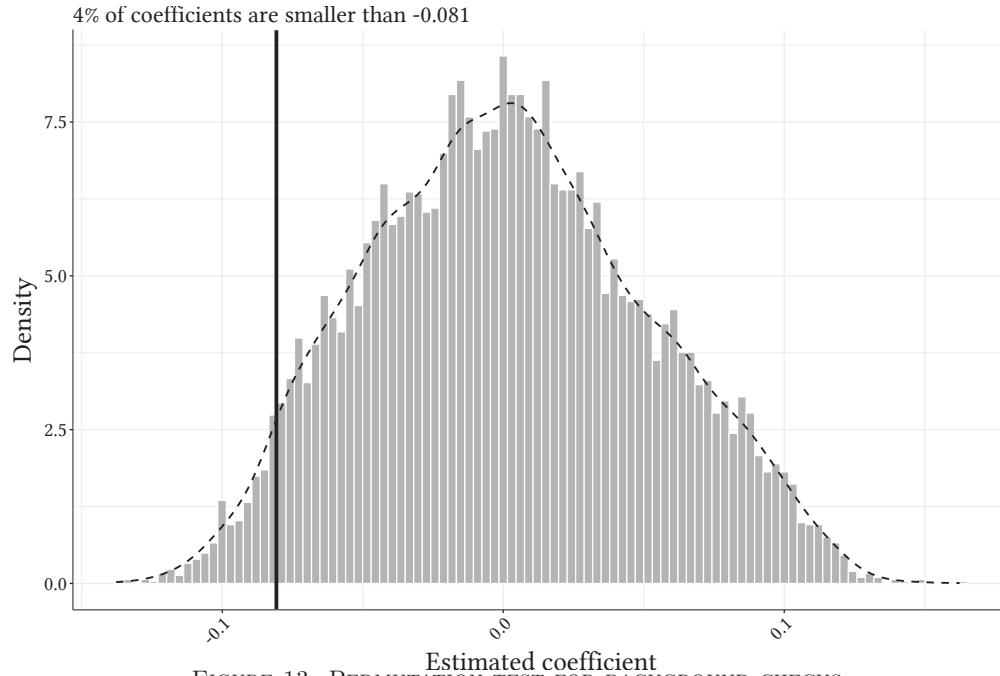


FIGURE 13: PERMUTATION TEST FOR BACKGROUND CHECKS

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.

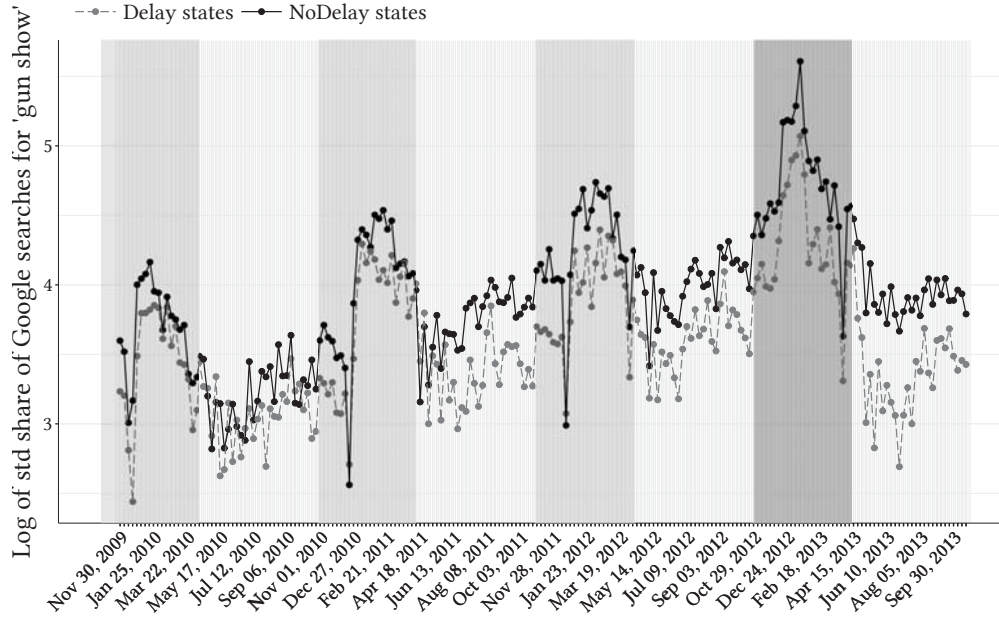


FIGURE 14: LOG GOOGLE SEARCHES FOR “GUN SHOW” IN *Delay* VS *NoDelay* STATES

Log weekly averages of daily normalized Google searches for the expression “gun show” in *Delay* states and *NoDelay* states between November 2009 and October 2013. The sample encompasses data for all states consistently included in our main specification. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

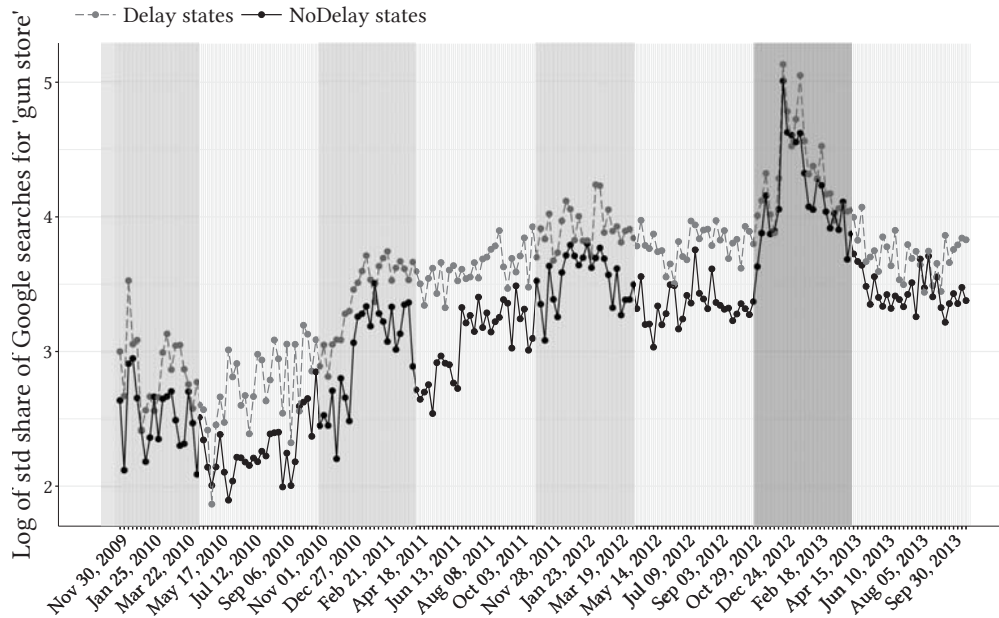


FIGURE 15: LOG GOOGLE SEARCHES FOR “GUN STORE” IN *Delay* VS *NoDelay* STATES

Log weekly averages of daily normalized Google searches for the expression “gun store” in *Delay* states and *NoDelay* states between November 2009 and October 2013. The sample encompasses data for all states consistently included in our main specification. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

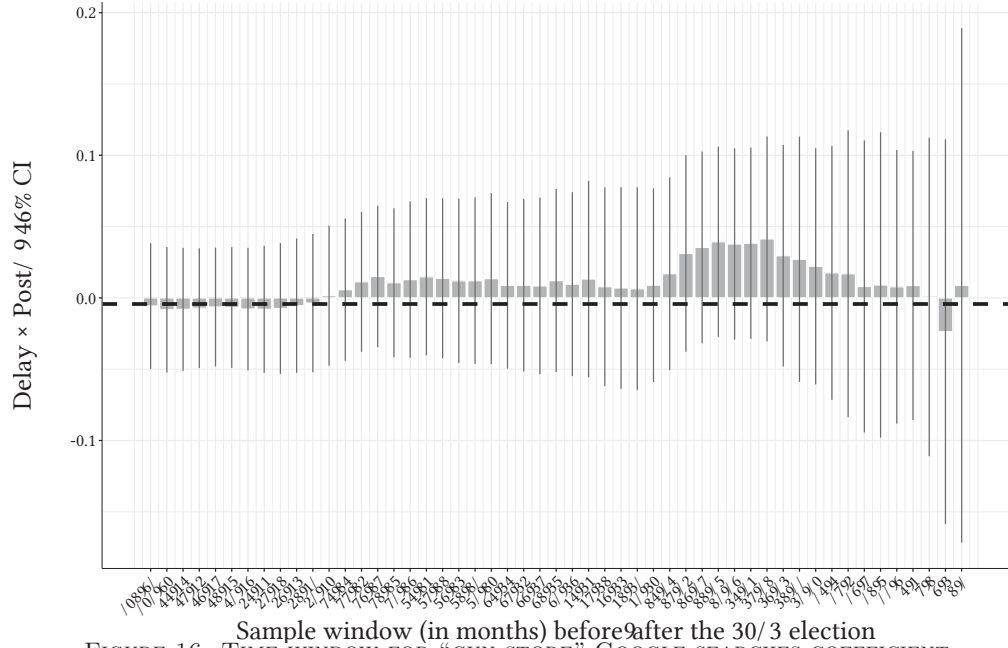


FIGURE 16: TIME WINDOW FOR “GUN STORE” GOOGLE SEARCHES COEFFICIENT

Coefficients for Δ_{52} Log Google searches for “gun store” including a decreasing number of weeks before and after the 2012 election in the regression and corresponding 95% confidence intervals. 103/51 means that 103 weeks prior to and 51 weeks after the demand shock are included (=154 weeks in total), etc. The dashed line indicates our baseline, i.e. the magnitude when including 104 weeks prior and 52 weeks after the demand shock.

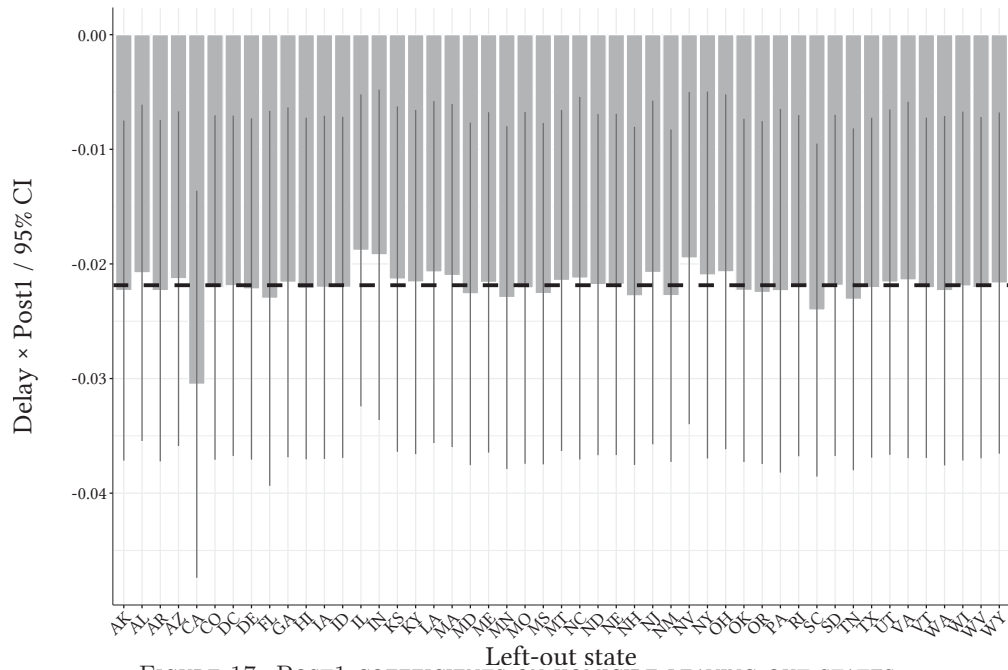


FIGURE 17: POST1 COEFFICIENTS ON HOMICIDE LEAVING OUT STATES

Coefficients for Δ_{12} Log handgun homicides per 100,000 inhabitants after 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 state.

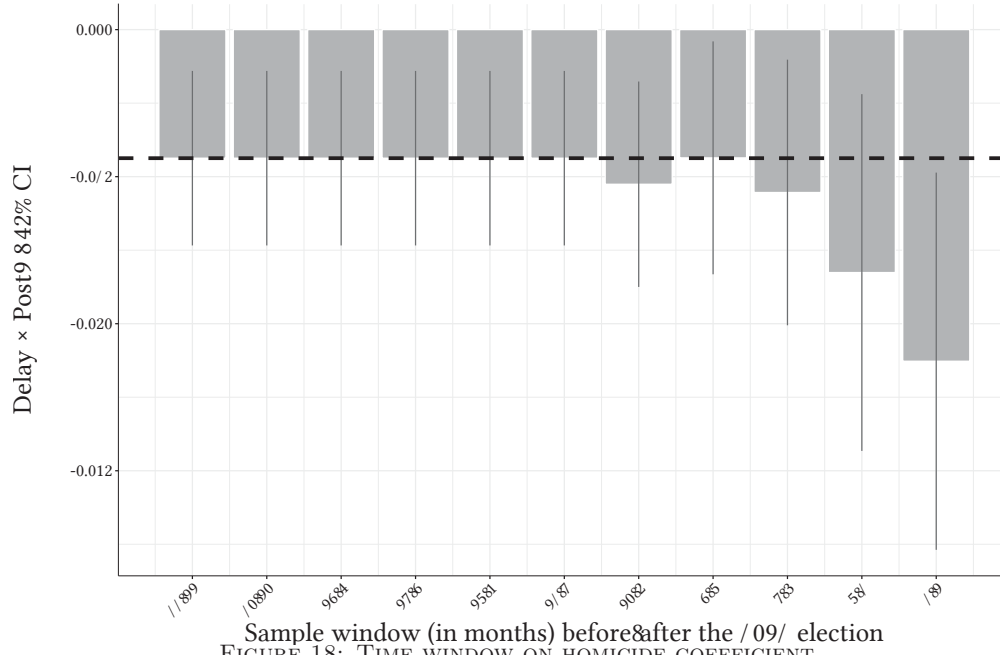


FIGURE 18: TIME WINDOW ON HOMICIDE COEFFICIENT

Coefficients for Δ_{12} Log handgun homicides per 100,000 inhabitants including a decreasing number of months before and after the 2012 election 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.

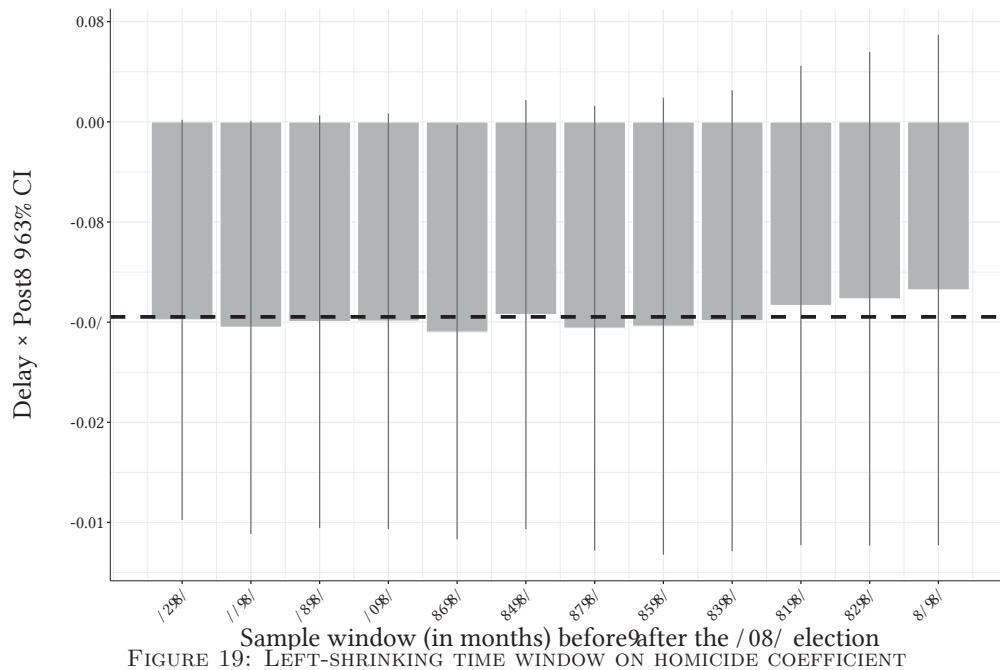


FIGURE 19: LEFT-SHRINKING TIME WINDOW ON HOMICIDE COEFFICIENT

Coefficients for Δ_{12} Log handgun homicides per 100,000 inhabitants including a decreasing number of months before the 2012 election in the regression and corresponding 95% confidence intervals. 23/12 means that 23 months prior to and 12 months after the demand shock are included (=35 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.

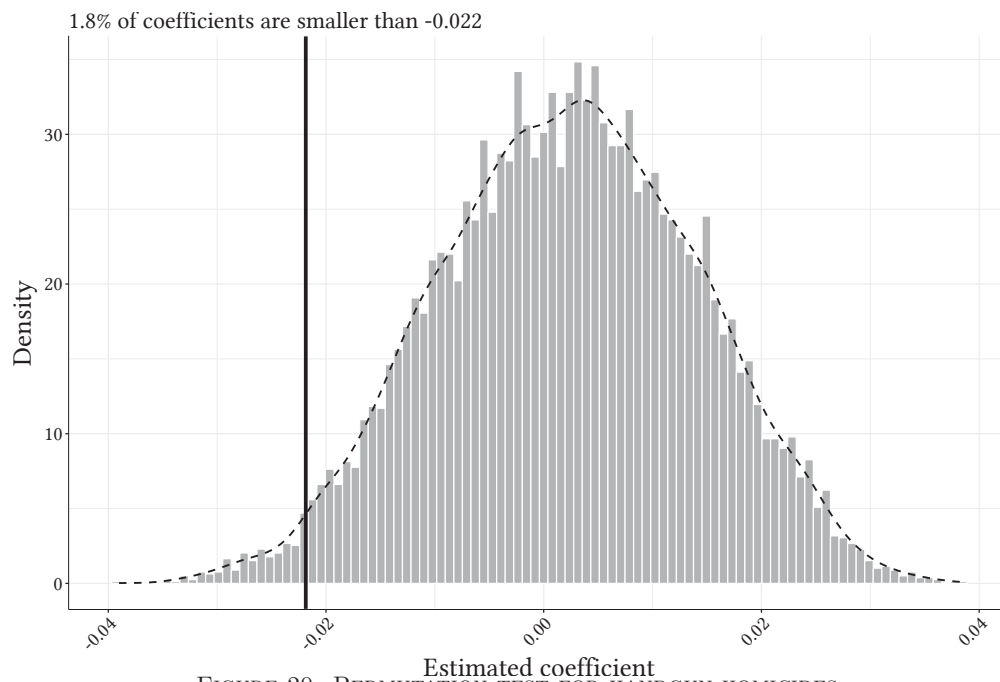


FIGURE 20: PERMUTATION TEST FOR HANDGUN HOMICIDES

Density of coefficients of 10,000 regressions as in Table 6, column 3, 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.

D Tables

TABLE 14: HANDGUN BACKGROUND CHECKS (VARYING THE SAMPLE)

	Δ_{12} Log of handgun sale background checks per 100,000 inhabitants									
	Cleaned NICS data - CT&MI (=BL1)		BL1 + CT & MI		BL1 + MI w/ law change		BL1 - states w/ NICS outliers		Full NICS data - CT&MI (=BL2)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	-0.081* (0.044)	-0.073** (0.033)	-0.062 (0.042)	-0.040 (0.042)	-0.084* (0.043)	-0.081** (0.033)	-0.079 (0.048)	-0.066** (0.034)	-0.779 (0.542)	-0.790 (0.521)
Delay×Post2	0.010 (0.064)	0.007 (0.084)	0.031 (0.060)	0.043 (0.080)	-0.000 (0.065)	-0.010 (0.085)	0.046 (0.058)	0.058 (0.070)	-0.375 (0.310)	-0.386 (0.290)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y	N	Y
States	43	43	45	45	44	44	40	40	49	49
Observations	1,516	1,516	1,588	1,588	1,552	1,552	1,440	1,440	1,764	1,764
R ²	0.539	0.594	0.539	0.593	0.545	0.598	0.577	0.622	0.261	0.433

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 15: HANDGUN BACKGROUND CHECKS (LOGS AND LEVELS)

	Handgun background checks per 100,000 inhabitants							
	Seasonal differencing (Baseline)				Within transformation			
	Logs (Baseline)		Levels		Logs		Levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.081* (0.044)	-0.073** (0.033)	-66.211*** (23.895)	-55.313*** (19.723)	-0.054 (0.090)	-0.107** (0.045)	-72.428** (32.484)	-68.820*** (22.516)
Delay×Post2	0.010 (0.064)	0.007 (0.084)	-14.166 (18.522)	-4.637 (21.528)	0.073 (0.079)	0.005 (0.083)	-8.818 (18.992)	-3.972 (21.428)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	2,047	2,047	2,047	2,047
Mean DV	0.194	0.194	44.999	44.999	5.860	5.860	213.573	213.573
R ²	0.539	0.594	0.636	0.695	0.970	0.979	0.921	0.942

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 16: HANDGUN BACKGROUND CHECKS (DIFFERENT WEIGHTS)

Weights	Δ_{12} Log of handgun background checks per 100,000 inhabitants					
	Population		Adult population		None	
	(1)	(2)	(3)	(4)	(5)	(6)
Delay×Post1	-0.081*	-0.073**	-0.079*	-0.072**	-0.030	-0.042
	(0.044)	(0.033)	(0.044)	(0.033)	(0.042)	(0.042)
Delay×Post2	0.010	0.007	0.009	0.004	-0.029	-0.055
	(0.064)	(0.084)	(0.064)	(0.084)	(0.059)	(0.081)
Year-Month FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y
States	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	1,516	1,516
R ²	0.539	0.594	0.538	0.593	0.561	0.608

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Weights change according to the specification.

TABLE 17: HANDGUN BACKGROUND CHECKS (TREND SPECIFICATIONS)

Trend model	Δ_{12} Log of background checks per 100,000 inhabitants							
	Trends from full sample				Trends from pre-period			
	Handgun		Other		Handgun		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.073**	-0.121*	-0.030	-0.054	-0.061**	-0.054	-0.025	-0.016
	(0.033)	(0.069)	(0.023)	(0.040)	(0.030)	(0.057)	(0.022)	(0.033)
Delay×Post2	0.007	-0.047	0.048	0.022	0.007	-0.027	0.048	0.034
	(0.084)	(0.119)	(0.062)	(0.082)	(0.082)	(0.124)	(0.060)	(0.083)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	Y	Y	Y	Y	Y	Y	Y	Y
State Trends ²	N	Y	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	1,516	1,516	1,516	1,516
R ²	0.594	0.649	0.724	0.764	0.555	0.466	0.698	0.629

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 18: HANDGUN BACKGROUND CHECKS (WILD-CLUSTER BOOTSTRAP)

Standard errors	Δ_{12} Log of background checks per 100,000 inhabitants							
	State-level clustering (Baseline)				Wild-cluster bootstrap (k=10000)			
	Handgun		Other		Handgun		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.081*	-0.073**	-0.039	-0.030	-0.081**	-0.073*	-0.039*	-0.030
	(0.044)	(0.033)	(0.027)	(0.023)	(0.039)	(0.041)	(0.023)	(0.026)
Delay×Post2	0.010	0.007	0.043	0.048	0.010	0.007	0.043	0.048
	(0.064)	(0.084)	(0.055)	(0.062)	(0.045)	(0.056)	(0.042)	(0.040)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	1,516	1,516	1,516	1,516
R ²	0.539	0.594	0.689	0.724	0.539	0.594	0.689	0.724

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors are in parentheses and calculated as stated: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 19: HANDGUN BACKGROUND CHECKS (OTHER GUN LAWS)

	Δ_{12} Log of handgun sale background checks per 100,000 inhabitants							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.081*	-0.073**	-0.071*	-0.067**	-0.034	-0.053*	-0.082*	-0.073**
	(0.044)	(0.033)	(0.038)	(0.030)	(0.035)	(0.032)	(0.046)	(0.033)
Delay×Post2	0.010	0.007	0.000	-0.008	-0.026	-0.062	-0.004	-0.009
	(0.064)	(0.084)	(0.066)	(0.080)	(0.077)	(0.088)	(0.061)	(0.084)
No Stand-Your-Ground×Post1			-0.048*	-0.028				
			(0.025)	(0.023)				
No Stand-Your-Ground×Post2			0.051	0.072				
			(0.078)	(0.088)				
Mental Checks×Post1					-0.122***	-0.049		
					(0.040)	(0.034)		
Mental Checks×Post2					0.102	0.187**		
					(0.085)	(0.085)		
Domestic Abuser's Law×Post1							0.011	0.025
							(0.028)	(0.030)
Domestic Abuser's Law×Post2							0.145**	0.172**
							(0.058)	(0.071)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	1,516	1,516	1,516	1,516
R ²	0.539	0.594	0.543	0.598	0.550	0.609	0.556	0.613

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 20: HANDGUN BACKGROUND CHECKS (GUN PREFERENCES)

Δ_{12} Log of handgun sale background checks per 100,000 inhabitants								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.081*	-0.073**	-0.071*	-0.063**	-0.074*	-0.065*	-0.058	-0.058
	(0.044)	(0.033)	(0.042)	(0.032)	(0.044)	(0.033)	(0.048)	(0.037)
Delay×Post2	0.010	0.007	-0.003	-0.007	0.001	-0.006	-0.031	-0.050
	(0.064)	(0.084)	(0.066)	(0.086)	(0.069)	(0.091)	(0.069)	(0.092)
Gun Stores p.c.×Post1			170.566*	170.039*				
			(96.905)	(97.767)				
Gun Stores p.c.×Post2			-232.867	-240.442				
			(178.115)	(205.031)				
Gun Owners p.c.×Post1					0.131	0.134		
					(0.136)	(0.143)		
Gun Owners p.c.×Post2					-0.208	-0.264		
					(0.363)	(0.411)		
Gun Shows p.c.×Post1							0.191**	0.100
							(0.090)	(0.123)
Gun Shows p.c.×Post2							-0.412**	-0.566**
							(0.187)	(0.254)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	1,516	1,516	1,516	1,516
R ²	0.539	0.594	0.544	0.599	0.540	0.596	0.552	0.613

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 21: GOOGLE SEARCHES FOR “GUN SHOW” (LOGS AND LEVELS)

Standardized share of Google searches for “gun show”								
	Seasonal differencing (Baseline)				Within transformation			
	Logs (Baseline)		Levels		Logs		Levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.026	-0.094	-10.762***	-8.050**	-0.615***	0.023	-28.962***	-13.373***
	(0.086)	(0.128)	(3.226)	(3.473)	(0.218)	(0.088)	(10.425)	(3.835)
Delay×Post2	-0.222**	-0.290**	-3.539	-0.827	-0.873***	-0.235***	-18.674***	-3.085
	(0.097)	(0.142)	(4.076)	(5.491)	(0.231)	(0.079)	(5.321)	(3.021)
Year-Week FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	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
R ²	0.257	0.276	0.407	0.422	0.419	0.715	0.475	0.737

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks, % hispanics and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 22: GOOGLE SEARCHES FOR “GUN STORE” (LOGS AND LEVELS)

	Standardized share of Google searches for “gun store”							
	Seasonal differencing (Baseline)				Within transformation			
	Logs (Baseline)		Levels		Logs		Levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	0.037 (0.082)	-0.017 (0.090)	-1.046 (3.611)	-0.024 (2.605)	-0.283 (0.181)	0.035 (0.073)	-8.376 (7.763)	-2.459 (3.848)
Delay×Post2	-0.027 (0.097)	-0.081 (0.133)	0.667 (2.386)	1.689 (3.627)	-0.353** (0.173)	-0.035 (0.112)	-5.371 (4.509)	0.546 (2.267)
Year-Week FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	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
R ²	0.230	0.247	0.419	0.441	0.511	0.665	0.440	0.802

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks, % hispanics and % with internet access. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 23: HOMICIDE RATES (LOGS AND LEVELS)

	Handgun homicides per 100,000 inhabitants							
	Seasonal differencing (Baseline)				Within transformation			
	Logs (Baseline)		Levels		Logs		Levels	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.022*** (0.008)	-0.019** (0.010)	-0.021** (0.009)	-0.017 (0.012)	-0.034*** (0.009)	-0.033*** (0.011)	-0.037*** (0.010)	-0.034** (0.013)
Delay×Post2	-0.018 (0.015)	-0.015 (0.018)	-0.019 (0.018)	-0.015 (0.021)	-0.012 (0.013)	-0.011 (0.019)	-0.012 (0.015)	-0.008 (0.022)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,047	3,047	3,047	3,047	3,047	3,047
Observations	109,692	109,692	109,692	109,692	146,256	146,256	146,256	146,256
Mean DV	0.000	0.000	0.001	0.001	0.245	0.245	0.287	0.287
R ²	0.008	0.019	0.005	0.015	0.478	0.491	0.334	0.350

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 24: HOMICIDE RATES (DIFFERENT WEIGHTS)

Weights	Δ_{12} Log of handgun homicides per 100,000 inhabitants					
	Population		Adult population		None	
	(1)	(2)	(3)	(4)	(5)	(6)
Delay×Post1	-0.022*** (0.008)	-0.019** (0.010)	-0.022*** (0.007)	-0.019* (0.010)	0.003 (0.007)	0.005 (0.009)
Delay×Post2	-0.018 (0.015)	-0.015 (0.018)	-0.017 (0.015)	-0.014 (0.018)	-0.014* (0.008)	-0.011 (0.010)
Year-Month FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,047	3,047	3,047	3,047
Observations	109,692	109,692	109,692	109,692	109,692	109,692
R ²	0.008	0.019	0.008	0.019	0.002	0.012

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Weights change according to the specification.

TABLE 25: HANDGUN HOMICIDE RATES (VARYING THE SAMPLE)

	Δ_{12} Log of handgun homicides per 100,000 inhabitants									
	Drop CT & MI (=BL2)		BL2 + CT&MI		BL2 + MI w/ law change		BL2 - states w/ NICS outliers		Cleaned NICS data - CT&MI (=BL1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	-0.022*** (0.008)	-0.019** (0.010)	-0.018** (0.008)	-0.018* (0.010)	-0.021*** (0.007)	-0.017* (0.010)	-0.023*** (0.008)	-0.020* (0.010)	-0.022*** (0.008)	-0.018* (0.010)
Delay×Post2	-0.018 (0.015)	-0.015 (0.018)	-0.021 (0.014)	-0.021 (0.017)	-0.016 (0.015)	-0.012 (0.018)	-0.019 (0.016)	-0.016 (0.019)	-0.018 (0.015)	-0.015 (0.018)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,138	3,138	3,130	3,130	2,823	2,823	3,047	3,047
Observations	109,692	109,692	112,968	112,968	112,680	112,680	101,628	101,628	106,414	106,414
R ²	0.008	0.019	0.008	0.019	0.008	0.019	0.008	0.019	0.008	0.019

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 26: HOMICIDE RATES (TREND SPECIFICATIONS)

Trend model	Δ_{12} Log of homicides per 100,000 inhabitants							
	Trends from full sample				Trends from pre-period			
	Handgun		Any		Handgun		Any	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.019** (0.010)	-0.017 (0.017)	-0.021 (0.016)	-0.022 (0.022)	-0.019** (0.010)	-0.018 (0.016)	-0.021 (0.016)	-0.024 (0.022)
Delay×Post2	-0.015 (0.018)	-0.013 (0.023)	0.005 (0.023)	0.004 (0.029)	-0.015 (0.018)	-0.014 (0.022)	0.005 (0.022)	0.002 (0.029)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	Y	Y	Y	Y	Y	Y	Y	Y
County Trends ²	N	Y	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,047	3,047	3,047	3,047	3,047	3,047
Observations	109,692	109,692	109,692	109,692	109,692	109,692	109,692	109,692
R ²	0.019	0.046	0.016	0.043	0.008	0.008	0.006	0.006

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 27: HOMICIDE RATES (WILD-CLUSTER BOOTSTRAP)

Standard errors	Δ_{12} Log of homicides per 100,000 inhabitants							
	State-level clustering (Baseline)				Wild-cluster bootstrap (k=10000)			
	Handgun		Any		Handgun		Any	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.022*** (0.008)	-0.019** (0.010)	-0.024** (0.012)	-0.021 (0.016)	-0.022*** (0.008)	-0.019** (0.009)	-0.024** (0.010)	-0.021* (0.012)
Delay×Post2	-0.018 (0.015)	-0.015 (0.018)	0.002 (0.017)	0.005 (0.023)	-0.018** (0.007)	-0.015* (0.008)	0.002 (0.012)	0.005 (0.014)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,047	3,047	3,047	3,047	3,047	3,047
Observations	109,692	109,692	109,692	109,692	109,692	109,692	109,692	109,692
R ²	0.008	0.019	0.006	0.016	0.008	0.019	0.006	0.016

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors are in parentheses and calculated as stated: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 28: HOMICIDE RATES (REMOVE BORDER COUNTIES)

	Δ_{12} Log of homicides per 100,000 inhabitants							
	Baseline				Excluding border counties			
	Handgun		Any		Handgun		Any	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post1	-0.022*** (0.008)	-0.019** (0.010)	-0.024** (0.012)	-0.021 (0.016)	-0.017** (0.007)	-0.014 (0.009)	-0.019 (0.013)	-0.016 (0.017)
Delay×Post2	-0.018 (0.015)	-0.015 (0.018)	0.002 (0.017)	0.005 (0.023)	-0.017 (0.015)	-0.014 (0.019)	0.004 (0.019)	0.007 (0.025)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,047	3,047	2,845	2,845	2,845	2,845
Observations	109,692	109,692	109,692	109,692	102,420	102,420	102,420	102,420
R ²	0.008	0.019	0.006	0.016	0.007	0.018	0.005	0.015

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 29: HOMICIDE RATES (STATE LEVEL)

	Δ_{12} Log of homicides per 100,000 inhabitants							
	Handgun				Any		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delay×Post	-0.019** (0.009)							
Delay×Post1		-0.023** (0.010)	-0.026** (0.010)	-0.028** (0.013)	-0.026* (0.013)	-0.027 (0.018)	-0.003 (0.011)	-0.002 (0.014)
Delay×Post2		-0.014 (0.014)	-0.015 (0.015)	-0.017 (0.019)	0.004 (0.020)	0.003 (0.026)	0.020* (0.011)	0.021 (0.014)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y	Y	Y
State Trends	N	N	N	Y	N	Y	N	Y
States	49	49	49	49	49	49	49	49
Observations	1,764	1,764	1,764	1,764	1,764	1,764	1,764	1,764
R ²	0.036	0.036	0.150	0.162	0.134	0.145	0.135	0.143

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

TABLE 30: GOOGLE SEARCHES FOR “GUN STORE” (EXCLUDING INDIVIDUAL CONTROLS)

	Δ_{52} Log of standardized share of Google searches for “gun store”															
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Delay×Post1	-0.205** (0.084)	-0.192** (0.080)	0.037 (0.082)	-0.017 (0.090)	-0.013 (0.091)	-0.074 (0.099)	0.032 (0.081)	-0.021 (0.088)	0.038 (0.082)	-0.013 (0.096)	0.055 (0.082)	0.012 (0.096)	0.012 (0.086)	-0.038 (0.089)	0.038 (0.076)	-0.018 (0.085)
Delay×Post2	-0.181** (0.092)	-0.168 (0.110)	-0.027 (0.097)	-0.081 (0.133)	-0.036 (0.092)	-0.097 (0.127)	-0.033 (0.093)	-0.086 (0.126)	-0.028 (0.098)	-0.079 (0.130)	-0.020 (0.095)	-0.063 (0.132)	-0.017 (0.095)	-0.067 (0.134)	-0.047 (0.094)	-0.102 (0.133)
Year-Week FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Log(pop)	N	N	Y	Y	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
% Rural	N	N	Y	Y	Y	Y	Y	N	Y	Y	Y	Y	Y	Y	Y	Y
% Hispanic	N	N	Y	Y	Y	Y	Y	Y	N	N	Y	Y	Y	Y	Y	Y
% Black	N	N	Y	Y	Y	Y	Y	Y	Y	Y	N	N	Y	Y	Y	Y
% Below Poverty Line	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	N	N	Y	Y
% HHs w Internet	N	N	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	N	N
States	49	49	49	49	49	49	49	49	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	7,693	7,693	7,693	7,693	7,693	7,693	7,693	7,693
R ²	0.112	0.136	0.230	0.247	0.214	0.232	0.215	0.232	0.211	0.229	0.210	0.228	0.214	0.231	0.211	0.228

Notes: Observations are at the state-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: * p<0.1; ** p<0.05; *** p<0.01. Included control variables are stated. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

	Δ_{12} Log of murders per 100,000 inhabitants																	
	UCR			NVSS (UCR sample)			NVSS (full sample)											
	Handgun	Any	Other	Handgun	Any	Other	Handgun	Any	Other									
Delay×Post1	(1) -0.014 (0.011)	(2) -0.013 (0.013)	(3) -0.017 (0.013)	(4) -0.016 (0.018)	(5) -0.001 (0.010)	(6) 0.000 (0.012)	(7) -0.019** (0.009)	(8) -0.017 (0.012)	(9) -0.023* (0.013)	(10) -0.022 (0.019)	(11) -0.003 (0.012)	(12) -0.003 (0.016)	(13) -0.022*** (0.008)	(14) -0.019** (0.010)	(15) -0.024** (0.012)	(16) -0.021 (0.016)	(17) -0.002 (0.010)	(18) -0.001 (0.013)
Delay×Post2	(1) 0.003 (0.019)	(2) 0.004 (0.025)	(3) 0.015 (0.022)	(4) 0.016 (0.031)	(5) 0.018 (0.011)	(6) 0.019 (0.014)	(7) -0.016 (0.017)	(8) -0.014 (0.020)	(9) 0.003 (0.021)	(10) 0.004 (0.028)	(11) 0.022* (0.012)	(12) 0.021 (0.015)	(13) -0.018 (0.015)	(14) -0.015 (0.018)	(15) 0.002 (0.017)	(16) 0.005 (0.023)	(17) 0.022*** (0.008)	(18) 0.024*** (0.011)
Year-Month FE	Y	Y	Y	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	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Counties	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229	2,229	3,047	3,047	3,047	3,047	3,047	3,047
Observations	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244	80,244	109,692	109,692	109,692	109,692	109,692	109,692
R ²	0.010	0.021	0.009	0.019	0.008	0.019	0.009	0.020	0.007	0.017	0.006	0.015	0.008	0.019	0.006	0.016	0.005	0.014

77

E Additional Analyses

E.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.

In the following, we present a regression using the monthly data on actual gunshows per 100,000 inhabitants as dependent variable in Table 32, to investigate if a similar pattern holds for the supply of gun stores. Regardless of the specification used, the results seem to provide strong evidence for an actual increase in gunshows for *NoDelay* states during *Post1* period by 3%. However, a quick look at the timing of this effect in Figure 22 shows that the increase in gun shows in *NoDelay* states started in July 2012, i.e. four months before the onset of our treatment. This effect becomes slightly more pronounced after November 2012 but is never significantly different from the initial shift in July 2012. We conclude that the supply of gun shows in *Delay* states very likely did not significantly increase over *NoDelay* states as a result of the demand shock.

E.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

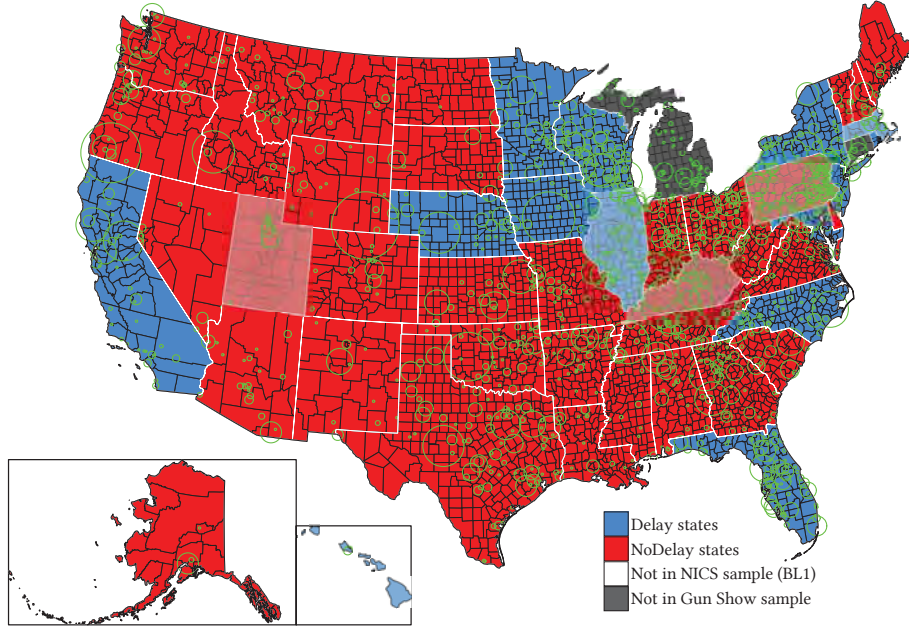


FIGURE 21: LOCATIONS OF GUN SHOWS

Map of the United States showing the distribution of gun shows between November 2009 and October 2013. 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. Red counties are located in *NoDelay* states. Blue counties are located in *Delay* states. Shaded states are entirely dropped in the NICS sample. Dark grey counties are not present in the Gunshow sample.

TABLE 32: GUN SHOWS

	Δ_{12} Log of gun shows per 100,000 inhabitants			
	(1)	(2)	(3)	(4)
Delay×Post	-0.029*** (0.008)			
Delay×Post1		-0.040*** (0.010)	-0.034*** (0.008)	-0.030*** (0.008)
Delay×Post2		-0.019** (0.007)	-0.014* (0.008)	-0.010 (0.008)
Year-Month FE	Y	Y	Y	Y
Controls	N	N	Y	Y
County Trends	N	N	N	Y
Counties	3,047	3,047	3,047	3,047
Observations	109,692	109,692	109,692	109,692
R ²	0.033	0.033	0.041	0.083

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

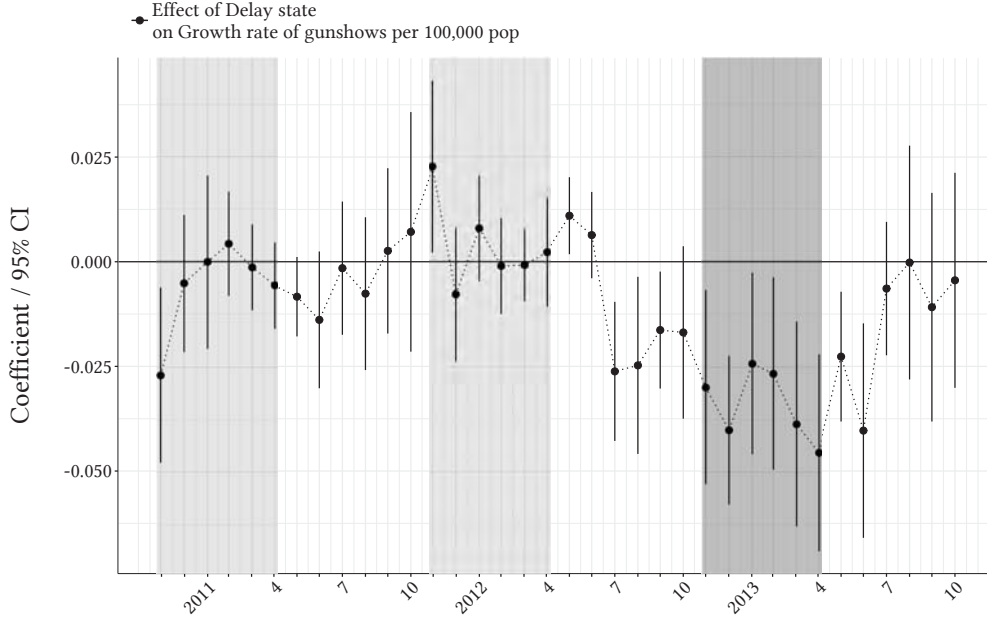


FIGURE 22: EVENT STUDY GRAPH FOR GUNSHOWS

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log of monthly gun shows per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

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).⁴⁷

Results for violent crime are reported in Table 33. 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 12. Column 1 shows that violent crime rates marginally decreased by about 0.5% on average during the demand shock in *Delay* states. The largest and only statistically significant coefficients are the ones on rape. All coefficients apart from the ones on robbery and manslaughter are negative.

The UCR data allow us to further split incidents of murder, robbery and aggravated assault by the main type of weapon used. Table 34 shows the results for each of these three crime categories when dividing them into whether a firearm was used. Also here, we do not see any significant response in *Delay* and *NoDelay* states during our treatment

⁴⁷See also Appendix H for the data cleaning procedure we applied.

TABLE 33: VIOLENT CRIMES

Δ_{12} Log of incidents per 100,000 inhabitants												
	Any Violent		Murder		Mansl'ter		Rape		Robbery		Agg. Assault	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Delay×Post1	-0.005 (0.019)	-0.007 (0.021)	-0.019 (0.012)	-0.021 (0.015)	0.002 (0.003)	0.001 (0.004)	-0.056 (0.041)	-0.069* (0.036)	0.028 (0.020)	0.016 (0.026)	-0.004 (0.023)	0.000 (0.023)
Delay×Post2	-0.021 (0.027)	-0.022 (0.033)	0.009 (0.019)	0.007 (0.025)	-0.004 (0.002)	-0.004 (0.003)	-0.072 (0.055)	-0.086* (0.051)	-0.029 (0.031)	-0.041 (0.037)	-0.001 (0.029)	0.004 (0.033)
Year-Month FE	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
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
R ²	0.006	0.034	0.008	0.019	0.004	0.013	0.010	0.033	0.008	0.028	0.006	0.035

Notes: All regressions use 80,244 observations from 2,229 counties. Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

TABLE 34: CRIMES BY TYPE OF WEAPON

Δ_{12} Log of incidents per 100,000 inhabitants																											
	Murder						Robbery						Aggr. Assault														
	All			Handgun			Other			All			Gun			Other			All			Gun			Other		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)									
Delay×Post1	-0.017 (0.013)	-0.016 (0.018)	-0.014 (0.011)	-0.013 (0.013)	-0.001 (0.010)	0.000 (0.012)	0.028 (0.020)	0.016 (0.026)	0.044 (0.032)	0.014 (0.045)	0.017 (0.019)	0.008 (0.019)	-0.004 (0.023)	0.000 (0.023)	0.011 (0.022)	-0.012 (0.033)	-0.005 (0.023)	-0.001 (0.024)									
Delay×Post2	0.015 (0.022)	0.016 (0.031)	0.003 (0.019)	0.004 (0.025)	0.018 (0.011)	0.019 (0.014)	-0.029 (0.031)	-0.041 (0.037)	-0.004 (0.037)	-0.034 (0.049)	-0.040 (0.031)	-0.049 (0.036)	-0.001 (0.029)	0.004 (0.033)	-0.007 (0.034)	-0.030 (0.043)	0.001 (0.028)	0.005 (0.034)									
Year-Month FE	Y	Y	Y	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	Y	Y	Y									
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y									
R ²	0.009	0.019	0.010	0.021	0.008	0.019	0.008	0.028	0.011	0.043	0.008	0.028	0.006	0.035	0.006	0.031	0.006	0.036									

Notes: All regressions use 80,244 observations from 2,229 counties. Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

period. Murder sees an insignificant relative decrease in firearm-related offenses while the effects of both robbery and aggravated assault is more mixed in terms of magnitude and sign. Corresponding event-studies in Figures 23 to 28 echo this finding. Importantly, none of the violent crime categories experiences a significant relative *increase*, which provides evidence against the existence of a strong deterrence effect.

Table 35 reports our findings for the categories of non-violent crime. As shown in column 1, the non-violent crime rate saw a relative decrease in *Delay* states during the demand shock and its aftermath. These results are also highly significant for both periods in the specification using county trends. The estimate for simple assault is positive but insignificant. Burglary, larceny and vehicle theft yield significant negative coefficients for at least one of the two (*Post1* and *Post2*) periods. Looking at the event-study graphs for each of these in Figures 29, 30 and 31 shows that none of these effects coincides with the start of the *Post1* period, and indicates a pre-trend for larceny. We therefore do not necessarily interpret the results in Table 35 as outcomes of relatively decreased gun ownership. One can, however, conclude that none of the non-violent crime categories points in the direction of a strong deterrence effect from higher firearm ownership in *NoDelay* states.

E.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 affected suicides and accidents involving a handgun. In Table 36 we use our baseline specification to create corresponding estimates for suicides and accidents. For accidents we find an insignificant and small relative *increase* in overall incidents in columns 1 and 2 while those related to a handgun in specifications 3 and 4 show no response. This does not fully support 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 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

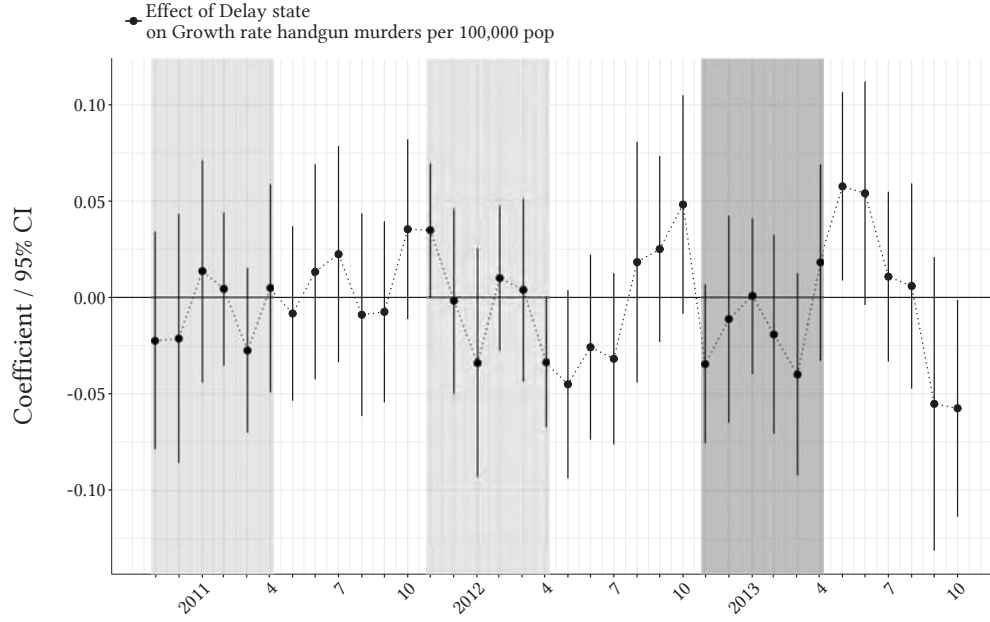


FIGURE 23: EVENT STUDY GRAPH FOR HANDGUN MURDER RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log handgun murders per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

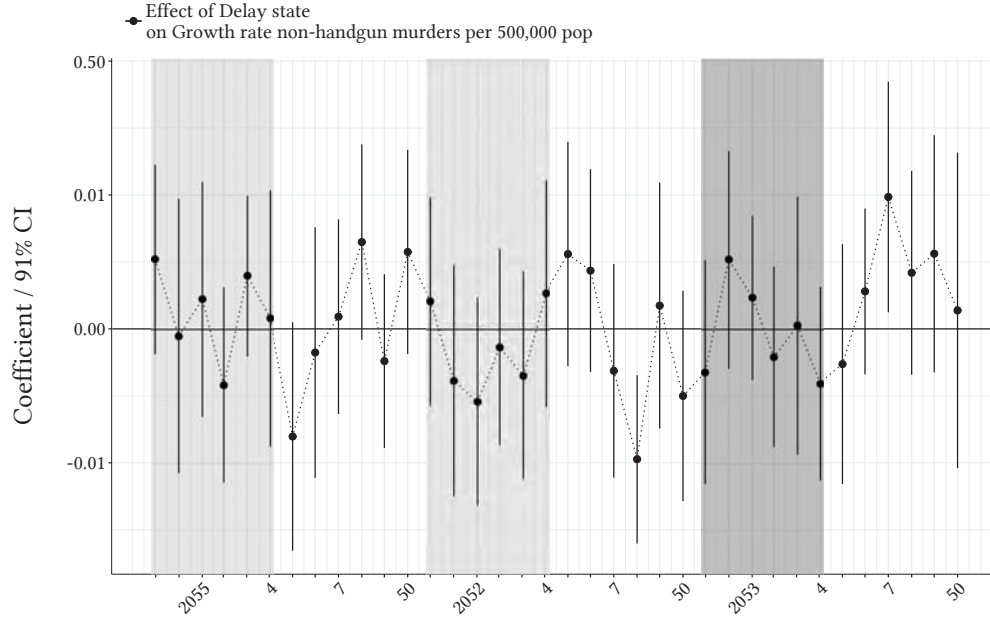


FIGURE 24: EVENT STUDY GRAPH FOR NON-HANDGUN MURDER RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log non-handgun murders per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

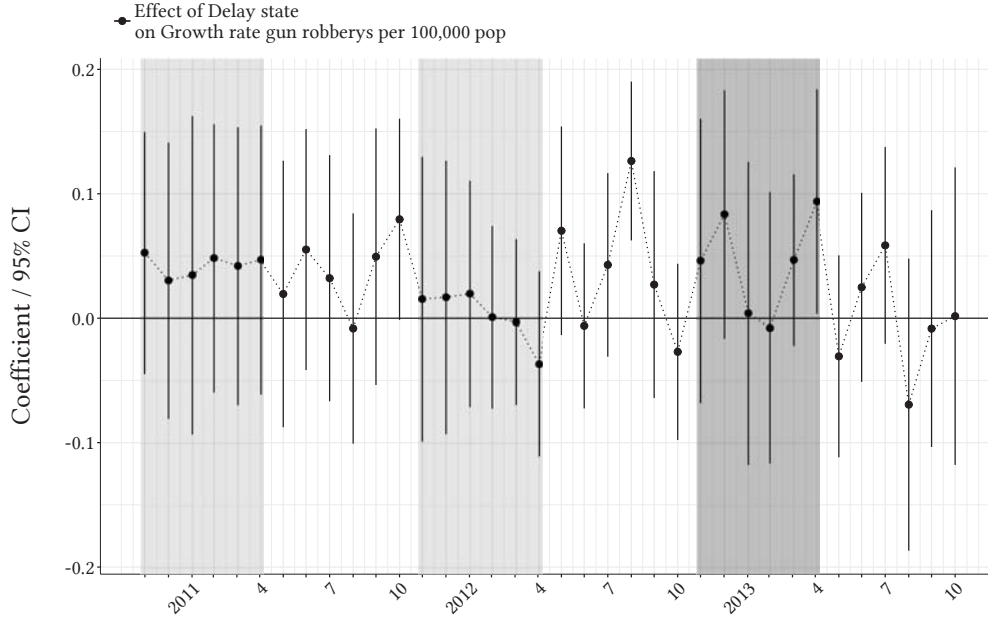


FIGURE 25: EVENT STUDY GRAPH FOR FIREARM ROBBERY RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log firearm robberies per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

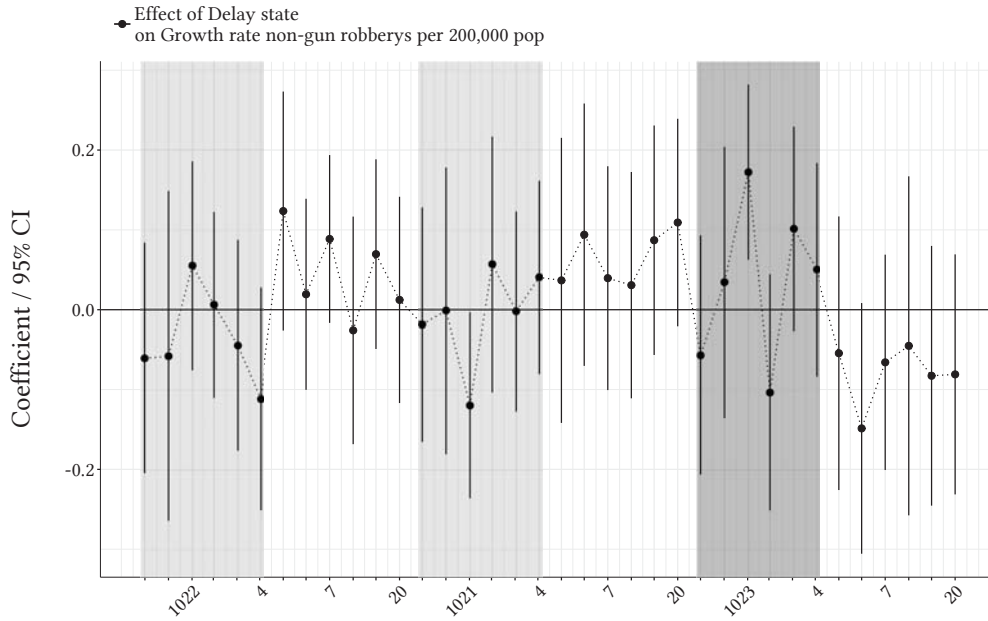


FIGURE 26: EVENT STUDY GRAPH FOR NON-FIREARM ROBBERY RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log non-firearm robberies per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

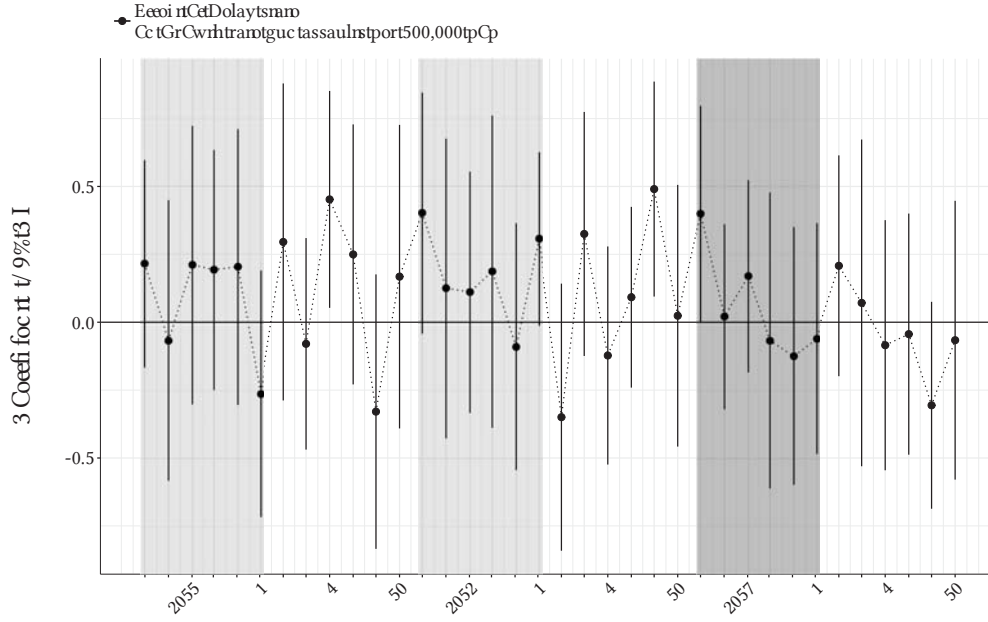


FIGURE 27: EVENT STUDY GRAPH FOR FIREARM ASSAULT RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log firearm assaults per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

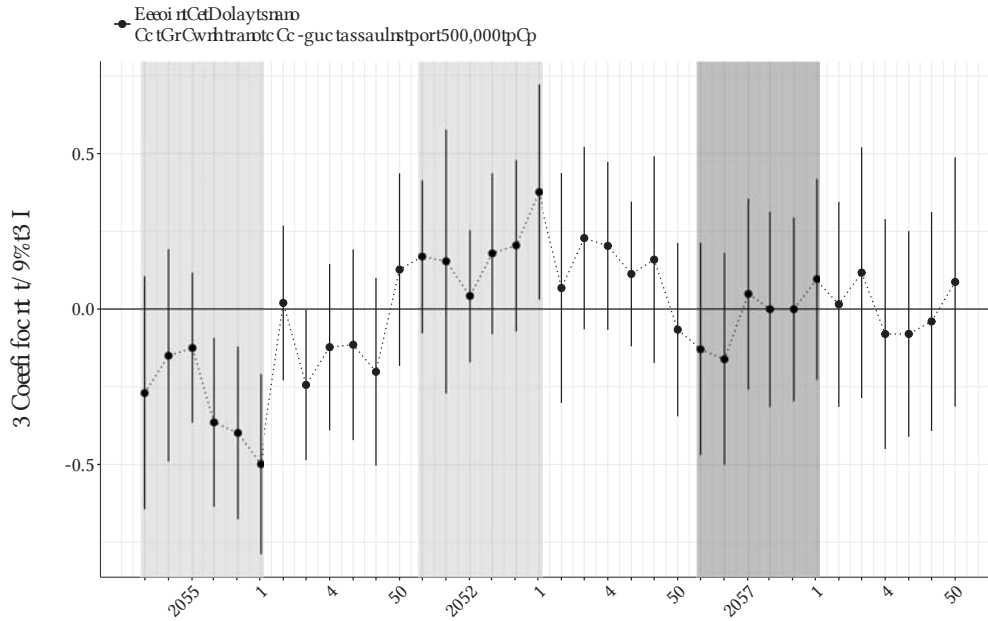


FIGURE 28: EVENT STUDY GRAPH FOR NON-FIREARM ASSAULT RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log non-firearm assaults per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

TABLE 35: NON-VIOLENT CRIMES

	Δ_{12} Log of incidents per 100,000 inhabitants									
	Any Non-Violent		Simple Assault		Burglary		Larceny		Veh.Theft	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	-0.020 (0.016)	-0.032** (0.013)	0.020 (0.014)	0.014 (0.017)	-0.001 (0.025)	-0.023 (0.021)	-0.045*** (0.016)	-0.058*** (0.015)	-0.011 (0.037)	-0.025 (0.030)
Delay×Post2	-0.025** (0.011)	-0.037*** (0.013)	0.007 (0.015)	0.000 (0.022)	-0.024 (0.022)	-0.046* (0.024)	-0.035** (0.014)	-0.048*** (0.018)	-0.050 (0.032)	-0.063** (0.031)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
R ²	0.037	0.091	0.009	0.060	0.024	0.058	0.027	0.073	0.008	0.034

Notes: Observations are at the county-level. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

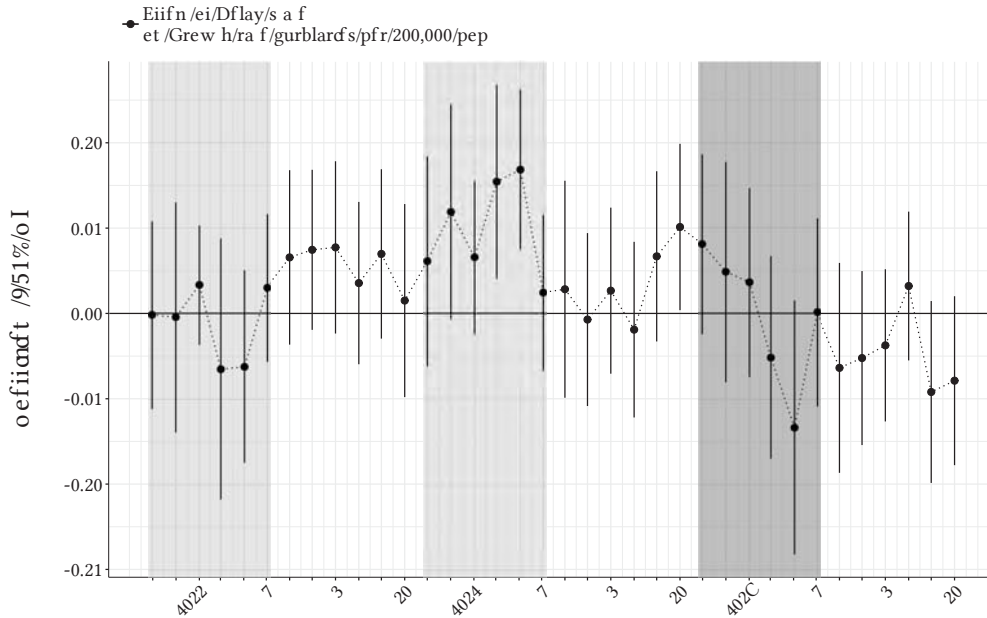


FIGURE 29: EVENT STUDY GRAPH FOR BURGLARY RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log burglaries per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

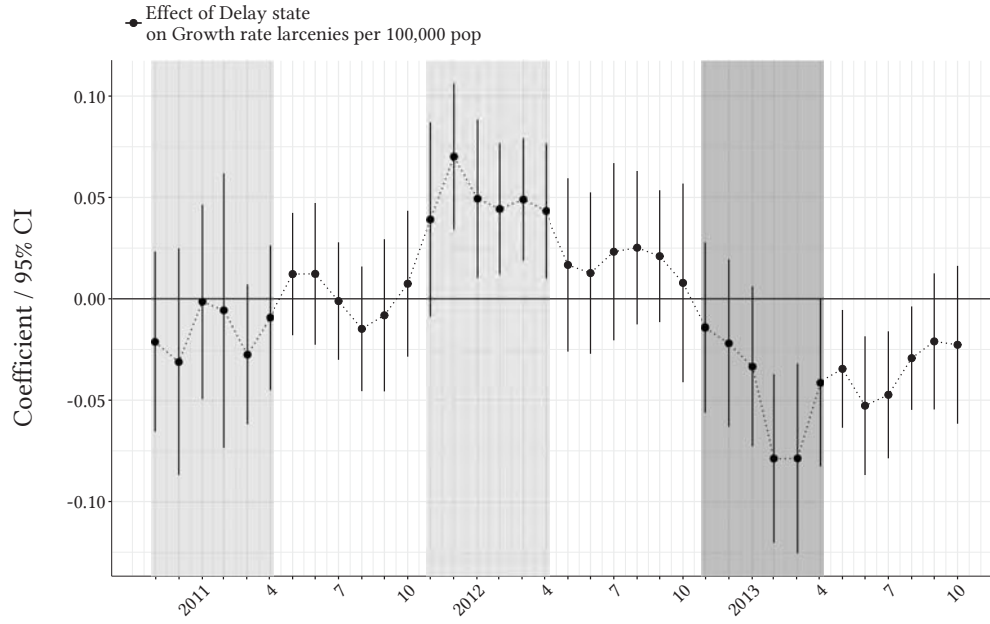


FIGURE 30: EVENT STUDY GRAPH FOR LARCENY RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log larcenies per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

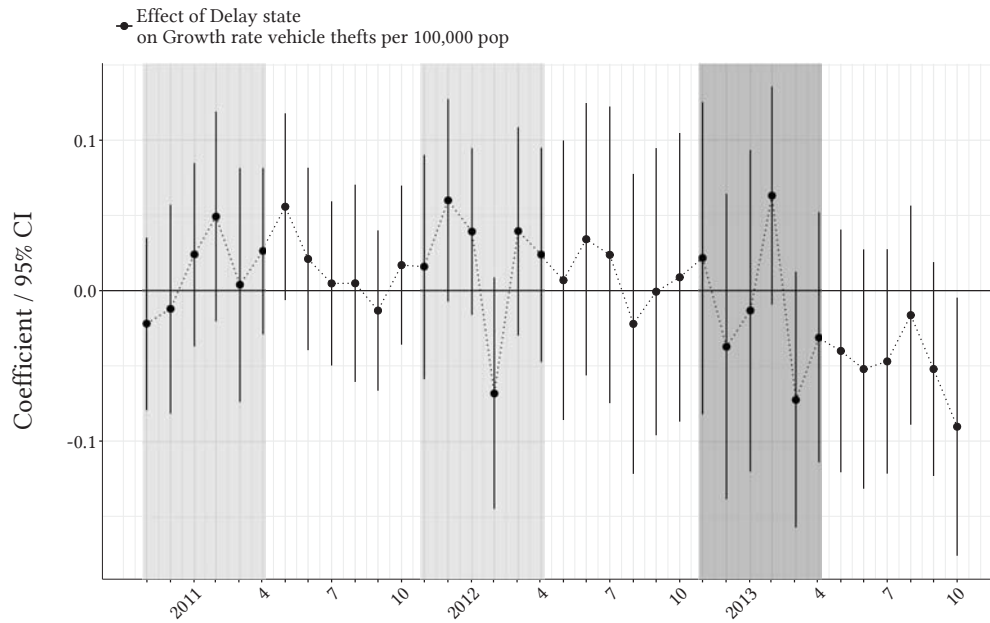


FIGURE 31: EVENT STUDY GRAPH FOR VEHICLE THEFT RATE

Coefficients and 95% confidence intervals for the effect of being in a *Delay* state on Δ_{12} Log vehicle thefts per 100,000 inhabitants for each month between November 2010 and October 2013. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years.

TABLE 36: ACCIDENTS AND SUICIDES

	Δ_{12} Log of mortality rate per 100,000 inhabitants											
	Accidents						Suicides					
	Any		Handgun		Other		Any		Handgun		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Delay×Post1	0.011	0.008	-0.000	-0.000	0.012	0.009	-0.008	0.004	-0.014	-0.004	0.002	0.008
	(0.015)	(0.015)	(0.003)	(0.004)	(0.015)	(0.015)	(0.014)	(0.018)	(0.012)	(0.013)	(0.013)	(0.016)
Delay×Post2	0.009	0.005	0.001	0.001	0.009	0.005	0.002	0.013	0.003	0.013	-0.000	0.005
	(0.015)	(0.019)	(0.002)	(0.003)	(0.015)	(0.019)	(0.015)	(0.018)	(0.015)	(0.017)	(0.013)	(0.017)
Year-Month FE	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
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
R ²	0.004	0.016	0.003	0.012	0.004	0.016	0.003	0.013	0.003	0.013	0.003	0.013

Notes: All regressions use 109,692 observations from 3,047 counties. The sample period is November 2010 until October 2013, i.e. an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Where no seasonal differencing is applied, the nominal sample period starts in November 2009. Standard errors clustered at the state-level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % blacks and % hispanics. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

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.⁴⁸

Columns 7 to 12 show the reaction of suicide rates. Importantly, those related to handguns do not show a significant reaction but are negative and thus point in the expected direction. Our findings are thus qualitatively in line with prior research which has argued that having a gun in the home is positively associated with suicide by firearm (Anglemyer, Horvath, and Rutherford, 2014). Concerning the size and lack of significance in columns 9 and 10, one has to bear in mind that 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 also seems unlikely 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.

⁴⁸Section 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.

F 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 November 2009 and October 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 60 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 40 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 2 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 30 days to be issued, and it is valid for one gun only.

TABLE 37: SOURCES USED FOR DETERMINING DELAY LAWS DURING THE SAMPLE PERIOD

State	Waiting Period	Source	Permit Delay	Source
AL	0	SFL (2017)	0	SFL (2017)
AK	0	SFL (2017)	0	SFL (2017)
AZ	0	SFL (2017)	0	SFL (2017)
AR	0	SFL (2017)	0	SFL (2017)
CA	10	SFL (2017), RAND (2018)	0	SFL (2017), RAND (2018)
CO	0	SFL (2017)	0	SFL (2017)
CT	0	SFL (2017)	60	SFL (2017), RAND (2018)
DC	10	RAND (2018)	0	RAND (2018)
DE	0	SFL (2017)	0	SFL (2017)
FL	3	RAND (2018)	0	SFL (2017)
GA	0	SFL (2017)	0	SFL (2017)
HI	14	SFL (2017), RAND (2018)	20	SFL (2017), RAND (2018)
ID	0	SFL (2017)	0	SFL (2017)
IL	3	SFL (2017), RAND (2018)	30	SFL (2017), *
IN	0	SFL (2017)	0	SFL (2017)
IA	3	RAND (2018)	0	RAND (2018)
KS	0	SFL (2017)	0	SFL (2017)
KY	0	SFL (2017)	0	SFL (2017)
LA	0	SFL (2017)	0	SFL (2017)
ME	0	SFL (2017)	0	SFL (2017)
MD	7	SFL (2017), RAND (2018)	30	SFL (2017), BJS (2005)
MA	0	SFL (2017)	40	SFL (2017), †
MI	0	SFL (2017)	5	SFL (2017), BJS (2005)
MN	7	SFL (2017), RAND (2018)	0	RAND (2018)
MS	0	SFL (2017)	0	SFL (2017)
MO	0	SFL (2017)	0	SFL (2017)
MT	0	SFL (2017)	0	SFL (2017)
NE	0	SFL (2017)	2	SFL (2017), RAND (2018)
NV	0	SFL (2017)	0	SFL (2017)
NH	0	SFL (2017)	0	SFL (2017)
NJ	7	SFL (2017), ‡	30	SFL (2017), RAND (2018)
NM	0	SFL (2017)	0	SFL (2017)
NY	0	SFL (2017)	180	SFL (2017), RAND (2018)
NC	0	SFL (2017)	30	SFL (2017), RAND (2018), §
ND	0	SFL (2017)	0	SFL (2017)
OH	0	SFL (2017)	0	SFL (2017)
OK	0	SFL (2017)	0	SFL (2017)
OR	0	SFL (2017)	0	SFL (2017)
PA	0	SFL (2017), ¶	0	SFL (2017)
RI	7	SFL (2017), RAND (2018)	0	SFL (2017), RAND (2018)
SC	0	SFL (2017)	0	SFL (2017)
SD	0	SFL (2017)	0	SFL (2017)
TN	0	SFL (2017)	0	SFL (2017)
TX	0	SFL (2017)	0	SFL (2017)
UT	0	SFL (2017)	0	SFL (2017)
VT	0	SFL (2017)	0	SFL (2017)
VA	0	SFL (2017)	0	SFL (2017)
WA	0	SFL (2017)	0	SFL (2017)
WV	0	SFL (2017)	0	SFL (2017)
WI	2	SFL (2017), RAND (2018)	0	SFL (2017)
WY	0	SFL (2017)	0	SFL (2017)

SFL (2017)=Siegel et al. (2017)

RAND (2018)=Cherney, Moral, and Schell (2018)

BJS (2005)=U.S. Department of Justice (2005)

*=<http://www.ilga.gov/legislation/ilcs/ilcs3.asp?ActID=1657>†=<https://malegislature.gov/Laws/GeneralLaws/PartI/TitleXX/Chapter140/Section131>‡=<https://law.justia.com/codes/new-jersey/2009/title-2c/2c-58/2c-58-2/>§=<https://www.ncleg.gov/enactedlegislation/sessionlaws/html/2013-2014/sl2013-369.html>¶=<https://www.psp.pa.gov/firearms-information/Pages/Pennsylvania-Instant-Checks-System---PICS.aspx>

G NICS Background Checks per State

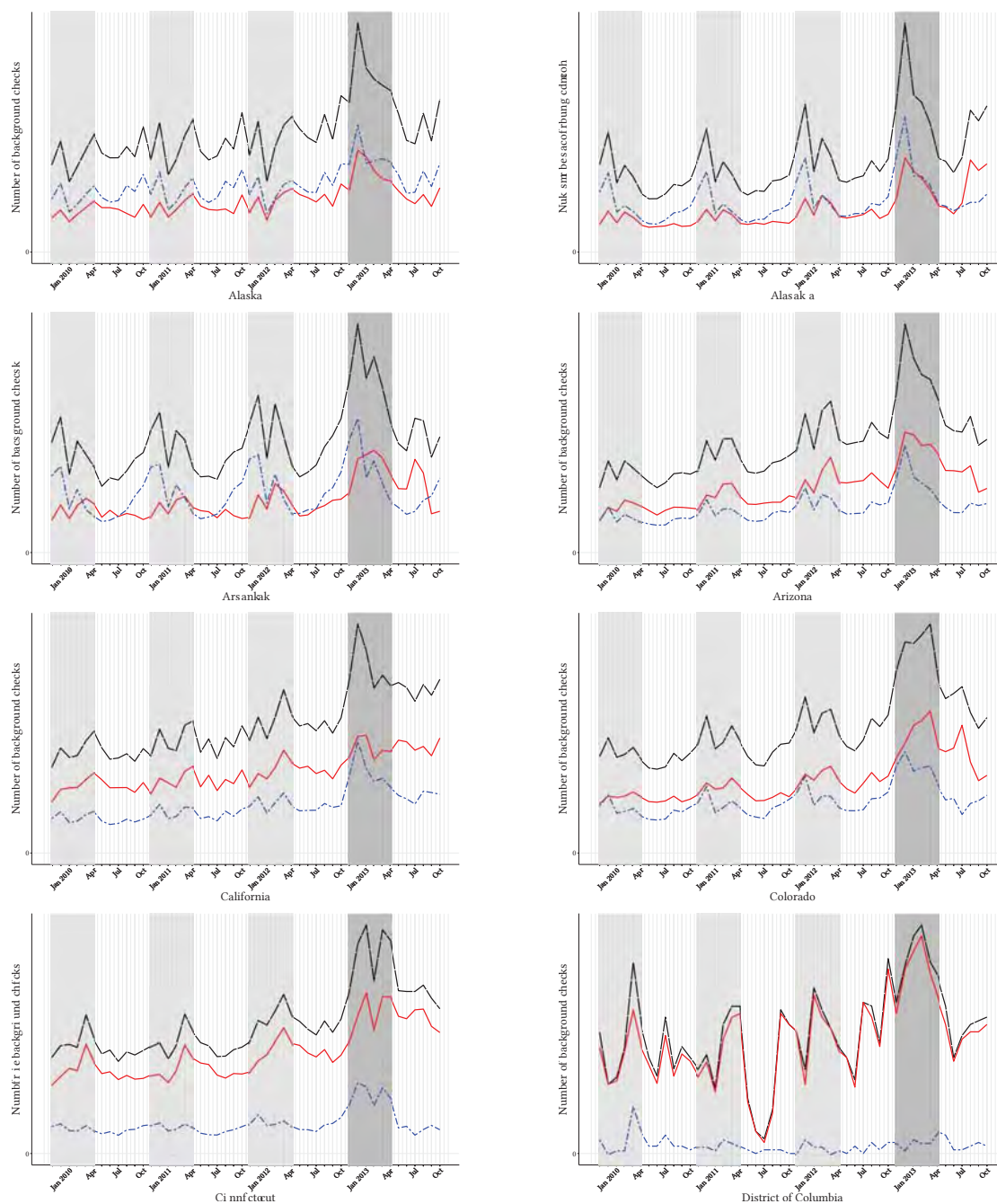


FIGURE 32: MONTHLY NICS BACKGROUND CHECKS, AL TO DC

Monthly state-wise NICS background checks plotted over time between November 2009 and October 2013 in absolute numbers. The red line shows background check for handguns, the blue line displays non-handgun checks, and the black line equals the sum of the two. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. Series of states removed from the NICS data as well as outlier dates are highlighted in yellow.

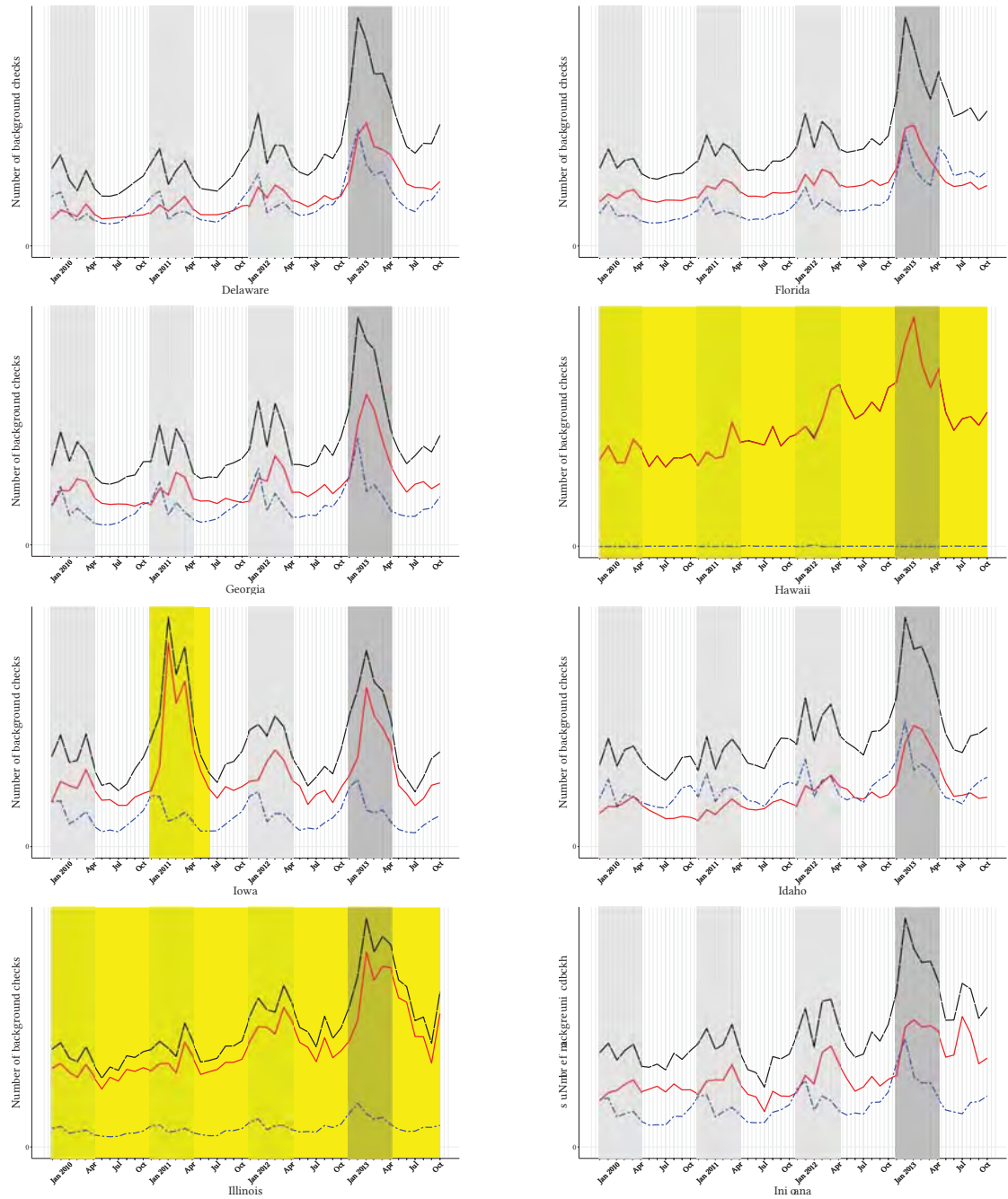


FIGURE 33: MONTHLY NICS BACKGROUND CHECKS, DE TO IN

Monthly state-wise NICS background checks plotted over time between November 2009 and October 2013 in absolute numbers. The red line shows background check for handguns, the blue line displays non-handgun checks, and the black line equals the sum of the two. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. Series of states removed from the NICS data as well as outlier dates are highlighted in yellow.

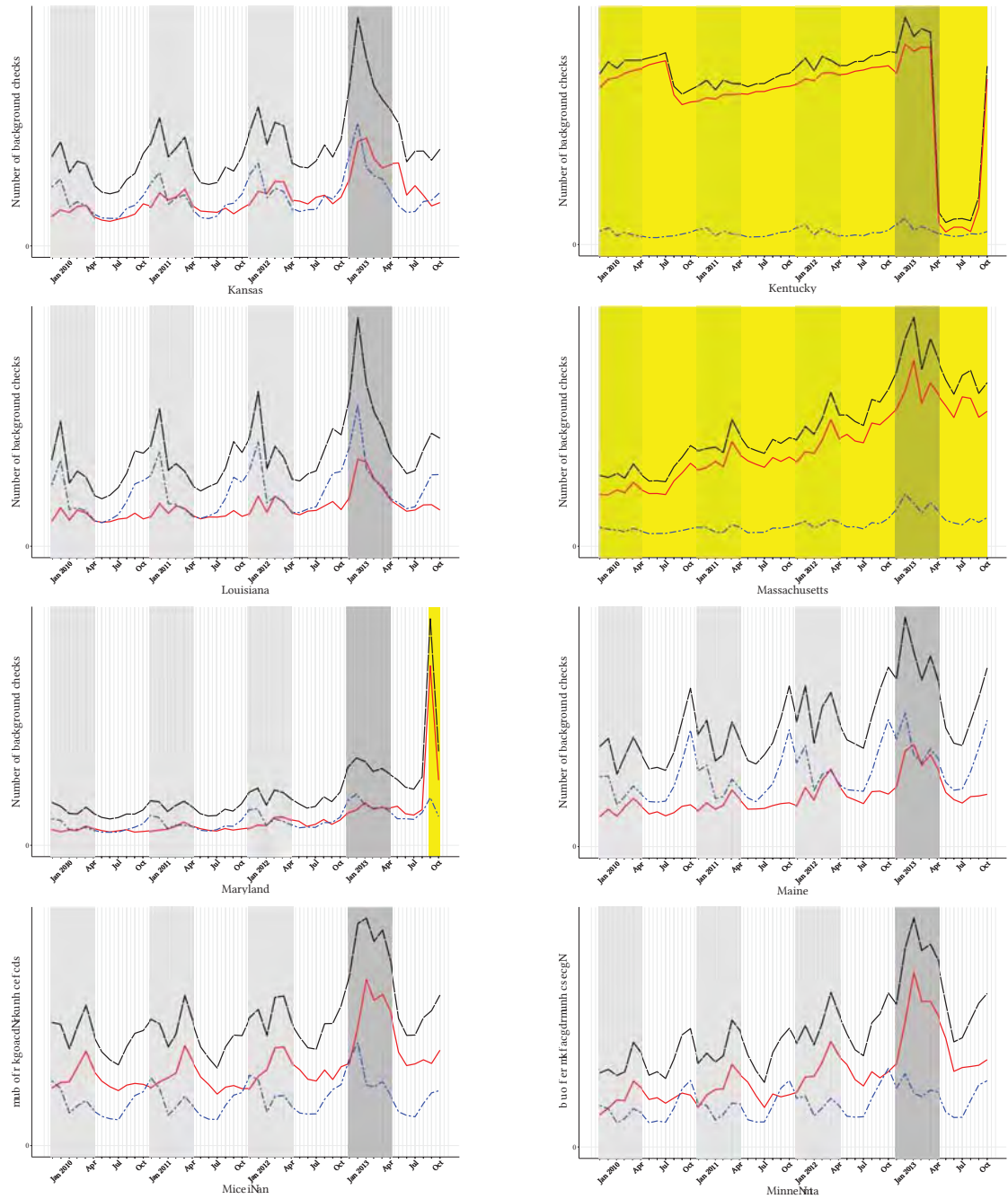


FIGURE 34: MONTHLY NICS BACKGROUND CHECKS, KS TO MN

Monthly state-wise NICS background checks plotted over time between November 2009 and October 2013 in absolute numbers. The red line shows background check for handguns, the blue line displays non-handgun checks, and the black line equals the sum of the two. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. Series of states removed from the NICS data as well as outlier dates are highlighted in yellow.

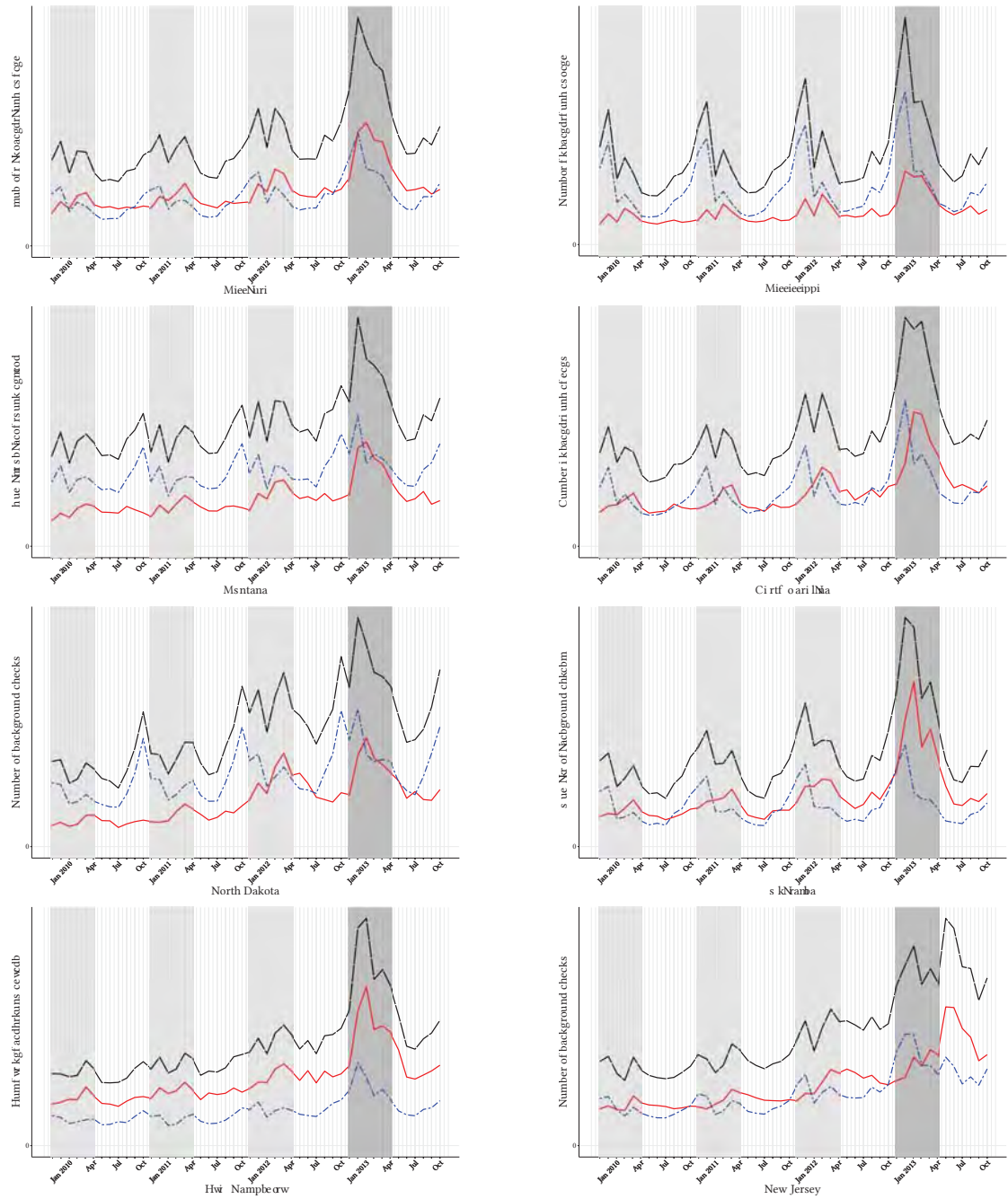


FIGURE 35: MONTHLY NICS BACKGROUND CHECKS, MO TO NJ

Monthly state-wise NICS background checks plotted over time between November 2009 and October 2013 in absolute numbers. The red line shows background check for handguns, the blue line displays non-handgun checks, and the black line equals the sum of the two. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. Series of states removed from the NICS data as well as outlier dates are highlighted in yellow.

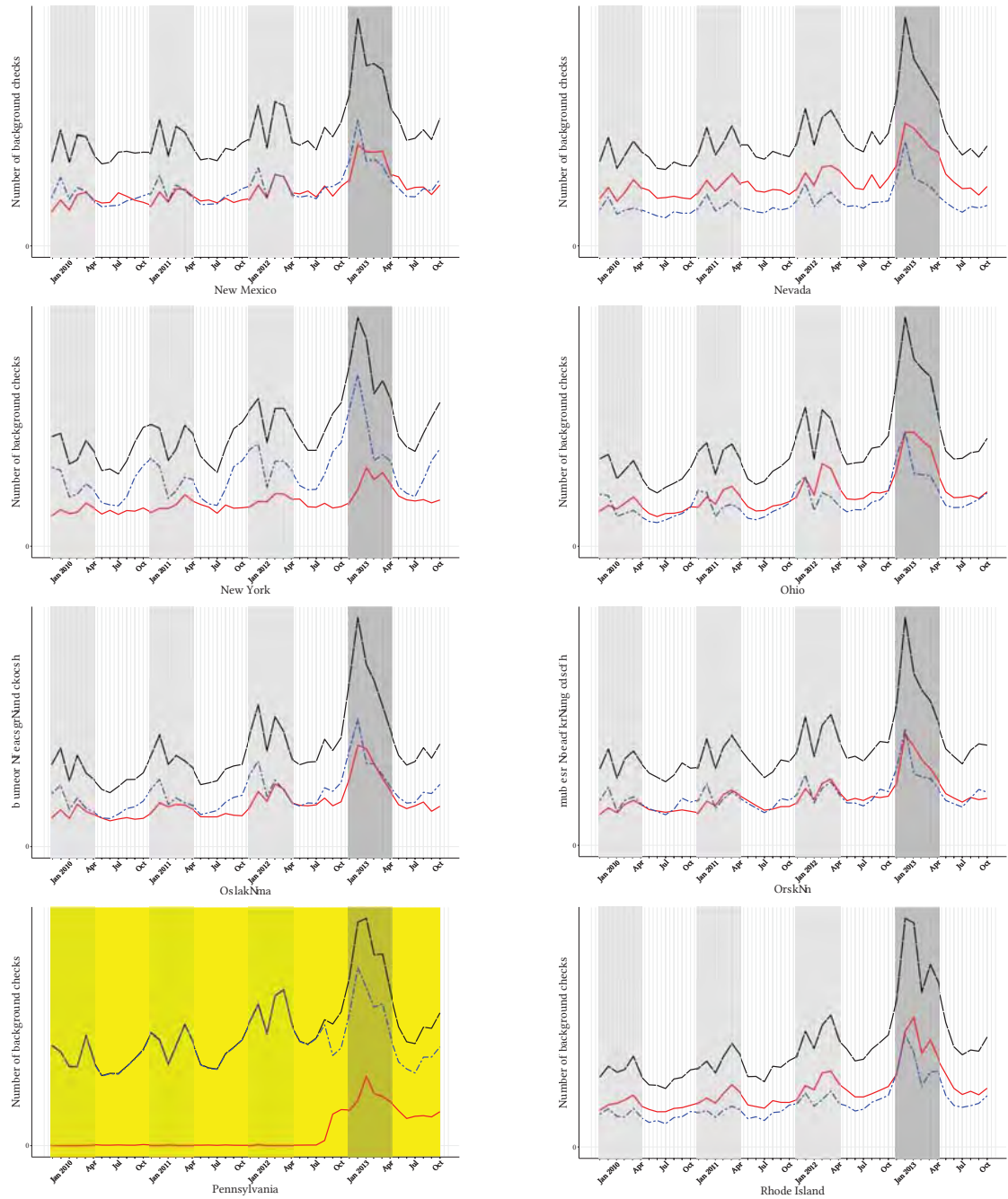


FIGURE 36: MONTHLY NICS BACKGROUND CHECKS, NM TO RI

Monthly state-wise NICS background checks plotted over time between November 2009 and October 2013 in absolute numbers. The red line shows background check for handguns, the blue line displays non-handgun checks, and the black line equals the sum of the two. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. Series of states removed from the NICS data as well as outlier dates are highlighted in yellow.

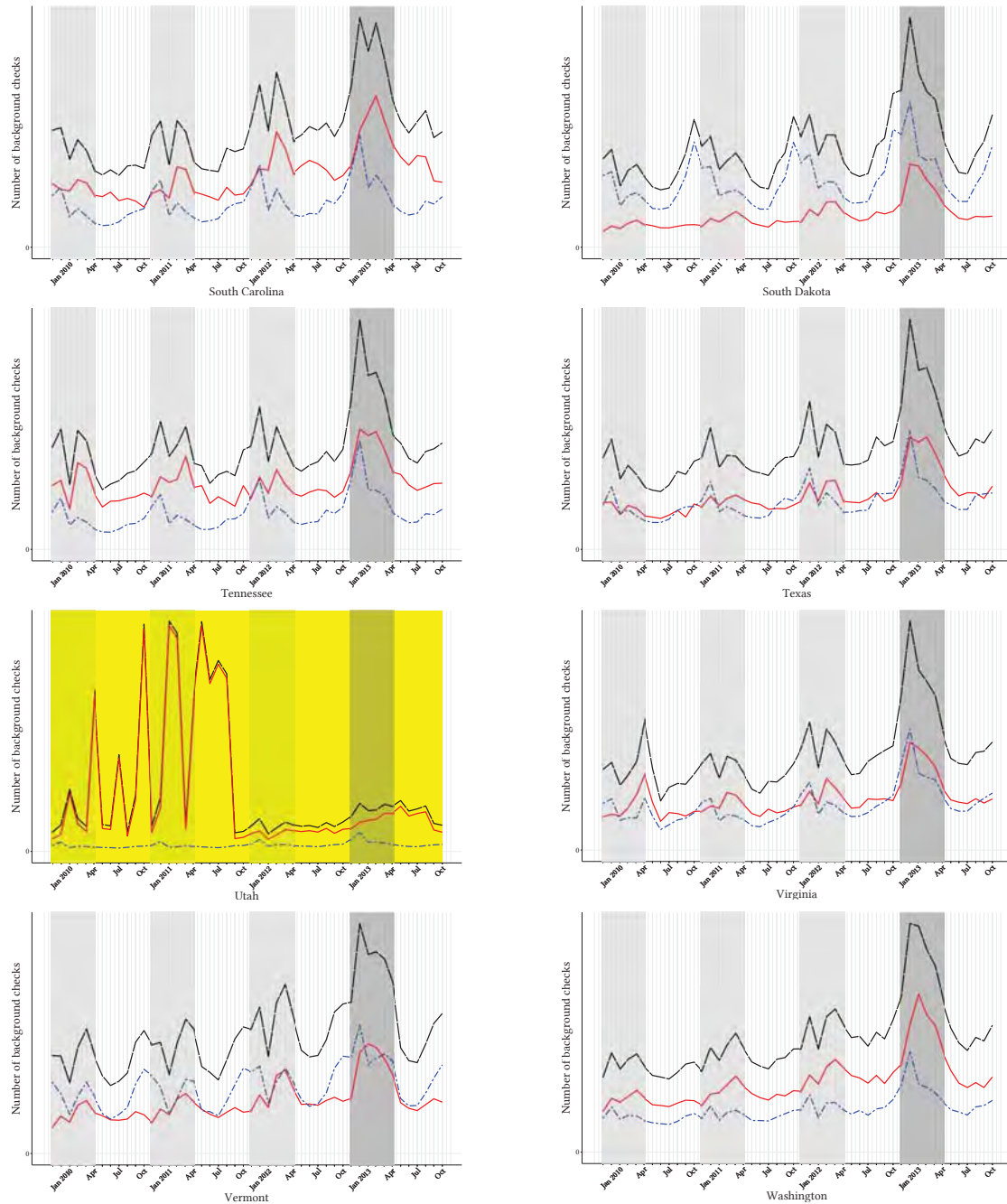


FIGURE 37: MONTHLY NICS BACKGROUND CHECKS, SC TO WA

Monthly state-wise NICS background checks plotted over time between November 2009 and October 2013 in absolute numbers. The red line shows background check for handguns, the blue line displays non-handgun checks, and the black line equals the sum of the two. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. Series of states removed from the NICS data as well as outlier dates are highlighted in yellow.

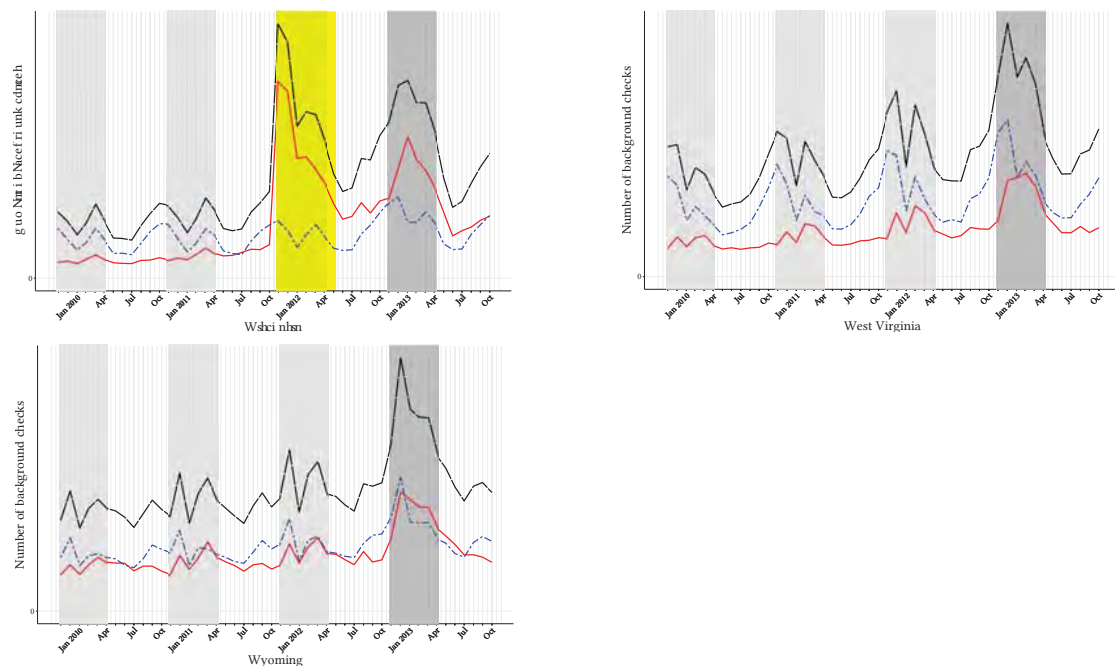


FIGURE 38: MONTHLY NICS BACKGROUND CHECKS, WI TO WY

Monthly state-wise NICS background checks plotted over time between November 2009 and October 2013 in absolute numbers. The red line shows background check for handguns, the blue line displays non-handgun checks, and the black line equals the sum of the two. The dark grey-shaded area includes the first six months after the 2012 election, i.e. November 2012 to April 2013. Light grey-shaded areas are marking the same period for preceding years. Series of states removed from the NICS data as well as outlier dates are highlighted in yellow.

H 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.⁴⁹ 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 -3.⁵⁰

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.

⁴⁹The 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.

⁵⁰In line with [Targonski \(2011\)](#) we ignore small negative values of at least -3. Those are usually corrections for misreporting in previous months.

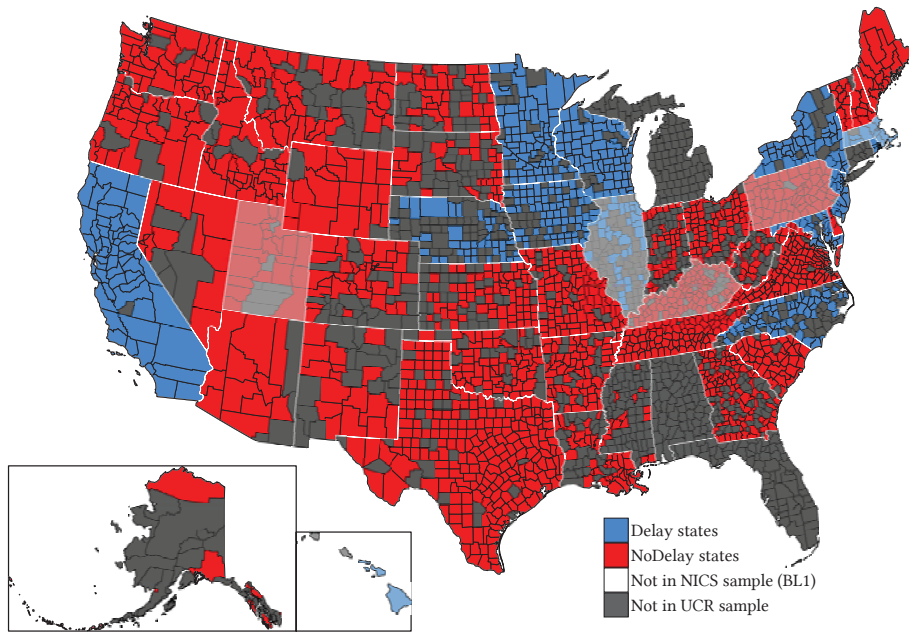


FIGURE 39: STATES AND COUNTIES REPRESENTED IN THE NICS AND UCR SAMPLES

Map of the United States showing the states contained in the NICS background check data and counties contained in the cleaned UCR crime data. Red counties are located in *NoDelay* states. Blue counties are located in *Delay* states. Shaded states are dropped in their entirety from the NICS sample. Dark grey counties are not present in the UCR sample. Note that the *NVSS* sample covers all counties in all states apart from Michigan and Connecticut.

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.