Dynamics in Gun Ownership and Crime – Evidence from the Aftermath of Sandy Hook

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Dynamics in Gun Ownership and Crime – Evidence from the Aftermath of Sandy Hook*

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Abstract

Gun rights activists in the United States frequently argue that the right to bear arms, as guaranteed by the Second Amendment, can help deter crime. Advocates of gun control usually respond that firearm prevalence contributes positively to violent crime rates. In this paper, we provide quasi-experimental evidence that a positive and unexpected gun demand shock led to an increase in murder rates after the mass shooting at Sandy Hook Elementary School and the resulting gun control debate in December 2012. In states where purchases were delayed due to mandatory waiting periods and bureaucratic hurdles in issuing a gun permit, firearm sales exhibited weaker increases than in states without any such delays. We show that this finding is hard to reconcile with standard economic theory, but is in line with findings from behavioral economics. States that saw more gun sales then experienced significantly higher murder rates in the months following the demand shock, as murders increased by 6-15% over the course of a year.

JEL codes: K42, H76, H10, K14

Keywords: Guns, crime, deterrence, demand shock, murder

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1 Introduction

Gun control has been a polarizing topic in United States politics over the past decades. The debate between gun rights and gun control advocates is fiercely fought, often relying on only a small set of arguments. Supporters of the right to possess firearms, as well as some conservative politicians often argue that arming citizens and abolishing gun-free zones will lead to decreases in violent crime. In 2012, a few days after the shooting at Sandy Hook Elementary School in Newtown, Connecticut, National Rifle Association (NRA) CEO Wayne LaPierre used the phrase "The only thing that stops a bad guy with a gun, is a good guy with a gun." (New York Times, 2012), to emphasize the importance of arming civilians to deter crime. To back their argument, gun rights proponents usually argue that while gun ownership has risen over the last decades, violent crimes have dipped. Gun control activists and many liberal politicians, on the other hand, point to the high numbers of violent crimes in the United States, in particular those committed with firearms. In their view, the significantly lower levels of homicides in similarly developed countries with stricter gun laws reflect a causal relationship between gun prevalence and crime rates (Brady Campaign, 2016). Additionally, they often assert that widespread availability of guns creates substantial risks to society if terrorists, convicted felons or domestic abusers obtain firearms easily and subsequently use them for criminal activities.

Since the political debate tends to be based on isolated observations and potentially spurious correlations, the provision of objective, scientific evidence seems imperative. More than 13,000 US residents are murdered every year and millions of Americans become victims in a crime. If a reduction or increase in gun ownership would effectively reduce these numbers, substantial welfare gains could be realized. This study therefore seeks to provide credible quasi-experimental evidence on the relationship between firearm purchases and crime rates. In contrast with large parts of the existing literature in economics, public health and criminology, we aim to move beyond mere correlational evidence and exploit arguably exogenous variation in the demand for firearms. This shift comes from a subset of the population which can reasonably be expected to buy guns for lawful purposes, such as self-defense.

In particular, we use a country-wide exogenous shock to firearm demand in the United States following the shooting at Sandy Hook Elementary School. Fear of tougher gun legislation and an increase in perceived need of self-defense capabilities drove up gun sales across the entire United States (Vox, 2016; CNBC, 2012). In most US states, citizens have instant access to firearms, i.e. they can take their gun home immediately after purchase. Some states, however, have implemented legislation intended to delay purchases, either by imposing mandatory waiting periods between purchase and receipt of a gun or by introducing other time-consuming bureaucratic hurdles such as making government issued gun purchasing permits mandatory. These delays led to differential firearm purchases following the shooting at Sandy Hook, a feature that we exploit to estimate changes in crime rates in a standard difference-in-differences setup.

In a first step, we show that handgun purchases in states with instantaneous access to guns increased more strongly following the tragic events at Sandy Hook. This finding is robust across several specifications and survives numerous robustness checks. Using Google search data, we rule out that these differences were counteracted by demand shifts to secondary markets (i.e. gun shows instead of licensed gun dealers), as the demand for gun shows did not tilt towards states with purchase delays. To identify the mechanism for the observed differential response in gun sales after the shock, we then analyze if there exists a similar difference in the intention to buy a firearm. Again using Google search data, we fail to detect significant differences in peoples' gun purchasing intentions. In other words, consumers in all states did not differ very much in their plan to buy firearms, but when it came to actually purchasing the gun, legislative delays led some consumers not to buy. This finding is hard to reconcile with common arguments from standard economic theory, such as transaction costs, but it is consistent with theories from behavioral economics. Leading explanations are partial projection bias or naïve present-bias.

The second step of our empirical analysis looks at the impact of the differential firearm purchases on various types of crimes. We find that states granting instant access to firearms see significantly more murders and manslaughters after Sandy Hook. After twelve months, allowing instant access to guns is associated with an estimated 6-15% increase in murder rates, which implies that between 45 and 102 lives could have been saved from murder in each month of 2013 if mechanisms to delay purchases had been in place in all US states. While murder and manslaughter rates increase, we also find that most other categories of crime remain unaffected apart from a small drop in simple assaults. This provides tentative evidence against the commonly made claim of guns leading to a credible deterrence effect, in which criminals avoid certain types of crimes, fearing backlash from armed victims. Additional analyses confirm the robustness of the effect on murders and we discuss why we deem our identifying assumptions to be valid.

This study is related to several streams of research. Scholars from the disciplines of economics, criminology and public health have previously tried to find empirical support for the relationship of firearm ownership and violent crime rates, with mixed results. Several studies find that more guns lead to more crime (e.g. Cook, 1978; Cook and Ludwig, 2006; Duggan, 2001; Hemenway and Miller, 2000; Kaplan and Geling, 1998; Miller, Azrael, and Hemenway, 2002; Miller, Hemenway, and Azrael, 2007; Siegel, Ross, and King III, 2013; Sorenson and Berk, 2001).[1] Other studies tackle the issue more indirectly by estimating the effect of gun legislation (Fleegler et al., 2013; Luca, Malhotra, and Poliquin, 2017) or gun shows (Duggan, Hjalmarsson, and Jacob, 2011) on crime rates, based on the idea that those in turn might influence gun prevalence.

The most prominent study to find a negative effect is the controversial book by Lott (2013).[2] He argues that the enactment of concealed carry laws has created a credible deterrent, such that criminals abstained from committing crimes, and that availability of firearms through this channel decreases violent crimes. His findings are supported by the results in Lott and Mustard (1997); Bartley and Cohen (1998) and Moody (2001). Other research suggests that there is no statistical relationship between gun prevalence and crime (Kates and Polsby, 2000; Kleck and Patterson, 1993; Lang, 2016; Moody and Marvell, 2005). All of these studies however rely on correlations for inference and, in the absence of credible identification, thus give rise for omitted variables bias. No clear effect is reported in Kovandzic, Schaffer, and Kleck (2013), in which the authors use an instrumental variables approach. Their suggested instruments for gun ownership however seem unlikely to satisfy the exclusion restriction (voter share for the Republican party, share of veterans and subscriptions to gun-related outdoor magazines).

Recent contributions attempt to causally identify effects using changes in gun laws and then comparing states with legislative changes to states where the laws remained intact. Ludwig and Cook (2000) for example are interested in the effects of introducing

[1] An excellent survey discussing in particular the early contributions is provided by Hepburn and Hemenway (2004), newer contributions are discussed by Kleck (2015).

[2] John Lott has been criticized for his methods and some have even accused him of fabricating data (see `https://web.archive.org/web/20130304061928/http:/www.cse.unsw.edu.au/~lambert/guns/lindgren.html`). Since it is not the scope of the paper, we leave it to others to judge the validity of his work.

waiting periods through the Brady Act. They find no clear-cut evidence that waiting periods contribute to changes in violent crimes. Rudolph et al. (2015) analyze the effect of the introduction of Connecticut's mandatory pistol purchasing permit in 1995 and find a strong decrease in homicides. The papers by Dube, Dube, and García-Ponce (2013) and Chicoine (2016) look at the expiration of the Federal Assault Weapons Ban and subsequent violence in Mexican municipalities. They find that the availability of assault rifles following the expiration significantly increased violent crimes.

Very much related is the study by Edwards et al. (2017) who look at within-state variation of waiting periods and purchasing permits over time to study the effect on suicides and homicides. They do not detect a significant impact on homicides. Using gun law changes to identify effects, however, might be problematic if buyers can anticipate legislative changes and adjust the timing of firearm purchases. In the case of the Brady Act, almost three years passed between the introduction of the bill and when it went into effect, leaving ample scope for behavioral adjustments. Additionally, gun laws might be passed endogenously as a reaction to trends in crime rates. We improve on this identification in two ways. First, the gun demand shock was unanticipated and consumers therefore were not able to adjust their purchases. Second, we use existing variation in state gun legislation and do not look at changes in gun laws. Additionally, since we find a strong effect in gun sales but not in the intention to purchase a firearm, we effectively demonstrate that these states are in fact comparable with respect to their reaction to the shock. We therefore contribute to the literature by providing well-identified evidence from an exogenously timed gun demand shock which allows us to more precisely determine the relationship of gun ownership and crime rates.

By offering an explanation for why gun purchasing delays may lead to fewer relative purchases that is based on findings from behavioral economics, we additionally contribute to the recent literature in public economics that analyzes the effect of behavioral biases for public policies (e.g. Chetty, Looney, and Kroft, 2009; Choi, Laibson, and Madrian, 2011).[3] We also relate to studies in economics that link behavioral shortcomings with criminal activity, violent behavior and policing. 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

[3]Excellent summaries on the application of behavioral biases in public policy are provided by DellaVigna (2009) and Chetty (2015).

unexpected losses of the home football team increase instances of domestic violence, and Mas (2006) observes a decline in policing quality after a lost salary arbitration by the respective police union in New Jersey.

Our results have important implications for other researchers and policy makers. First, we complement the literature on the relationship of gun ownership and crime by providing well-identified estimates of the effect of an exogenously timed change in firearm ownership rates. Since, according to anecdotal evidence, our sample of interest purchased firearms for lawful purposes and not for criminal activity, the significant increase in murder rates seems even more striking, especially since Fabio et al. (2016) report that only a small fraction of crimes are committed using legally acquired firearms. Second, our findings do not detect much of a deterrence effect, suggesting that criminals either do not anticipate changes in firearm ownership correctly, or that they have a negligible impact on their choices. Therefore, arming citizens to prevent crimes does not seem to be a very promising approach. Third, our findings could prove very helpful to legislators deciding about gun control measures, as designing effective regulations can have non-negligible welfare effects, and save a substantial number of lives. Waiting periods and purchasing permits appear very promising to at least somewhat reduce impulsive acts of violence. Fourth, this paper suggests that cognitive biases and limitations established by the growing field of behavioral economics can meaningfully be applied in a context of public policy and research on crime, and should thus be taken into account when modeling individual behavior of gun owners and criminals.

This paper is organized as follows: Section 2 provides details about gun laws in the United States, describes the tragic events at Sandy Hook Elementary School and the subsequent firearm demand shock. Section 3 in turn describes our data in detail and explains our estimation strategy. Results can be found in Section 4 and Section 5 concludes.

2 Background

2.1 Gun Laws in the United States

The Second Amendment to the United States Constitution protects the right of citizens to keep and bear arms. The federal government, as well as state and local governments have in the past however enacted laws that make it harder or require more effort from citizens to acquire firearms. On the federal level, the most important pieces of legislation for this study 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 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. 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.[4]

Most of the constraints on private firearm ownership at the state level attempt to either prohibit convicted felons or otherwise potentially dangerous people from acquiring guns for non-lawful purposes, or restrict the usefulness of firearms for non-lawful purposes independent of the buyer. One restriction of substantial interest to our study 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).

[4]Excellent overviews of all restrictions in the respective states can be found in The Brady Campaign (2013), NRA (2016) and Law Center to Prevent Gun Violence (2016).

As of 2016, nine states and the District of Columbia have imposed mandatory waiting periods. California and D.C. require ten days, Hawaii 14 days, Rhode Island seven days and Illinois between 24 hours (long guns) to 72 hours (handguns) on all firearm purchases. Minnesota is the only state to require seven days wait between purchase and pickup of handguns and assault rifles only. Maryland and New Jersey impose seven days for handguns, while Florida and Iowa impose a three day waiting period for handguns. Wisconsin has repealed its 48 hour waiting time on handguns in 2015.

Furthermore some states require a license to possess or buy a firearm prior to the actual purchase, which due to bureaucratic hurdles can also impose a waiting time. In Connecticut, a handgun eligibility certificate may take up to 90 days before being issued. Before buying a gun in Hawaii, prospective gun owners have to obtain a permit to purchase which can take up to 20 days to be issued. Buyers in Illinois have to obtain a Firearm Owner's Identification card (FOID) before being allowed to purchase an unlimited number of firearms in the following ten years. Obtaining an FOID can take up to 30 days. The state of Maryland requires buyers to hold a Handgun Qualification License which will be issued or denied within 30 days of application. In Massachusetts, authorities may take up to 30 days to process a request for a license to carry or a Firearm Identification Card (FID), where the former allows unlimited purchases of any firearms without additional paperwork and the latter is restricted to rifles and shotguns. Residents of New Jersey in turn must obtain a permit to purchase a handgun for each purchase separately, while they can purchase unlimited shotguns and rifles with a Firearms Purchaser Identification Card (FPIC). Authorities may take up to 30 days to issue such a permit. In New York, a license to possess or carry a handgun is necessary for each gun and obtaining one can take up to six months. In North Carolina, a license to purchase a handgun can take up to 14 days to be issued, and it is valid for one gun only. Residents of Rhode Island need to wait up to 14 days to receive their pistol safety certificate (blue card). Table 1 summarizes the waiting periods and license requirements for handguns across states.

2.2 The Shooting at Sandy Hook Elementary School

On the morning of December 14, 2012, then 20-year-old Adam Lanza, a resident of Newtown, Connecticut, first shot and killed his mother at their home before driving to

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

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

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

Sandy Hook Elementary School, where he shot and killed six adult school employees and 20 students, who were between six and seven years old. Although the carnage only lasted about five to ten minutes, Lanza was able to discharge his firearms (a semi-automatic AR-15 type assault rifle and a pistol) 156 times, averaging approximately one shot fired every two to four seconds. He committed suicide shortly after the first law enforcement officers arrived at the scene. Eyewitness and police reports describe that Lanza was acting calmly throughout and killed his victims with targeted shots to the head. Even after several years, his motives are still not fully understood. Lanza did not leave any documents that could explain his thoughts, 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 US high or grade school and the third deadliest mass shooting in US 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 ideas to President Obama in January 2013, who announced to proceed with 23 executive actions. These were aimed at improving background checks, addressing mental health issues and insurance coverage of treatment thereof, 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.

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

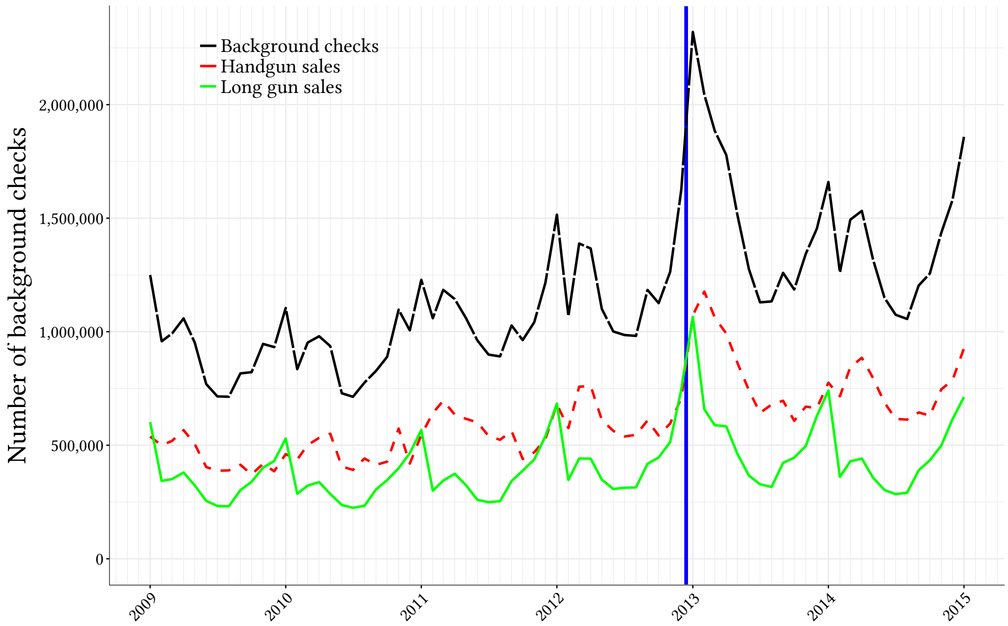

FIGURE 1: NICS BACKGROUND CHECKS BEFORE AND AFTER SANDY HOOK

Monthly federal NICS gun sale background checks plotted over time between 2009 and 2015 in absolute numbers. The blue vertical line marks the date of the shooting at Sandy Hook Elementary School. The red line shows background check for handguns, the green line for long guns, and the black line displays the sum of the two.

for the first time." (The Intercept, 2015). But the increased interest in firearms was not just restricted to brick-and-mortar stores. In a recent paper, Popov (2016) shows that prices for gun parts in an online market sharply increased by 20% after Obama's announcement for tougher gun legislation. Figure 1 shows the spike in gun sales – it displays the evolution of firearm background checks over time, before and after the Sandy Hook shooting. While gun sales generally increase at the end of the year, the spike following the Sandy Hook shooting is much more pronounced than in the years immediately before and after. It is these new customers and their differential propensity to acquire firearms in some states that we seek to exploit for our data analysis.

2.3 A Behavioral Motivation for Firearm Purchase Delays

There exist several theoretical approaches that could explain why a purchasing delay would result in some individuals not buying guns at all. In the following, we therefore provide insights from standard economic theory, as well as behavioral economics that predict differential purchasing decisions depending on whether delays exist or not. We also establish more precise conditions under which these theories hold, such that our data analysis can deliver supporting or opposing evidence for the theories in question. Note that the purpose of this exercise is not to support our empirical findings, since our results hold independent of any theory. We rather aim to provide a theoretical foundation to shed light on possible reasons behind the observed facts, and as a by-product are able to test which theory best explains the patterns evident in the data.

In standard economic theory, the primary reason for differential purchasing reactions given identical preferences root in differing transaction costs. Without delays, purchasing a gun requires the prospective buyer to travel to the gun store, file the necessary paperwork, and take home their gun, all of which creates costs. Delays, however, require additional effort. In the case of waiting periods, a prospective buyer not only has to travel to the gun store and file the necessary paperwork, he or she would also have to come back after a few days to pick up their gun. The additional costs associated with a second visit to their gun dealer can outweigh the net benefit of purchasing, and therefore prevent some marginal customers to buy a firearm. If for example each trip to a gun dealer generates utility losses of c, and a gun provides utility gains of v, the assumption $2c > v > c$ is sufficient to observe a differential reaction depending on whether the state implemented waiting periods or not. Mandatory handgun permits that are only issued after a delay create a similar effect. Before prospective buyers can undertake their trip to the gun store, file the paperwork and take home their gun, they have to travel to their closest public authority commissioned with issuing these types of permits, file the necessary paperwork there and wait for the permit to be issued. If the costs associated with getting a permit are k, then $k + c > v > c$ will lead to states with delayed permits experiencing lower rates of gun purchases. Importantly, transaction costs in a full information setup with perfectly rational agents require that there already exists a differential interest in buying a firearm between the onset of the demand shock and the act of buying the gun.[5] This is due to the fact that buyers incorporate transaction costs in their decisions and then either buy a gun or not.

Other reasons why we would observe a differential reaction depending on the implementation of delays is due to arguments from behavioral economics. Leading explanations include (partial) projection bias (Loewenstein, O'Donoghue, and Rabin, 2003) and (naïve) present-biased consumers (O'Donoghue and Rabin, 1999, 2001).[6] Under partial

[5] We will discuss the appropriateness of the assumption of full information about delay legislation when we present our results regarding the intention to buy a firearm.

[6] Section C.1 in the appendix discusses additional theories.

projection bias, consumers project current preferences to future decisions, as they only partially anticipate changes in preferences that might happen once they move away from the current state. If decision makers are sufficiently aware of the possibility of changing preferences in the future, they might decide to not buy a firearm eventually. Under present-bias (O'Donoghue and Rabin, 1999) where the future is uniformly discounted with $\beta < 1$, and in the absence of delays, prospective buyers will decide to buy if $v > c$. When delays are present however, this condition becomes stricter $\beta v > c$, because the utility from owning the gun has been shifted to the future and is therefore discounted by the prospective buyer. Depending on the ratio of v and c, and their relationship with β, some present-biased consumers will therefore not buy a gun when facing delays.

The difference between these behavioral patterns and assuming transaction costs is however that under present-bias or partial projection bias, the intention of purchasing a gun can be identical between states that impose delays and states that do not. For the case of present-bias, this can easily be seen in a two-period model, in which a gun can be purchased in either period 1 or period 2. While a naïve present-biased prospective buyer will not buy in period 1 because $\beta v - c < \beta(v - c)$ when $\beta < 1$, they still believe in period 1 that they will buy eventually in period 2, because $\beta v - \beta c > 0$. This behavior arises because the prospective buyer underestimates how heavy the costs of buying the gun will weigh in the future. He therefore might make plans to purchase a gun in the next period, but never follows through. For partial projection bias, the decision maker might form intentions to purchase, but abstain from buying as he expects his actual preferences to realize differently when making the purchase.

We can test whether transaction costs on the one hand or motives from behavioral economics such as partial projection bias or present-bias on the other hand explain a divergence in gun sales. If we find evidence that prospective buyers exhibit strong differences when it comes to their intention to buy a gun after the shock, this can be reconciled with transaction costs but not with behavioral arguments. In contrast, if we observe differential purchasing behavior while the interest in buying a gun is independent of whether the state implemented a delay, this is evidence for motives from behavioral economics to play a role.

3 Data & Estimation Strategy

3.1 Estimation Strategy & Identification

Following the shooting at Sandy Hook, firearm demand in the United States increased strongly, both for fear of tougher legislation, as well as a higher perceived need of self-protection. As some states allow their residents to instantly purchase the guns of their choosing, the higher demand in those states could immediately translate into increased sales. States that were imposing mandatory waiting periods or that had a time-consuming application process for purchasing permits, however, were able to delay transactions, possibly discouraging buyers from eventually buying any guns. We therefore define all states that had a positive waiting period for handguns or that require a time-consuming permit to be issued prior to purchase as "delayed states", as listed in Table 1: California, Florida, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Minnesota, New Jersey, New York, North Carolina, Rhode Island, Wisconsin and the District of Columbia. All other states we subsume under "instant states".[7] Connecticut is removed from all samples, since that state might have been affected differently by the shooting at Sandy Hook, as Newtown lies in Connecticut.[8]

We proceed by first showing that delayed states have a smaller increase in gun sales than instant states. Then, we continue by investigating the effect of differential firearm purchases on crime rates. There exist several potential outcomes for such an analysis. First, crime rates could increase as new gun owners might turn criminal. For example, a domestic dispute otherwise gone unnoticed to law enforcement might suddenly turn violent with one spouse shooting and killing the other. It is also conceivable that if new gun owners are marginally law-abiding in the sense that their low income is weakly preferred to being criminals, any income shock might turn them criminal, a profession potentially more lucrative for someone armed. Second, crime rates could decrease, because armed citizens serve as a credible deterrent to criminals. Robbing someone on

[7] We do not utilize the length of the delay for our analysis for two reasons. First, using the absolute length of the delay essentially puts a high weight on New York due its very long delay. Second, the ex ante effect of a longer delay seems unclear. While one can argue that a small delay does not pose a substantial hurdle and should therefore not generate strong differences in sales, it could also be argued that small delays only deter those who suffer from behavioral biases the most. This could amplify the effect. A simple binary measure abstracts from this issue.

[8] None of our results depend on this decision, as the point estimates remain virtually identical. Appendix Section B contains the results when including Connecticut for the two most important tables from our results section. All other tables are available from the authors.

the street or burglarizing someone's apartment becomes more dangerous if the likelihood of the victim being armed is higher, therefore decreasing the relative profitability of being a criminal. Additionally, engaging in bar fights or similar altercations suddenly becomes more risky if the opponent has a gun at their disposal. Third, crime rates could overall stay unchanged, but there could be a shift from less severe to more severe crimes. Speaking in the examples above, the domestic dispute or the bar fight due to the availability of lethal force could turn from assault to murder, effectively reducing crimes in one category and raising it in the other.

Our data analysis uses a classical difference-in-differences (DiD) approach, in that we compare crime changes due to the Sandy Hook shooting between delayed and instant states. Our estimation equation reads

$$y_{it} = \alpha + \gamma_i + \lambda_t + \beta(POST_t \times INSTANT_i) + \delta \mathbf{X_{it}} + \gamma_i t + \epsilon_{it} \tag{1}$$

where y_{it} is our measure of gun purchases in the first step of the analysis, and our measure of crime rates in the second step, γ_i denotes location and λ_t time fixed effects. Our coefficient of interest is β, capturing the joint influence of dummy $POST_t$ (all time periods after the shooting at Sandy Hook) and dummy $INSTANT_i$ (locations that belong to instant states). $\mathbf{X_{it}}$ represents a vector of time-invariant control variables interacted with time fixed effects, thus allowing for a time-varying influence. $\gamma_i t$ is a time trend, allowed to vary within each location unit and ϵ_{it} is the error term.

To assure credible identification and validity of our DiD estimator, we need two assumptions to be fulfilled: First, we must not have differential trends in the absence of treatment in our outcome measures to ensure that divergence between instant and delayed states is indeed driven by the treatment. We will address this concern by allowing for location-specific time trends and investigating changes in gun purchasing intentions to establish comparability of instant and delayed states. Second, there must not have been other events responsible for the divergence occurring at approximately the same time as the treatment. Placebo regressions and shifting the onset of the treatment will deliver evidence that the effect is particular to a very small time window that coincides with the shooting. We furthermore argue that the timing of the shooting at Sandy Hook is entirely exogenous to any relevant outcome variables: The perpetrator

Adam Lanza apparently had prepared the crime for years, investigators believe he downloaded videos and other materials related to the shootings at West Nickel Mines School (2006) and Columbine High School (1999) on his computer. The shooting came as a surprise to law enforcement and Lanza's social environment and apparently were not triggered by any other public events. Therefore, we assume strict exogeneity of the event to our outcome variables.

Additionally, in order to establish a causal link between the demand shock and crime rates, credible identification requires that the shock only affected firearm demand and not crime rates directly other than through the changes in gun prevalence. Given that a mass shooting itself is a crime and might therefore differentially influence attitudes towards violence, we will address this potential issue by extending our analysis to include the date of the 2012 Presidential election, to see if the re-election of President Obama (a non-violence related gun demand shock) contributes to our estimated coefficient from the shooting at Sandy Hook. Furthermore, we include a large set of covariates with time-varying influence that are commonly associated with determining crime rates, to net out the effect of observables on both, gun sales and crime rates.

3.2 Datasets

3.2.1 Firearm Ownership & Demand

Unfortunately, no reliable information about gun ownership and inflow of new guns exists at a sufficiently fine geographic (e.g. county) level. Researchers therefore have relied on proxies for gun ownership levels, including subscription to a gun magazine (Duggan, 2001), fraction of suicides committed with a firearm (Cook and Ludwig, 2006; Azrael, Cook, and Miller, 2004; Kleck, 2004), and gun ownership questions from the General Social Survey (GSS) (Glaeser and Glendon, 1998).

Since we are interested in the increase in gun ownership rather than the stock of guns, we use applications from the National Instant Criminal Background Check System (NICS), which is available from the FBI for each state and month since 1998 and has already been used for this purpose before (Lang, 2013, 2016).[9] Each purchase of a new firearm at a federally licensed firearm dealer triggers an application for a background

[9] The data can be downloaded at `https://www.fbi.gov/file-repository/nics_firearm_checks_-_month_year_by_state_type.pdf`.

check.[10] In total, we obtain monthly data for background checks of handgun purchases in 45 states (and the District of Columbia) between November 1998 and December 2014.[11]

While the NICS data gives us a good idea of actual firearm purchases, we would also like to measure the ex ante interest in buying a firearm. We therefore extract daily search data from Google Trends (`http://google.com/trends`) for the expression 'gun store' for each state between 2009 and 2014. Google Trends is a data service that reports the relative frequency of specific Google search expressions across time and geography. The search term 'gun store' has been shown to be highly predictive of the willingness to purchase a firearm in the time-series dimension (Scott and Varian, 2014). Unfortunately, Google Trends data is always rescaled and sometimes censored, such that some manual conversions are needed to make meaningful comparisons. First, for each query, Google rescales data to be between 0 and 100, where 100 is assigned to the largest value in the entire time window of the query. Thus, we only obtain data on the relative occurrence of the expression in one particular state on each date within a 3 month time window. To correctly adjust the results in the time dimension, we designed the queries such that months were overlapping, for example the first query downloaded January to March data, the second March to May, the third May to July and so on. We then rescaled the data based on the overlaps using January of 2009 as a baseline. In order to also make the cross-section comparable, we designed a query over the entire time period across all states to obtain relative weights for each state (Durante and Zhuravskaya, 2018). Second, Google Trends data is censored to zero if the number of searches falls below a certain, not publicly known threshold. Since we have no reason to believe that censoring affects states across our treatment groups differently, we do not employ corrective action.

Note that our NICS measure of changes in gun ownership potentially misses some trades on secondary markets (e.g. through gun shows). 2016 Democratic nominee for President Hillary Clinton's campaign has suggested multiple times that up to 40% of guns are purchased at gun shows, a number that has however been criticized to be

[10] State permit holders to purchase a gun are exempt from the background check, if the process of obtaining their permit involved passing a background check (since November 30, 1998 only NICS background checks qualify).

[11] Alabama, New Jersey, and Maryland changed their gun laws during the period of our study. This leads to strong spikes in the data. For Pennsylvania, we observe zero background checks during 2012 and data from Hawaii does not allow to distinguish between permits and gun sales. Finally, Kentucky frequently rechecks permit holders, which also leads to unnatural spikes in the data.

misleading (Washington Post, 2015). Duggan, Hjalmarsson, and Jacob (2011) instead reports that in 1993/1994, transactions at gun shows only accounted for about 4% of all firearms transactions. A study by the Bureau of Alcohol, Tobacco and Firearms (1999) furthermore estimates that 50-75% of dealers at gun shows are federally licensed, therefore being required to perform background checks on all transactions, even at gun shows. Additionally, eighteen states and DC have passed laws that require federal background checks in most or all private transactions. Therefore, the majority of transactions at gun shows should be reflected in the NICS background checks. Since most of the states that employ some form of purchase delay are also those states that require background checks for private transactions, we would at most underestimate the difference in firearm acquisitions between instant and delayed states.

To alleviate remaining concerns, we collect daily data from Google Trends for the search expression 'gun show' for each state between 2009 and 2014. We employ the same scaling procedure as explained above and thus obtain a measure that allows us to investigate the temporal development of demand for gun shows across states with or without waiting periods or delays. Addtionally, we collect data on the location and timing of gun shows. The website `http://www.gunshowmonster.com/` provides a database of future and past gun shows across the United States. The website allows users to make submissions, which after editorial approval will be published and therefore provides decent coverage: our sample contains 8764 gun shows between July 2009 and December 2014 in almost all US states. We aggregate gun shows on the county level for each month. Note that the sample is surely incomplete and possibly even skewed towards certain states with easier access to guns. We therefore only use this data in some supplementary estimations to show that the effects regarding the supply and demand for gun shows go in similar directions. Figure 7 in the appendix shows the locations of gun shows in 2012 and 2013 using green circles, where the circle size increases in the number of gun shows at a certain location.

3.2.2 Crime

As our outcome measure in the second step, where we investigate the effects of gun ownership on crime rates, we use the FBI's Uniform Crime Reports (UCR): Offenses Known and Clearances by Arrest (USDOJ: FBI, 2013; USDOJ: FBI, 2014a; USDOJ:

FBI, 2015a).[12] Approximately 18,000 federal, state, tribal, county and local law enforcement agencies voluntarily submit detailed monthly crime data, either through their state's UCR program or directly to the FBI, about offenses known to these agencies. Variables include the monthly count of different types of crime for each law enforcement agency, such as murder, manslaughter, rape, assault, robbery, burglary, larceny and vehicle theft. The data also comprises variables that distinguish the type of weapon used (e.g. firearm, knife, strong arm) for example in robberies and assaults, and they allow to distinguish between severity for some crimes, such as simple assault versus aggravated assault, or forcible and non-forcible rape.

Unfortunately, some agencies in the data set are not reporting consistently. Common reporting mistakes include large negative absolute values for crimes, or continuously reporting zero crimes. We address this issue by following the guidelines for cleaning UCR data from 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.[13] 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

[12] For a placebo regression using different years later in the main text we additionally use (USDOJ: FBI, 2011; USDOJ: FBI, 2012).

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

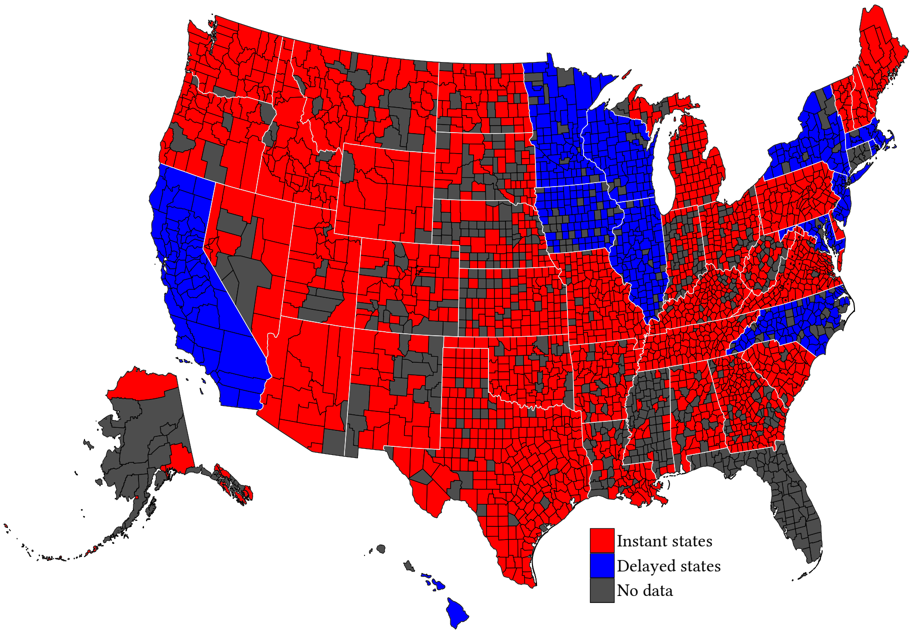

Figure 2: Counties represented in the UCR sample

Map of the United States showing our UCR sample. Red counties are located in instant states. Blue counties are located in delayed states. Grey counties are not present in the sample.

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.[14]

In addition to the approach by Targonski (2011), we drop data from all counties that do not report for the full time period of our study, and all counties that always report zero crimes. If all reporting agencies in any county cover less than 50% of the county's population, we also remove these data points to make sure that control variables reflect the characteristics of the sample.[15] We then aggregate the crime data for all cases of murder (with and without firearms), manslaughter, rapes, robberies (with and without firearms), aggravated assaults (with and without firearms), simple assaults, burglaries (including forced and non-forced entry), larceny, and vehicle theft, for each US county that we have data for, and for each month between December 2011 and November 2013. This generates a data set of 15 types of crimes in 2,441 counties and approximately 58,000 observations. Figure 2 shows all counties remaining in our sample.

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

[15] The decision to set the cutoff at 50% was made arbitrarily, but even keeping all counties does not qualitatively change our results. Details will be given in the results section.

3.2.3 Crime: Supplementary Data Sets

To obtain homicide data for the counties not present in our UCR data set, we obtain the universe of death certificates from the National Vital Statistics System (NVSS) through the Center for Desease Control and Prevention (CDC). This data set contains ICD-10 codes for each death recorded in the United States at the county of residence. We use this data set to generate county totals of homicides with and without firearms.

3.2.4 Controls

To control for potential confounds and account for differences in socio-economic characteristics across counties, we obtain several covariates. In selecting variables, we follow the choice of controls from the many correlational studies that investigate the relationship of firearm prevalence and crime (e.g. Cook and Ludwig, 2006; Kovandzic, Schaffer, and Kleck, 2012, 2013). From the 2010 US Decennial Census, we use % rural, % blacks, % hispanics, the log of median income and % males aged 18-24.[16]

4 Results

4.1 The Firearm Demand Shock After Sandy Hook

Since we predict some people to abstain from firearm purchases in delayed states, the first step of our analysis investigates the effect of the shooting at Sandy Hook on NICS background checks for handguns. Higher demand after the shooting should translate into more purchases in both groups of states, but the increase should be more pronounced in instant states. Figure 3 shows a massive spike in background checks for instant states right after the shooting at Sandy Hook Elementary School. There also seems to be a stronger than usual spike for the delayed states, but it is clearly smaller.

Employing our DiD estimation strategy, we estimate the differential effect of the shooting in instant and delayed states using regression analysis. Table 2 reports the results of a linear regression of the number of monthly background checks per 100,000 population on being in instant states after the shooting at Sandy Hook, as explained in section 3.1 above. Odd numbered columns show the total effect for the period after Sandy Hook, while even numbered columns split the post period into two, where the

[16] When the analysis is performed on a higher level of aggregation (e.g. the state level), we aggregate county level data.

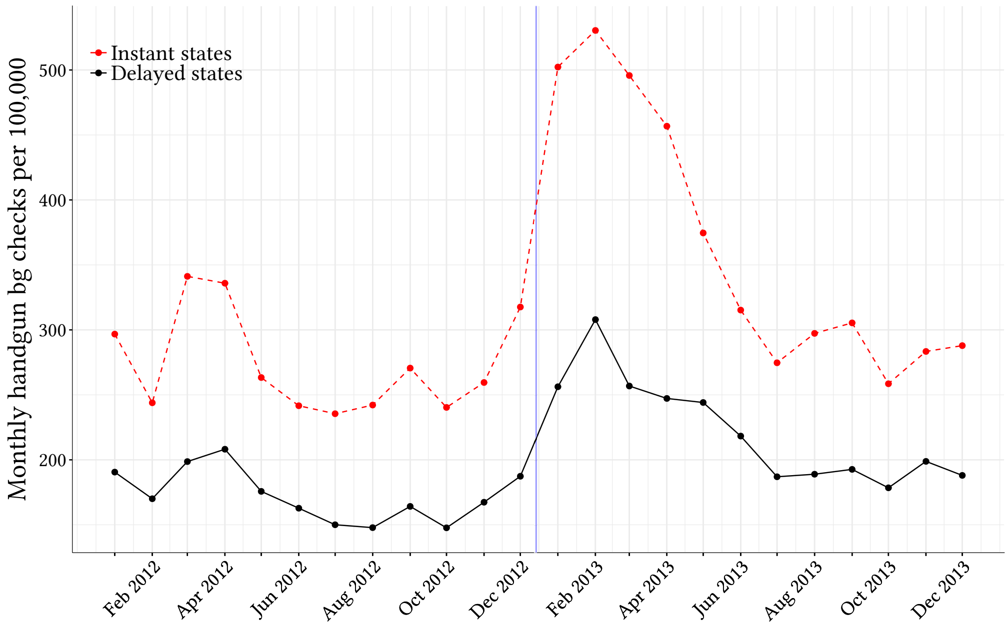

Figure 3: Background checks for handguns in delayed vs instant states

Monthly NICS background checks per 100,000 inhabitants for handguns in delayed states (black) and instant states (red) in 2012 and 2013. The blue vertical line marks the date of the shooting at Sandy Hook Elementary School.

first period (Post1) covers the first five months after the shooting, while the second period (Post2) covers the remaining months. We do this to show that the effect was particularly pronounced during the first five months, when legislative action with respect to gun laws was still pending. In all specifications, we include a time window of one year around the shooting at Sandy Hook, i.e. December 2011 to November 2013. We chose this time frame to reduce the risk of picking up trend breaks that the linear trends cannot account for (for example due to other events).

Column 1 contains a baseline specification without any controls or time trends, but including month and state fixed effects. We cluster standard errors at the state level to account for serial correlation in outcomes, and regressions are weighted by the state population to not give less densely populated states more explanatory power than high density states. The effect for handgun sale background checks is positive and significant, showing that purchases increased as a result of the shooting at Sandy Hook by more in instant states than in delayed states. Column 2 shows that this effect is strongly driven by the reaction in the first five months after the shooting. In column 3, we add our set of controls to take potential confounds into account. Included variables are % rural, %

Table 2: Handgun background checks

	Monthly handgun sale background checks per 100,000 inhabitants					
	(1)	(2)	(3)	(4)	(5)	(6)
Instant × Post	41.649**		33.692		76.852**	
	(16.839)		(21.132)		(32.777)	
Instant × Post1		107.992***		75.975***		64.822**
		(27.297)		(24.330)		(30.849)
Instant × Post2		−5.739		3.490		−15.535
		(19.770)		(25.262)		(27.380)
State FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
State FE×t	N	N	N	N	Y	Y
States	45	45	45	45	45	45
Observations	1,080	1,080	1,080	1,080	1,080	1,080
Mean DV	265.67	265.67	265.67	265.67	265.67	265.67
R^2	0.844	0.863	0.893	0.898	0.924	0.928

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

blacks, % hispanics, the log of median income and % males aged 18-24 (all interacted with month fixed effects to allow for a potentially different impact each month). In doing so, we account for the fact that gun ownership dynamics might be different in more rural versus more urban states, and might differ along dimensions such as income and education levels, for reasons not present in our estimation equation. The choice of these controls is not arbitrary, but follows the approach of Cook and Ludwig (2006); Kovandzic, Schaffer, and Kleck (2012, 2013) addressing the critique of Kleck (2015) that most prior studies have either used irrelevant covariates or too few if any. Adding controls slightly decreases the coefficient and makes it insignificant ($p = 0.11$). When splitting up the time period in column 4, however, we see that there is still a strong effect shortly after the shooting which is then counteracted by noise further away from the shooting. Column 5 then also adds a linear time trend for each state to account for potentially different pre-trends in the states. This strongly raises our estimated coefficient, and it becomes significant again, effectively showing that diverging pre-trends are not a concern in our setting. Column 6 again splits the sample, confirming the results from the previous columns. Note that each column also indicates the weighted mean of the dependent variable in the bottom section to allow for a judgment of the treatment magnitude. In the case of our specification in column 5, our estimated effect implies a 29% stronger increase in handgun sales in instant over delayed states.

While five out of six specifications show a significant effect that led to more handgun purchases in instant over delayed states following the shooting at Sandy Hook, the specification in column 5 is our preferred estimate. Including location-specific time trends ensures that differential pre-trends do not bias the results, and allowing controls to vary over time provides a very flexible approach to dealing with regional idiosyncrasies that may amplify seasonal patterns. Therefore, we continue using this specification throughout the rest of the paper, and provide robustness checks relative to this estimation strategy.

4.2 Robustness of the Findings

We have already argued that transactions at gun shows should be largely reflected in our background check data or should bias the effect downwards. To ensure that the demand for gun shows had not changed more strongly in delayed states, we investigate shifts in Google searches for the search term 'gun show'. Table 3 uses our preferred regression specification from Table 2, but now utilizes a varying time window around the shooting at Sandy Hook for our Google search expression of interest. Column 1 inspects the variation in Google searches for the first seven days before and after the shooting, which we expand to 30 days in column 2, 90 days in column 3, 365 days in column 4 and finally 730 days (= 2 years) in column 5. All estimated coefficients are positive, and especially for the longer time periods statistically significant, suggesting that if anything, shifts to the secondary market were stronger in instant than in delayed states.[17] Therefore we are confident that demand did not shift to secondary markets more strongly in delayed states. Section C.2 in the appendix additionally provides some tentative evidence that the supply of gun shows did not tilt towards delayed states either and the results qualitatively match the findings for gun show demand.

4.3 Mechanism: Transaction Costs versus Behavioral Motives

To test whether behavioral biases or transaction costs are more likely to explain our findings, we analyze if the ex ante intention to purchase a gun was affected similarly

[17] Figure 9 in the appendix depicts the evolution of Google searches graphically.

by the demand shock. We test this assumption using daily Google Trends data on searches for the word 'gun store', which has been shown to be a good predictor of firearm purchasing intentions (Scott and Varian, 2014). Table 4 repeats the regression specification from Table 4, but now uses Google searches for 'gun store' as the dependent variable. Column 1 inspects the variation in Google searches for the first seven days before and after the shooting, which we expand to 30 days in column 2, 90 days in column 3, 365 days in column 4 and finally 730 days (= 2 years) in column 5. None of the specifications show a difference in firearm purchasing interest that would reject our notion that instant and delayed states were equally affected by the demand shock.[18] In fact, all estimated coefficients are small, switch sign across specifications, and are only a negligible fraction of the mean of the dependent variable. We therefore feel confident arguing that although gun purchases differed significantly, changes in the intent to buy a gun were not significantly different across delayed and instant states.

As we explained earlier, this similar intent to buy a firearm cannot be reconciled by publicly known transaction costs alone, but rather requires some form of behavioral buyers among prospective gun buyers who, at the time of the shock, believe to purchase a gun in the future. The important assumption under which this result holds, however, is that prospective buyers are well-informed about their state's gun laws, especially about waiting periods and delayed purchasing permits. We argue that this assumption is reasonable for several reasons. First, searches for 'gun store' do not capture interest in learning about gun laws, but rather intend to locate the closest gun store. Since most people presumably know that there exist differing gun laws in their respective states, they will in most cases research the process of obtaining a gun before finding a local dealer. Second, since a transaction cost argument requires prospective buyers to be marginal after the shock, they will potentially at other points in time have thought about owning a gun already and should therefore be more likely to be familiar with gun laws. This is especially true if the shock did not extremely shift preferences for firearms. Third, since the estimated effect is not significant in any of our specifications, and also very small compared to baseline levels, the share of users not knowing about gun laws would need to be substantial. We conducted a short survey on surveymonkey.com in which we asked 119 participants about the gun laws in their state. Of the 113 respondents that

[18] Figure 8 in the appendix shows the development of Google searches between November 2011 and January 2013 graphically.

completed all questions, 88 (= 78%) correctly identified whether their state implemented waiting periods or required purchasing permits. Excluding all participants that are sure that they will not buy a gun in the next two years increases the share of respondents to correctly identify their state's gun laws only slightly to 82% (46 of 56). We therefore deem the assumption of knowledge about gun laws at the time of search for gun store locations as viable and argue that transaction costs alone are therefore not responsible for the divergence, but rather prospective buyers suffering from behavioral biases.

4.4 The Effect of Firearm Availability on Crime

Since following the shooting at Sandy Hook Elementary, there were more new guns owned in instant than in delayed states, we would like to know how this subsequently affected crime rates. Table 5 reports results from linear regressions, using our preferred specification from our first step analysis regarding changes in gun ownership. The regressions include data from December 2011 to November 2013, i.e. one year prior and after the shooting at Sandy Hook. In column 1 we regress the sum of all crimes per 100,000 inhabitants in each county on our treatment indicator (being in an instant state after the shooting at Sandy Hook). We include county and month fixed effects, and report robust standard errors clustered at the state level and to not give less densely populated counties more explanatory power, we weigh the regressions by population. The effect on overall crime is positive, but far from statistical significance. In column 2, we add our set of control variables to account for potential confounds. The coefficient grows, but is still not significant. We include a linear time trend for each county in column 3. This approach insures us against simply picking up diverging pre-trends, and the effect stays insignificant. In columns 4 through 6 we repeat the exercise but only look at violent crimes according to the UCR definition.[19] Now, our regression pick up a significant positive impact in our preferred specification, something we will look at more closely below. This column also suggest that trends in those countries were actually converging, and that only after netting out these trends do the effects of guns on crime become visible. Columns 7 through 9 show non-violent crimes and here, we do not detect any significant or sizable effect.

[19] A violent crime is defined as either being murder, non-negligent manslaughter, forcible rape, robbery, or aggravated assault. Non-violent crimes are all remaining crimes.

Table 3: Google searches for 'gun show'

	Daily Google searches for 'gun show'				
	7 days	30 days	90 days	365 days	730 days
	(1)	(2)	(3)	(4)	(5)
Instant × Post	7.163	15.700*	11.340	14.306**	7.753**
	(12.026)	(8.663)	(8.728)	(5.628)	(3.140)
State FE	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y	Y
States	50	50	50	50	50
Observations	400	1,300	2,600	5,200	10,400
Mean DV	64.78	57.67	45.53	39.02	33.93
R^2	0.843	0.772	0.772	0.755	0.738

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

Table 4: Google searches for 'gun store'

	Weekly Google searches for 'gun store'				
	7 days	30 days	90 days	365 days	730 days
	(1)	(2)	(3)	(4)	(5)
Instant × Post	−6.052	2.363	2.378	0.823	−0.834
	(8.745)	(6.724)	(6.612)	(5.038)	(2.301)
State FE	Y	Y	Y	Y	Y
Week FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y	Y
States	50	50	50	50	50
Observations	400	1,300	2,600	5,200	10,400
Mean DV	48.07	40.75	33.84	29.3	25.72
R^2	0.886	0.853	0.828	0.806	0.792

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

Table 5: Baseline: All crimes

	Monthly incidents per 100,000 inhabitants								
	Total			Violent			Nonviolent		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Instant × Post	1.368	5.071	6.952	0.302	0.484	1.340**	1.066	4.587	5.612
	(4.087)	(3.752)	(7.031)	(0.521)	(0.500)	(0.610)	(4.063)	(3.617)	(6.770)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	N	Y	Y	N	Y	Y
County FE×t	N	N	Y	N	N	Y	N	N	Y
Counties	2441	2441	2441	2441	2441	2441	2441	2441	2441
Observations	58,584	58,584	58,584	58,584	58,584	58,584	58,584	58,584	58,584
Mean DV	337.76	337.76	337.76	30.35	30.35	30.35	307.41	307.41	307.41
R^2	0.934	0.940	0.953	0.887	0.893	0.907	0.928	0.935	0.949

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

We proceed by partitioning violent crimes into their components. Table 6 shows the regressions in our preferred specification for each type of violent crime.The first column replicates column 6 from the previous table. Column 2 reports the results on murder and shows that instant states experience significantly higher murder rates after the shock in gun demand following the shooting at Sandy Hook. The estimated effect size of 0.059 additional murders per 100,000 population constitutes a 15.52% increase in murders over delayed states, an estimate close to what Luca, Malhotra, and Poliquin (2017) report. Since the population of instant states comprises approximately 178 million people, this implies that gun purchasing delays could have prevented approximately 105 murders in each month of 2013, and up to 1260 lives could have been saved from murder in instant states in only one year had such legislation been in place. Column 3 displays the results for manslaughter, also showing a significant and substantial increase. The estimated coefficient implies an increase of almost 100%, but note that manslaughter cases are extremely rare (as the crimes are classified by the police often before the prosecution decides on charges), such that in absolute terms this only translates into about 16 additional manslaughters. Columns 4 to 6 show that other types of violent crime such as rape, robberies, or aggravated assault were not significantly affected and, unlike murder and manslaughter, in a marginal order of magnitude compared to their baseline value. We thus find no evidence of a deterrence effect: None of the violent crimes experiences a significant downshift to counteract the uptick in murder rates.

Table 7 reports our findings for the most common non-violent crimes. Column 1 repeats the last column from Table 5. Column 2 reports results on simple assault, column 3 on burglary, column 4 on larceny and column 5 on vehicle theft. Of these crimes, only simple assault sees a significant decrease, but the estimated magnitude is relatively small. There are two leading explanations for this effect. First, this could be the result of a mild deterrence effect according to which some people become less aggressive towards others if the risk of encountering an armed person has increased. Second, crimes that would be classified as assaults in the absence of a gun could now have turned deadly and show up in the manslaughter or murder columns. Since the latter is limited to the increases in deadly violent crime estimated above, deterrence may certainly constitute an important explanation. We find the non-results on larceny and vehicle theft particularly reassuring, since changes in these crimes could easily be spurious correlations—finding an ex ante reason for why they should be affected by changes in gun ownership seems challenging (Lott, 2013, p. 29).

A non-effect in aggravated assaults or robberies could however still mean that a higher share of these crimes were now committed using a firearm. In Table 8, we therefore split up murders, robberies and aggravated assaults into those committed with a gun and those without. Columns 1, 4 and 7 report the overall effect on the three types of crime, while columns 2, 5 and 8 report only those in which a gun was used. Columns 3, 6 and 9 finally show the effect for these crimes when no firearm was used. For murder, it becomes apparent that the entire increase stems from murders with guns, while both for robberies, as well as aggravated assaults there is no statistically significant relationship. These findings suggest that the increased gun ownership did not lead to a significantly higher or lower share of assaults or robberies being committed with a firearm. It therefore seems that the major criminal activity these newly acquired guns were used for were additional homicides, a finding consistent with impulsive acts of violence, which gun purchasing delays aim to prevent.

Our results show that murder rates strongly increased in instant states as compared to delayed states after the gun demand shock following the shooting at Sandy Hook

Table 6: Violent crimes

	Monthly incidents per 100,000 inhabitants					
	All	Murder	Mansl'ter	Rape	Robbery	Agg. Assault
	(1)	(2)	(3)	(4)	(5)	(6)
Instant × Post	1.340**	0.059***	0.009**	0.094	0.597	0.589
	(0.610)	(0.021)	(0.004)	(0.127)	(0.370)	(0.394)
County FE	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y
Counties	2441	2441	2441	2441	2441	2441
Observations	58,584	58,584	58,584	58,584	58,584	58,584
Mean DV	30.35	0.38	0.01	2.41	8.9	18.66
R^2	0.907	0.337	0.098	0.509	0.938	0.844

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

Table 7: Non-violent crimes

	Monthly incidents per 100,000 inhabitants				
	All	Simple Assault	Burglary	Larceny	Veh.Theft
	(1)	(2)	(3)	(4)	(5)
Instant × Post	5.612	−3.462**	2.317	6.703	0.045
	(6.770)	(1.635)	(1.960)	(5.047)	(0.579)
County FE	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y
Counties	2441	2441	2441	2441	2441
Observations	58,584	58,584	58,584	58,584	58,584
Mean DV	307.41	74.63	53.99	159.19	19.59
R^2	0.949	0.941	0.865	0.917	0.923

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

Table 8: Crimes by type of weapon

	Monthly murder incidents per 100,000 inhabitants								
	Murder			Robbery			Aggr. Assault		
	All	Gun	Other	All	Gun	Other	All	Gun	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Instant × Post	0.059***	0.066***	0.002	0.597	−0.016	0.613	0.589	0.009	0.581
	(0.021)	(0.025)	(0.022)	(0.370)	(0.149)	(0.378)	(0.394)	(0.177)	(0.359)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2441	2441	2441	2441	2441	2441	2441	2441	2441
Observations	58,584	58,584	58,584	58,584	58,584	58,584	58,584	58,584	58,584
Mean DV	0.38	0.19	0.2	8.9	3.41	5.49	18.66	3.93	14.73
R^2	0.337	0.407	0.195	0.938	0.903	0.919	0.844	0.784	0.797

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

Elementary School.[20] The estimated effect size suggests that a substantial number of lives could have been saved if access to guns had been delayed in all states. To ensure that our effect on murder rates is not driven by confounding factors, the following section provides detailed robustness checks to our regressions from this section.

4.5 Robustness Checks

To address concerns that our regressions are merely picking up seasonal effects that are different in delayed and instant states respectively, we repeat our regression from Table 6, but now instead of December 14, 2012 use December 14, 2010 as the event, and include only the years 2010 and 2011 in our estimation to mimic the data range used previously.[21] This placebo regression should only be picking up an effect if there is something particular about December and adjacent months that causes our estimates to be invalid. Table 9 reports the estimates for changes in several violent crimes after December 14, 2010. None of the crimes seems to be significantly affected, and importantly, neither is murder nor manslaughter. The coefficients in fact are much

[20]Table 13 in the appendix presents the results for murder, manslaughter and aggravated assault if all counties (even if the agencies cover less than 50% of the population) are included. The results are qualitatively similar.

[21]We chose 2010, because 2010 and 2011 were the latest years that were free of large mass shootings with more than 20 victims.

Table 9: Placebo regressions of violent crimes

	Monthly incidents per 100,000 inhabitants											
	All		Murder		Mansl'ter		Rape		Robbery		Aggr. Assault	
	2012	2010	2012	2010	2012	2010	2012	2010	2012	2010	2012	2010
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Instant × Post	1.340**	0.841	0.059***	−0.006	0.009**	−0.001	0.094	0.110	0.597	0.681	0.589	0.057
	(0.610)	(0.931)	(0.021)	(0.038)	(0.004)	(0.006)	(0.127)	(0.069)	(0.370)	(0.452)	(0.394)	(0.633)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2441	2327	2441	2327	2441	2327	2441	2327	2441	2327	2441	2327
Observations	58,584	55,848	58,584	55,848	58,584	55,848	58,584	55,848	58,584	55,848	58,584	55,848
Mean DV	30.35	31.64	0.38	0.38	0.01	0.01	2.41	2.28	8.9	9.34	18.66	19.64
R^2	0.907	0.914	0.337	0.352	0.098	0.104	0.509	0.435	0.938	0.939	0.844	0.862

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

closer to zero, suggesting that the previously uncovered effect can be attributed to the treatment rather than seasonal variation across groups of states.

Even if the effect is not purely seasonal, it could also be caused by unrelated events taking place in our sample period. Additionally one could be concerned that the effect is a construct of the choice of the time window around the event. Therefore, Figure 4 shows how the coefficient estimate on murder changes if we include data further away from the shooting. Initially, with only up to five months post and prior to the shooting, the effect is insignificant, but adding more data moves the coefficient away from zero and drastically decreases standard errors. Increasing the time window around the shooting far enough leads to a significantly positive estimate, starting at six months post and prior the shooting. The effect stays significant throughout, but peaks after the inclusion of seven months. We are therefore confident that our result is not simply an outlier driven by a favorable choice of the time window. Additionally, it seems unlikely that events further than seven months before or after the shooting initiated the observed effect, effectively excluding the 2013 US Federal Government shutdown, the 2013 DC Navy Yard shooting, but not ruling out the November 2012 re-election of President Obama and the July 2012 shooting in a movie theater near Denver, Colorado, which constitute the most notable events in the United States in 2012 and 2013.

Both events, however, would presumably contribute to the effect in a similar direction. As pointed out earlier, mass shootings during the Obama presidency have throughout increased gun sales. This would mean that our findings constitute the lower bounds of the effects, as the 2012 Denver shooting would have differentially affected gun sales already, making the effects due to the Sandy Hook shooting look smaller than they actually are. Similarly, the November 2012 election increased gun sales as citizens feared tougher gun legislation from a Democratic president (CNN Money, 2012).

There could also be the concern that the shooting at Sandy Hook not only influenced firearm demand, but also had a direct impact on crime rates. When comparing the differences in crime rates across delayed and instant states, we implicitly assume that they were driven by the different gun ownership dynamics and not directly changed by the event. It could however be conceivable that a violent act such as a mass shooting influenced attitudes towards violence differently in instant and delayed states, as the states differ across many dimensions. One way to address this concern is to find a

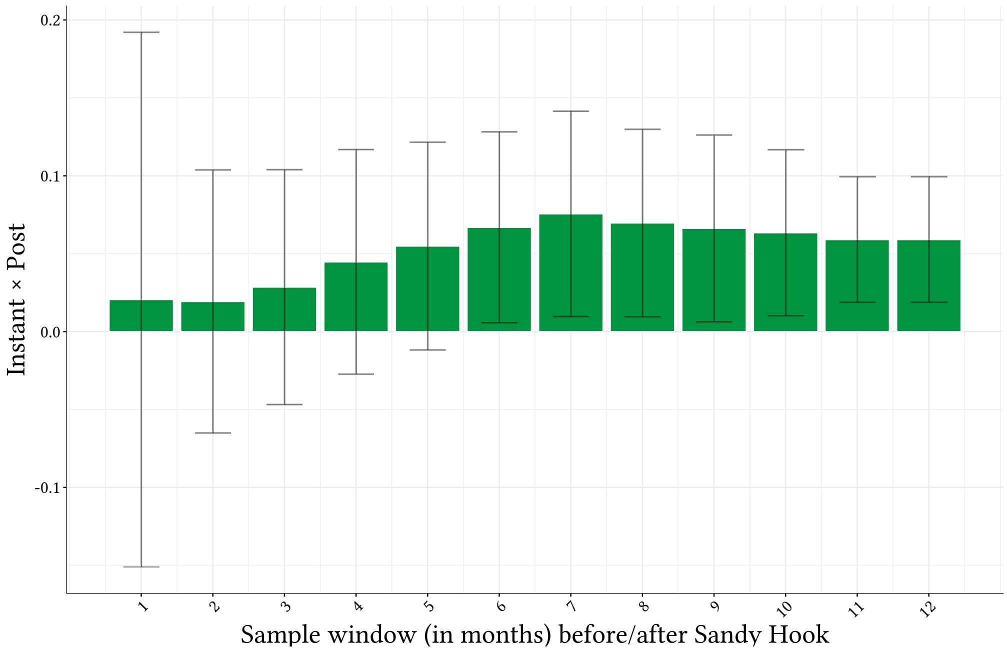

Figure 4: Time window on murder coefficient

Coefficients on murder including an increasing number of months before and after the shooting at Sandy Hook in the regression and corresponding 90% confidence intervals. 1 means the month post and prior to the shooting are included (=2 months in total), 2 means that 2 months prior and post the shooting are included (=4 months in total), etc.

non-violence related shock to firearm demand. One example is the 2012 Presidential election. Former Governor Mitt Romney took a more a liberal position on gun rights than President Obama, which earned him the endorsement of the NRA. After the election, gun sales increased strongly (CNN Money, 2012).

Unfortunately, the 2012 Presidential election happened shortly before the shooting at Sandy Hook Elementary School in November 2012. Separating the two events therefore seems impossible, such that we can only determine if the election contributed to the overall observed effect. Figure 5 reports the coefficients and corresponding 90% confidence intervals of the murder regression in Table 6 when the treatment date is shifted to an earlier or later month. Clearly, the effect is strongly positive and significant at date 0, i.e. right after the shooting at Sandy Hook took place. Shifting the date to the future makes the effect less strong and it becomes insignificant. Reassuringly, the coefficient also falls monotonically. Post-dating the event even further then makes the coefficient negative as should be expected if the actual response to the shooting predates the date at which we define our treatment. Pre-dating the treatment event by one period (to include the election in November 2012) however slightly increases

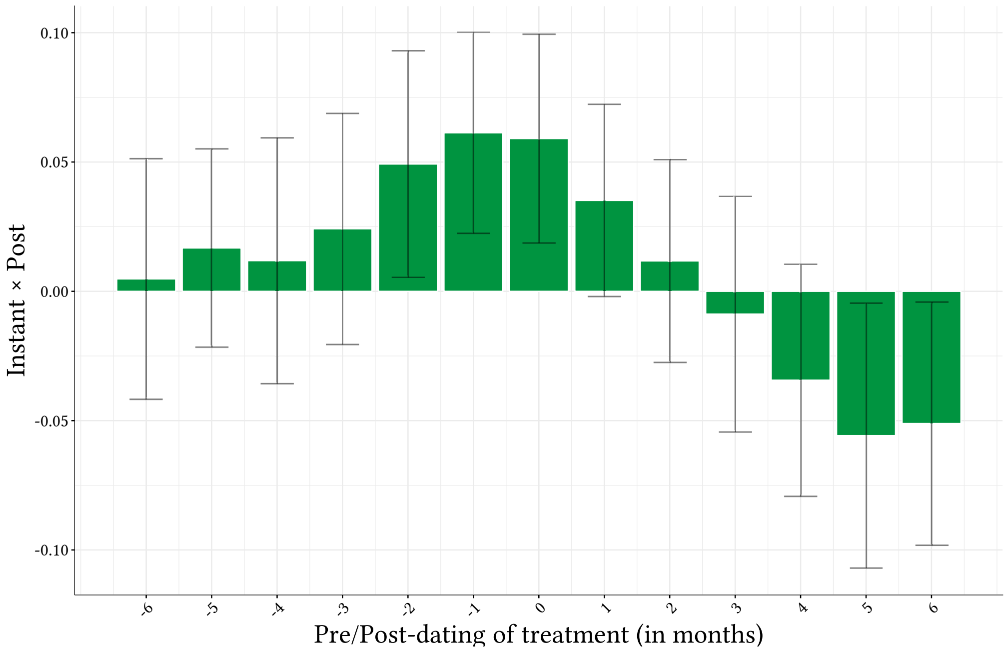

Figure 5: Timing of treatment onset

Coefficients on murder when temporally shifting the onset of the treatment and corresponding 90% confidence intervals. 0 denotes the shooting at Sandy Hook. Positive numbers denote months after the shooting, while negative numbers denote months prior to the shooting.

the effect and it becomes more significant. This suggests that the 2012 election indeed positively contributed to the effect. Pre-dating the treatment further than two months also delivers insignificant effects which is reassuring as the effect does not result from other events than the shooting and the election.[22] We are therefore confident that our estimated effect on murder rates resulting from the shooting at Sandy Hook is indeed due to differences in firearm ownership rates across instant and delayed states.[23]

Finally, we address the concern of outliers. In particular, one may be concerned that falsely categorizing states as either instant or delayed, or extreme patterns in homicide within a specific state before and after Sandy Hook may be driving our baseline results. We therefore perform a series of regressions where single states are omitted from the regression. If the coefficients become very unstable upon exclusion of single states, we should be very cautious with interpreting the effect of delays to be a general finding across the United States. Figure 6 reports the results from our murder regression with 90% confidence intervals, removing one state at a time for all states in our sample. The

[22] Figure 10 in the appendix recreates the same graph for our handgun background checks with qualitatively very similar results.

[23] Section C.3 in the appendix provides directionally similar, yet insignificant results for the 2008 election.

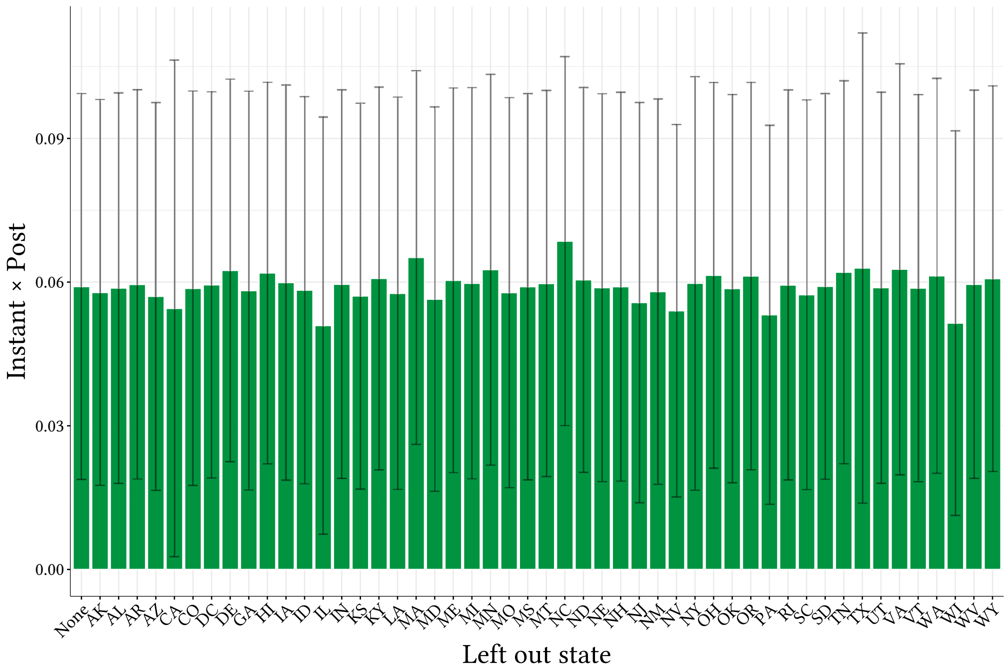

Figure 6: Coefficients on murder leaving out states

Coefficients on murder removing a single state (denoted on the x-axis) from the sample and corresponding 90% confidence intervals. Base denotes the inclusion of all states.

first bar shows the estimated effect for the full sample, the second bar does not include Alaska, the third bar shows the full sample minus Arkansas, and so forth. Across all specifications, the coefficient is of similar magnitude. In fact, it never falls below .04 and rarely exceeds .06 and overall seems to exhibit relatively little variance. The confidence bands further show that the effect is significant across all specifications.

Extending the Sample

Since the UCR data covered only around 2,500 of the approximately 3,100 US counties (and county-equivalents), we would like to see if we find similarly strong results in the remaining counties. The NVSS data provides us with the universe of homicides (not distinguishing between murder and manslaughter unfortunately) through death certificates.

We use this data in table 10. As a first sanity check, and in order to rule out that our previous results were driven by misreporting through the police, we redo our UCR analysis with respect to murder rates using the NVSS data. Columns 1 through 3 repeat the findings from earlier that the increase in overall murders stems solely from murders in which firearms were used. Columns 4 through 6 show the same sample of counties

Table 10: Extending the sample

	Monthly murders per 100,000 inhabitants								
	UCR			NVSS (UCR sample)			NVSS (full sample)		
	All	Gun	Other	All	Gun	Other	All	Gun	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Instant × Post	0.059***	0.066***	0.002	0.057***	0.059***	−0.002	0.025	0.042**	−0.017
	(0.021)	(0.025)	(0.022)	(0.021)	(0.021)	(0.018)	(0.028)	(0.019)	(0.016)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2441	2441	2441	2441	2441	2441	3133	3133	3133
Observations	58,584	58,584	58,584	58,584	58,584	58,584	75,192	75,192	75,192
Mean DV	0.38	0.19	0.2	0.44	0.29	0.15	0.45	0.3	0.15
R^2	0.337	0.407	0.195	0.389	0.421	0.138	0.365	0.392	0.132

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

but using the NVSS instead the UCR data. Reassuringly, the results are very similar: The increase in murder can be completely traced to an increase in gun-related murders. Columns 7 through 9 then extend the sample to all available counties and county-equivalents, 3133 in total. While column 8 shows that the effect on gun-related murders is similarly strong when adding the remaining counties, non-gun related murders induce a noise (with a negative coefficient) that drags down the estimate on total murders such that it becomes insignificant. The estimated coefficient implies an increase of about 45 murders in each month of 2013.

While this data suggests that the effect may be smaller than in the counties present in the UCR data, there are two caveats we would like the reader to note. First, the NVSS data records the county of residence at death which may not coincide with the place of death. Since this does not seem to affect the results in the 2,500 UCR counties, we deem it unlikely that this can explain the smaller effect in the 700 additional counties. Second, the counties not present in the UCR data have been excluded due to misreporting. Since death certificates after homicides are often issued by a medical examiner or coroner that may be integrated into the respective police force, there may be scope for misreporting in the NVSS data as well, although no such case is known to us.

5 Conclusion

With the political debate on gun control gaining traction amid several recent mass shootings, understanding the consequences of legislation limiting access to firearms is imperative. The existing literature has delivered contradicting results and there remains doubt about the identification strategies used in some of the previous contributions. This paper contributes to this important topic by presenting well-identified effects on crime rates from an exogenous country-wide gun demand shock that led to differential gun purchases depending on whether states had implemented mechanisms to delay purchases or not.

Our results show that in states without any firearms purchasing delays, gun sales rose stronger following the gun demand shock resulting from the shooting at Sandy Hook Elementary School in Newtown, Connecticut. The effect is strong and robust to a variety of controls and alternative specifications and does not seem to be caused by pre-existing time trends. Additional evidence suggests that the effect might be driven by behavioral gun buyers changing their plans rather than transaction costs, as results show that the intention to acquire firearms right after the shooting was not significantly different across groups of states, but large differences appear in actual gun sales. Our analysis also addresses transactions on secondary markets and provides evidence that sales at gun shows are unlikely to confound our results.

We then use these findings to explore how crime rates changed. While all other crime categories remain unaffected, murder rates increase significantly in states where gun purchases reacted more strongly. Several additional regressions and analyses address possible robustness and endogeneity issues. Our findings suggest that in each month of 2013 alone, between 45 and 102 murders could have been prevented if all states had implemented delays for gun purchases. The null finding regarding many other crime categories suggests that armed citizens might not provide a strong deterrent effect. Including the 2012 United States Presidential election as an additional event that drove up gun sales makes the effect stronger, suggesting that our results are not due to a direct effect of mass shootings on crime rates, but rather through increased firearm ownership rates.

At this point we would like to emphasize that the aim of this paper is not to speak in favor of the abolition of Second Amendment rights. The topic of allowing citizens to own guns is obviously very complex and has many dimensions that need to be taken into consideration, such as whether armed citizens can provide protection against exploitation by the government, or whether stripping legal gun owners from their rights will in the short run lead to more crime as potential victims become defenseless. Additionally, our results do not address the issue of widespread firearm access of criminals or structures of organized crime which are so pervasive in some larger US cities and that have their own roots and causes. Our study merely provides evidence on two aspects of the gun control debate, namely that more widespread gun ownership positively contributes to murder rates and that delaying gun purchases can help to prevent murders. These findings should therefore be taken into account when deciding about the extent and the means of gun control legislation.

Finally, we see our study as a good starting point to investigate additional, related issues. First, additional direct evidence on the circumstances under which gun ownership leads to increased violent crime is needed. While we conclude that only behavioral biases are consistent with our findings, it would be interesting to see which biases are actually responsible for our results. Second, it would be interesting to determine the exact relationship of victims and offenders. Third, the effect of the Sandy Hook shooting on suicides remains completely unknown. Given the large correlational evidence on the relationship of gun prevalence and suicides, knowing whether guns not particularly bought for the purpose of killing oneself actually impact suicide rates. We leave these issues for future research as they constitute different research questions or cannot yet be answered given the available data.

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A Figures

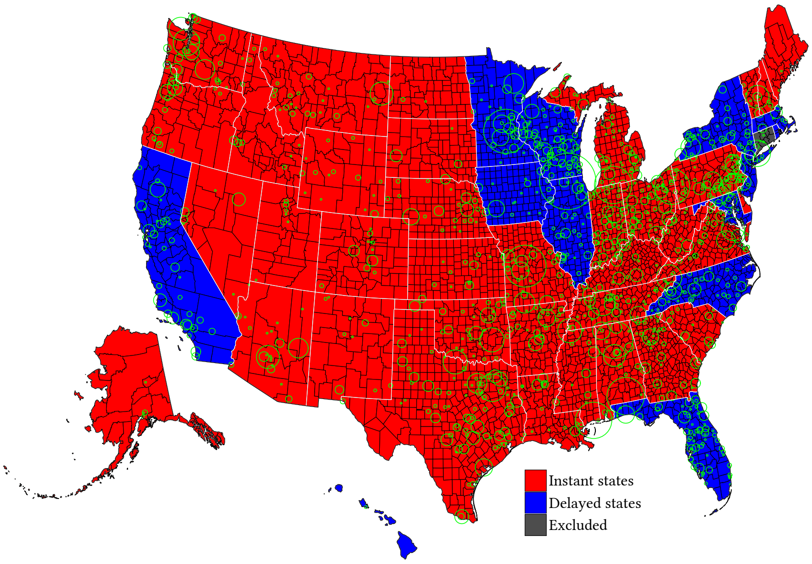

Figure 7: Locations of gun shows

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

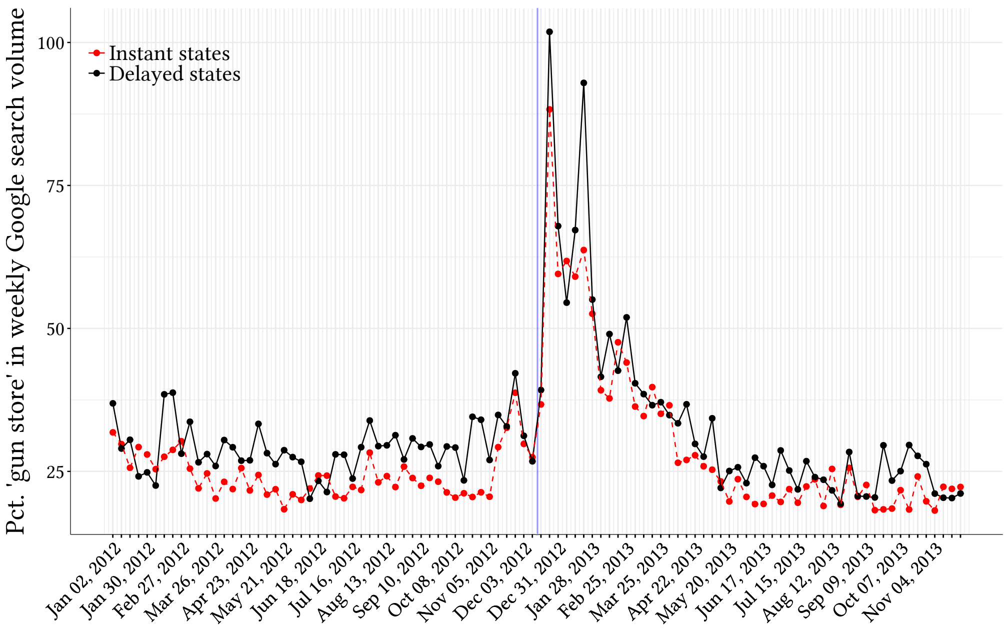

Figure 8: Google searches for 'gun store' in delayed vs instant states

Weekly averages of daily normalized Google searches for the expression 'gun store' in delayed states (black) and instant states (red) between January 2012 and December 2013. The blue vertical line marks the date of the shooting at Sandy Hook Elementary School.

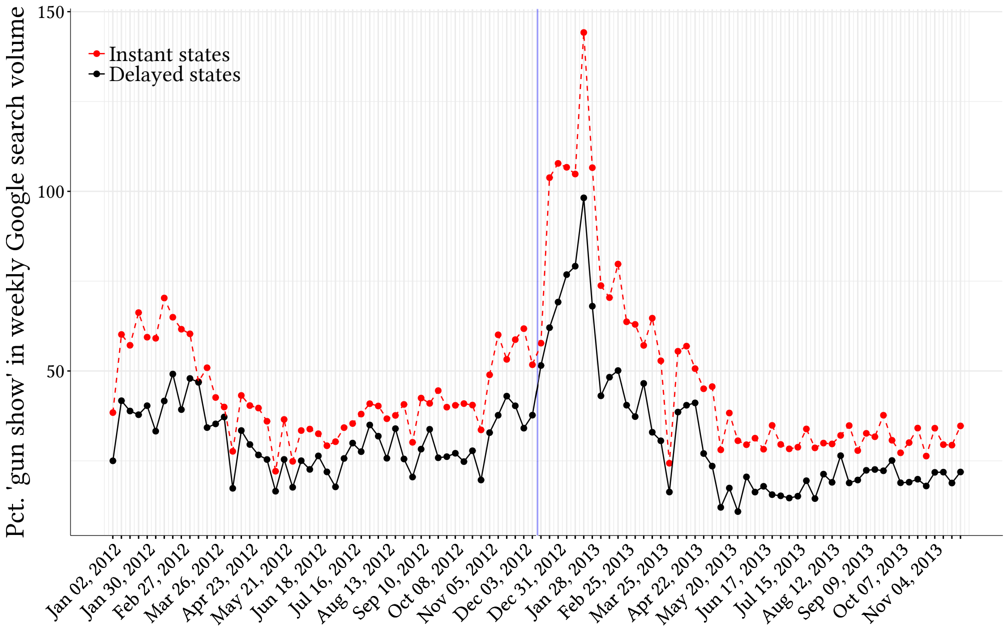

Figure 9: Google searches for 'gun show' in delayed vs instant states

Weekly averages of daily normalized Google searches for the expression 'gun show' in delayed states (black) and instant states (red) between January 2012 and December 2013. The blue vertical line marks the date of the shooting at Sandy Hook Elementary School.

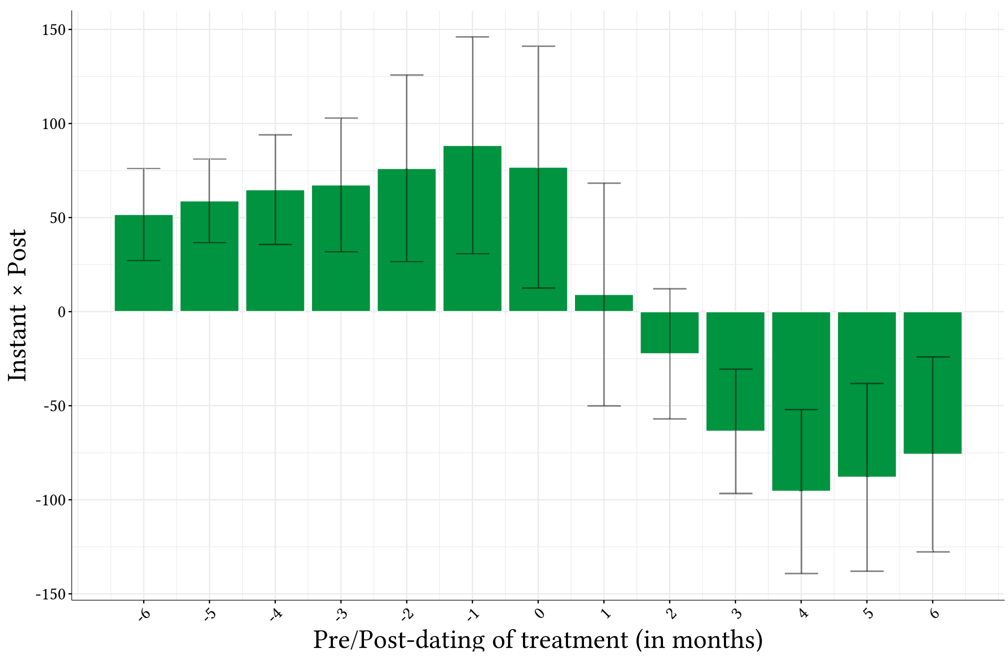

Figure 10: Timing of treatment onset

Coefficients on handgun sale background checks when temporally shifting the onset of the treatment and corresponding 90% confidence intervals. 0 denotes the shooting at Sandy Hook. Positive numbers denote months after the shooting, while negative numbers denote months prior to the shooting.

B Tables

Table 11: Handgun background checks (including CT)

	Monthly handgun sale background checks per 100,000 inhabitants			
	Without CT		With CT	
	(1)	(2)	(3)	(4)
Instant × Post	76.852**		76.822**	
	(32.777)		(32.625)	
Instant × Post1		76.627**		76.597**
		(32.770)		(32.618)
Instant × Post2		−18.744		−18.861
		(28.994)		(28.639)
State FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y
States	45	45	46	46
Observations	1,080	1,080	1,104	1,104
Mean DV	265.67	265.67	268.4	268.4
R^2	0.924	0.930	0.925	0.931

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

C Additional Analyses

C.1 Theoretical Motivation: Other Explanations

This section intends to present other theoretical explanations for our findings, and lay out the necessary assumptions. One candidate would be inattention to transaction costs. If consumers do not pay attention to the transaction costs when making the Google search for ‘gun store’, but only realize it when they actually buy the gun, we would observe a similar effect. Note however that once consumers arrive at a gun store to make a purchase, the transaction costs of the first trip are sunk and therefore those that intend to buy a gun should also do so. Additionally, when consumers search for ‘gun store’ in order to buy a gun, they presumably have spent time to deliberate the purchasing decision and therefore also thought about potential transaction costs. Most importantly, to generate a difference in gun sales, we need to impose an assumption that the inattention resolves between forming the intention to buying a gun and actually

Table 12: Violent crimes (including CT)

	Monthly incidents per 100,000 inhabitants											
	Without CT						With CT					
	All	Murder	Mansl'ter	Rape	Robbery	Agg. Assault	All	Murder	Mansl'ter	Rape	Robbery	Agg. Assault
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Instant × Post	1.318**	0.062***	0.009*	0.079	0.660*	0.517	1.259*	0.063***	0.009*	0.010	0.667*	0.519
	(0.665)	(0.023)	(0.005)	(0.133)	(0.395)	(0.419)	(0.656)	(0.022)	(0.005)	(0.155)	(0.386)	(0.414)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2248	2248	2248	2248	2248	2248	2254	2254	2254	2254	2254	2254
Observations	53,952	53,952	53,952	53,952	53,952	53,952	54,096	54,096	54,096	54,096	54,096	54,096
Mean DV	30.72	0.39	0.01	2.42	9.01	18.9	30.61	0.38	0.01	2.42	9	18.81
R^2	0.911	0.352	0.102	0.471	0.940	0.854	0.910	0.352	0.102	0.458	0.940	0.854

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

Table 13: Violent crimes (include low-coverage counties)

	Monthly incidents per 100,000 inhabitants											
	Data coverage >50% of 2010 population (baseline)						No coverage restriction					
	All	Murder	Mansl'ter	Rape	Robbery	Agg. Assault	All	Murder	Mansl'ter	Rape	Robbery	Agg. Assault
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Instant × Post	1.318**	0.062***	0.009*	0.079	0.660*	0.517	1.134	0.051**	0.008**	0.101	0.585	0.397
	(0.665)	(0.023)	(0.005)	(0.133)	(0.395)	(0.419)	(0.709)	(0.024)	(0.004)	(0.135)	(0.372)	(0.445)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2248	2248	2248	2248	2248	2248	2742	2742	2742	2742	2742	2742
Observations	53,952	53,952	53,952	53,952	53,952	53,952	65,808	65,808	65,808	65,808	65,808	65,808
Mean DV	30.72	0.39	0.01	2.42	9.01	18.9	30.27	0.38	0.01	2.42	8.72	18.74
R^2	0.911	0.352	0.102	0.471	0.940	0.854	0.878	0.283	0.088	0.460	0.918	0.806

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

going to the gun store. It also seems unclear where such an inattention should come from, given that we have established in the main text that consumers on average very well know the important parts of the gun laws in their state. We therefore deem this explanation to not be very realistic.

(Full) projection bias (Loewenstein, O'Donoghue, and Rabin, 2003) predicts that consumers' current taste is projected to the future, where it may not be accurate anymore. As a policy implication, offering return periods allows these consumers to make better decisions, as the purchase can be reversed when the realization of tastes is substantially different to the anticipated taste. Waiting periods for firearm purchases however will not interact with projection bias. Consumers interested in buying a gun (based on their projected utility) will do so, no matter whether they have to wait for their gun or not. Since the purchasing decision is made when conducting the background check and filing the paperwork, and not when picking up the firearm, there is little scope for projection bias that consumers are not aware of influencing the decision. Only if the actual taste realizes after the consumer searched about a gun store, but before buying the gun, can projection bias produce a wedge between intentions to buy and actual gun sales. Similarly, transient emotional reactions (such as impulsiveness) can only be expected to drive the difference if the emotional reaction ends after the decision to buy a gun is made, but before the actual purchase is done. Without this assumption, this would only lead to differential pick-up rates of purchased guns, but not necessarily to differences in purchases per se, as the purchase is made when filling out the paperwork and not when picking up the gun.

Imas, Kuhn, and Mironova (2016) describes how waiting periods can force consumers to make more deliberative choices (and less follow heuristics). The idea is that by separating the knowledge about the choice set by the actual choice, consumers have to think more carefully about their decision and thus become more patient. Note, however, that waiting periods for firearms do not impose a temporal distance between knowing about the choice set and *buying* a gun, but only between knowing about the choice set and *picking up* the gun. Therefore we should not expect to see an interaction of waiting periods with the decision to purchase in our setting.

Another standard economic theory argument that could be made is that consumers have a stochastic realization of their utility of buying a gun or update their beliefs about needing a firearm. After the shock, the utility is very high (or the belief to need a gun is high) such that the consumers intend to buy the firearm. After some time (when the purchase is conducted), the utility realizes to be lower (the belief to need a gun has decayed) and so consumers do not buy. Note, however, that this can only explain the pattern in the data if we are willing to assume that the utility realization is substantially different in instant versus delayed states (or the belief updating is different in instant over delayed states in a systematic way). Otherwise, these arguments do not interact with waiting periods, as they should only influence the pick-up rates of guns. We therefore deem these assumptions to be too restrictive.

C.2 Gun Shows: Extensive Margins

One could be concerned that lower demand for firearms in delayed 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), many states have regulations for sales at gun shows and the number of guns acquired at gun shows is estimated to be comparatively small. Additionally, we have demonstrated that the demand for gun shows did not tilt towards delayed states.

To show that also the supply of gun shows did not increase stronger in delayed than in instant states, Figure 11 shows the evolution of gun shows in both types of states graphically.

Table 14 reports regression results of the number of monthly gun shows per 100,000 inhabitants on the joint effect of being in an instant state after the Sandy Hook shooting, again utilizing our set of controls from previous regressions interacted with month fixed effects. Standard errors are clustered on the state level, we employ month and county fixed effects and a time trend that is allowed to be different in each county. Column 1 reports the results for the years immediately before and after the shooting at Sandy Hook. We expand the time dimension in column 2 by the years 2011 and 2014 respectively, and column 3 looks at all years of the Obama administration. Overall, there does not seem to be an effect of the shooting on the number of gun shows in delayed vs instant states, as the coefficient is very close to zero and not statistically significant at any conventional level. Only column 3 shows a significant effect, but it suggests that

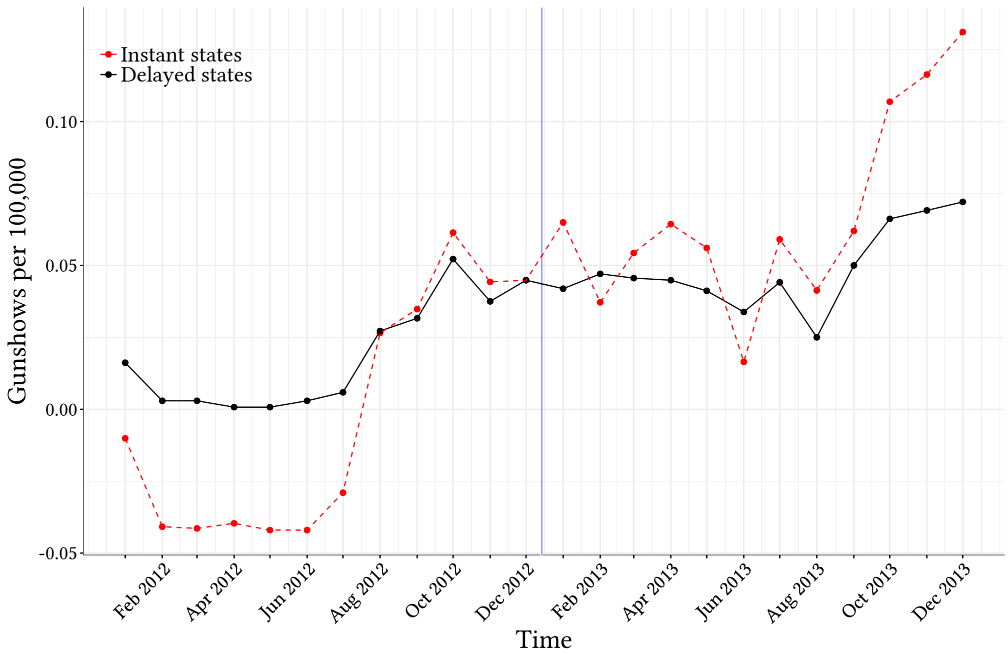

Figure 11: Gun shows in delayed vs instant states

Monthly number of gun shows per 100,000 inhabitants in delayed states (black) and instant states (red) between January 2012 and December 2013. The blue vertical line marks the date of the shooting at Sandy Hook Elementary School.

Table 14: Regressions of gun shows before and after Sandy Hook

	Monthly gun shows per 100,000 inhabitants			
	2012-2013		2011-2014	
	(1)	(2)	(3)	(4)
Instant × Post	−0.010		0.010	
	(0.008)		(0.008)	
Instant × Post1		−0.012		0.014*
		(0.008)		(0.008)
Instant × Post2		−0.026*		0.003
		(0.015)		(0.012)
County FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y
Counties	3133	3133	3133	3133
Observations	75,192	75,192	150,384	150,384
Mean DV	0.06	0.06	0.05	0.05
R^2	0.531	0.531	0.400	0.400

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

Table 15: Handgun background checks (2008 election)

	Monthly handgun sale background checks per 100,000 inhabitants			
	2012		2008	
	(1)	(2)	(3)	(4)
Instant × Post	76.852**		15.052	
	(32.777)		(12.225)	
Instant × Post1		64.822**		14.964
		(30.848)		(12.212)
Instant × Post2		−15.535		−22.095**
		(27.380)		(9.990)
State FE	Y	Y	Y	Y
Month FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
State FE×t	Y	Y	Y	Y
States	45	45	45	45
Observations	1,080	1,080	1,080	1,080
Mean DV	265.67	265.67	151.68	151.68
R^2	0.924	0.928	0.870	0.873

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

the supply of gun shows increased stronger in instant states after the shooting. Thus we can conclude that the supply of gun shows in delayed states very likely did not increase over instant states.

C.3 The 2008 Presidential Election

As an additional robustness check, and because we cannot separate the 2012 Presidential election from the shooting at Sandy Hook, we can re-run our regression of murders using the 2008 Presidential Election as the event that triggered another gun demand shift, without being violent in nature. According to anecdotal evidence, the 2008 election similarly drove up gun sales (CNN, 2008) and this is also confirmed by recent research (Depetris-Chauvin, 2015). We therefore repeat our regression from table 2 to see if gun sales were also differentially affected by the 2008 election. The results can be found in Table 15.

Clearly, the results for both long- and handguns are significant across all our specifications and go in the same direction as in our main specification. Table 16 then repeats our regression of violent crimes from Table 6, but now looks at the years of 2008

Table 16: Violent crimes after 2008 Presidential election

	Monthly incidents per 100,000 inhabitants											
	All		Murder		Mansl'ter		Rape		Robbery		Aggr. Assault	
	2012	2008	2012	2008	2012	2008	2012	2008	2012	2008	2012	2008
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Instant × Post	1.340**	2.165*	0.059***	0.060	0.009**	−0.001	0.094	−0.204	0.597	1.345**	0.589	0.964
	(0.610)	(1.272)	(0.021)	(0.040)	(0.004)	(0.005)	(0.127)	(0.180)	(0.370)	(0.536)	(0.394)	(0.794)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
County FE×t	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Counties	2441	2195	2441	2195	2441	2195	2441	2195	2441	2195	2441	2195
Observations	58,584	52,680	58,584	52,680	58,584	52,680	58,584	52,680	58,584	52,680	58,584	52,680
Mean DV	30.35	35.96	0.38	0.43	0.01	0.02	2.41	2.37	8.9	11.43	18.66	21.72
R^2	0.907	0.928	0.337	0.384	0.098	0.107	0.509	0.444	0.938	0.950	0.844	0.875

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

and 2009, and takes the 2008 Presidential election as the event date.[24] We observe an effect on murder rates that goes in the same direction as in Table 6 and is of similar magnitude, but that is not statistically significant due to larger standard errors. A Z-test (Clogg, Petkova, and Haritou, 1995) comparing our murder estimate in Table 6 with our estimate in Table 16 cannot find statistically significant differences between the two estimates (p = .9101). We view this exercise as suggestive that the 2008 Presidential election might have had a similar effect, but that we are unable to correctly identify the effect.

[24] For these years, we use the data by (USDOJ: FBI, 2010; USDOJ: FBI, 2011).