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Impulse Purchases, Gun Ownership, and Homicides: Evidence from a Firearm Demand Shock[1]

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Abstract: Do firearm purchase delay laws reduce aggregate homicide levels? Using variation from a 6-month countrywide gun demand shock in 2012/2013, we show that U.S. states with legislation preventing immediate handgun purchases experienced smaller increases in handgun sales. Our findings indicate that this is likely driven by comparatively lower purchases among impulsive consumers. We then demonstrate that states with purchase delays also witnessed comparatively 2% lower homicide rates during the

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same period. Further evidence shows that lower handgun sales coincided primarily with fewer impulsive assaults and points towards reduced acts of domestic violence.

JEL codes: K42, H76, H10, K14 **Keywords:** Guns, homicides, gun control

1 Introduction

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

One such group of policy measures, targeted explicitly at preventing impulsive acts of gun violence, are firearm purchase delay laws. These measures, by now in place in 15 U.S. states, create a temporal distance between the decision to buy a gun and its eventual receipt. Delays can last from 2 days up to 6 months and occur through mandatory waiting periods or bureaucratic hurdles associated with obtaining purchasing permits. Both measures provide gun buyers with a "cooling-off period" during which those with short-lived suicidal or homicidal intentions may reconsider their planned actions (Cook, 1978; Andrés and Hempstead, 2011). Since delay laws should also keep impulsive consumers without violent intentions from buying guns, they offer a unique avenue to investigate whether and how prevented firearm purchases by such individuals translate into reduced

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gun violence. However, such analysis would require a reasonably large shift in impulse purchases unrelated to local crime levels.

In this paper, we exploit one of the largest aggregate shocks to U.S. firearm demand to study the effects of handgun purchase delay laws. In a first step, we show that the existence of purchase delays led to a relative reduction in handgun sales during the six months after the 2012 Presidential election and the shooting at Sandy Hook Elementary School. During this period, fear of more restrictive gun control legislation and higher perceived need for self-defense capabilities led to record sales of firearms across the entire United States (Vox, 2016; CNBC, 2012). We use a difference in differences (DiD) framework, comparing handgun sale background checks (BGCs) in states with handgun purchase delays to states without such delays during the six-month window of increased firearm demand. Our baseline results indicate that states with purchase delay laws witnessed a 7-8% relative decrease in handgun sales. Differences in gun popularity and other types of firearm legislation cannot explain these results.

Next, we present evidence suggesting that lower purchasing levels were indeed more likely driven by impulsive buyers. We start by analyzing Google search data and show that delay laws did not lead to comparatively lower public interest in buying firearms during the demand shock. Handgun purchase delay laws thus did not seem to affect intentions to buy firearms, but only whether consumers' interest translated into actual purchases. Using state variation in delay lengths, we also do not observe a relationship between our estimated effect size and delay length. For deliberate and exponentially discounting consumers, these should have been positively correlated since delays smoothly reduce the discounted net present value of owning a gun. This discontinuous impact of delay lengths on purchases lends further credibility to the presence of impulsive consumers.

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In the second part of our analysis, we investigate the effect of delay laws on homicides. Using the same DiD framework, we find that counties in states with purchasing delays experienced a relative 2% decrease in overall homicide rates during the demand spike, which is entirely driven by homicides involving handguns. Our baseline estimate implies that about 200 lives could have been saved in the six-month period alone if handgun purchase delays had been in place in all U.S. states. An extensive set of robustness checks shows that our results are specific to the period of the demand hike and not driven by single states or the sample choice. Looking into the characteristics of the additional homicides in states without handgun purchase delays, we find evidence in line with the notion that gun ownership among impulsive buyers is associated with crimes of passion. [4] For female victims, the evidence points towards instances of domestic violence, as the majority of additional female homicides occurred inside the victim's home and arose from an argument. The affected killings of males occurred mainly outside of their homes but were similarly strongly related to arguments.

This study is related to three important streams of research. First, we add to the literature investigating the impact of firearm legislation, and in particular purchase delays, on crime rates. Previous studies found either decreases (Rudolph et al., 2015; Edwards et al., 2018; Luca, Malhotra, and Poliquin, 2017) or zero effects (Ludwig and Cook, 2000) on violent crime or homicides. As the adoption of firearm purchase delay laws may not be exogenous and law changes can be anticipated by prospective gun buyers, our paper substantially advances this literature by providing novel and credible identification through exploiting a sudden and unanticipated demand shock in conjunction with pre-

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

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existing delay laws.[5] We also provide suggestive evidence that our empirical setup mainly picks up the behavior of impulsive consumers without violent intentions and offers insights into the types of homicides prevented through purchase delays.

Second, we contribute to the extant literature in economics, criminology, and public health, studying the impact of firearm ownership on violent crime. The majority of studies find a positive relationship (see, e.g., Cook and Ludwig, 2006; Duggan, 2001; Miller, Azrael, and Hemenway, 2002; Miller, Hemenway, and Azrael, 2007; Siegel, Ross, and King, 2013). Some studies, however, also report no effect (Duggan, Hjalmarsson, and Jacob, 2011; Moody and Marvell, 2005; Kovandzic, Schaffer, and Kleck, 2013; Lang, 2016). A recent paper by Levine and McKnight (2017) shows with a different identification strategy that elevated gun exposure after the Sandy Hook shooting translated into higher rates of firearm-related accidents.[6] We confirm the positive link between gun ownership and homicides found in previous studies but are the first to look specifically into firearm homicide characteristics and highlight the role of impulsiveness.

Third, our evaluation of gun purchase delay laws contributes to the growing literature analyzing how policies can mitigate the consequences of behavioral biases (overviews are provided in Chetty, 2015; Bernheim and Taubinsky, 2018). To the best of our knowledge, we are the first to study impulsive behavior in the context of gun ownership. Few other studies at the intersection between behavioral economics and economics of crime have

[5]The identification strategy of overlaying cross-sectional variation in pre-existing characteristics with a common time-series shock has also been applied in other work (see, e.g., Nunn and Qian (2011).

[6]While gun-related accidents are not at the heart of our paper, supplementary results reported in the Appendix based on our own identification strategy cannot replicate those findings. Our main results suggest that the primary detrimental effect of increased gun ownership after the Sandy Hook shooting was an increase in gun-related *homicides*.

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also linked impulsiveness to criminal activity and acts of violence (Dahl and DellaVigna, 2009; Card and Dahl, 2011; Heller et al., 2017). We advance this literature by providing the first study to establish a link between firearm availability and the fatal consequences of impulsive behavior.

2 Background

2.1 Purchase Delay Laws in the United States

The Second Amendment to the United States Constitution protects the fundamental right of citizens to keep and bear arms. Federal, state, and local governments, however, have enacted laws making it harder and more cumbersome for citizens to acquire firearms. On the federal level, two crucial pieces of legislation are the Gun Control Act of 1968 and the Brady Handgun Violence Prevention Act. The Gun Control Act requires all professional gun dealers to 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 of November 1993 mandated BGCs for all gun purchases through FFL dealers and imposed a five-day waiting period to conduct these checks. Upon successful lobbying by the National Rifle Association (NRA), these waiting periods were set to expire when the FBI's National Instant Criminal Background Check System (NICS) was introduced in 1998. Since then, the NICS handles all BGCs related to the sales of firearms. While there is comparatively little regulation on gun ownership at the federal level, there is substantial heterogeneity in restrictions imposed by U.S. states. Constraints on private firearm ownership at the state level predominantly attempt to either prohibit potentially dangerous people such as convicted felons from acquiring guns or restrict the usefulness of firearms for unlawful purposes independent of the buyer.

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In this study, we focus on handguns since these, unlike long guns, have to be purchased in the state of residence, are a popular choice for self-defense, can be carried concealed, and are used in homicides substantially more often than long guns (Federal Bureau of Investigation, 2016). Our analyses utilize two types of delays between the decision to purchase and the moment the handgun is actually transferred. The first one is mandatory waiting periods. While the initial aim of waiting periods in the Brady Act was to give law enforcement agencies enough time to conduct BGCs, they also provide a "cooling-off" period and can thus help to prevent impulsive acts of violence (Cook, 1978; Andrés and Hempstead, 2011). In practice, buyers will perform a purchase (pass a NICS BGC and pay for the chosen gun) but can only receive their handgun after the waiting period has elapsed. The second measure is state requirements for licenses to lawfully possess or buy a handgun. Due to bureaucratic hurdles in the licensing process, these impose a de-facto waiting time. Prospective buyers have to request the permit at a local authority (e.g., a sheriff's office), pass a NICS BGC, and pay the associated fee.[7] Only after the permit has been processed and issued can they proceed with the purchase at their local dealer (usually without a renewed BGC).

In order to accurately determine the presence of delay laws and minimize misclassification, we utilize several sources and apply a rigorous coding procedure outlined with all details in Appendix Section A.1. The final state classification is reported in Appendix Table 27, which shows that during the period of our study, from November 2009 to October 2013, 15 states and the District of Columbia had adopted some form of delay laws throughout. Nine states (California, Florida, Hawaii, Illinois, Maryland, Minnesota, New Jersey, Rhode Island, Wisconsin) and the District of Columbia had

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

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imposed mandatory waiting periods on handgun purchases.[8] Connecticut, Hawaii, Illinois, Maryland, Massachusetts, New Jersey, New York, Nebraska, North Carolina, and Rhode Island all require a purchasing permit during the period of our study. Michigan abolished its handgun permit requirement in December 2012 and is thus the only state switching its delay legislation during our study period. For the remainder of this paper, we will refer to a state which implemented a mandatory waiting period, required a purchasing permit, or both, as a *Delay* state.[9] We refer to all other states as *NoDelay* states.

2.2 The Firearm Demand Shock of 2012/2013

Our analysis focuses on the firearm demand spike after the re-election of President Obama in November 2012 and the Sandy Hook shooting in December 2012. We decided on these two particular events to study the impact of delay laws on gun sales and homicides for two main reasons: first, these events then marked the largest hike in handgun sales since background data was collected in 1999. Such a strong shock is required in order to detect any statistically significant effects on firearm purchases and homicides. Secondly, unlike the numerous later shootings that grabbed nationwide attention, our setup features a pre-treatment period uncontaminated by other events, which is essential to accurately account for the seasonal nature of the data. In the following, we briefly describe the two events and the firearm demand hike of 2012/2013.

In the Presidential Election on 6 November 2012, President Barack Obama ran for a second term against Republican candidate Mitt Romney. While Romney took a more

[8]Wisconsin repealed its 48-hour handgun waiting period in only 2015 and is thus part of our sample.

[9]For purchasing permits, Table 27 states the maximum delay allowed by law. There is no reliable information on average delays that we are aware of. As we binarize the treatment, averaging would be inconsequential for our analysis.

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liberal position towards gun rights and was endorsed by the NRA, President Obama favored stricter gun control laws. In October 2012, almost all polls showed the race as within the margin of error, and President Obama's victory came so unexpectedly for Romney on election night that he had not even prepared a concession speech as internal polls had shown him winning (International Business Times, 2012). Similar to President Obama's first election in 2008, gun sales increased after his re-election, but this time with considerably larger magnitude (CNN, 2008; CNN Money, 2012; Depetris-Chauvin, 2015). This was likely because the President had started to speak more openly about favoring increased gun control measures in the wake of recent mass shootings, especially the one at a movie theater in Aurora, Colorado, in July 2012.

About a month later, on 14 December 2012, then 20-year-old Adam Lanza of Newtown, Connecticut first shot and killed his mother at their home before driving to Sandy Hook Elementary School. There he shot and killed six school employees and 20 students aged six to seven years. Lanza committed suicide shortly after the first law enforcement officers arrived at the scene. His motives are still not fully understood, but it has been suggested that he had a history of mental illness (New Yorker, 2014). The massacre was the deadliest ever U.S. school shooting and the third deadliest mass shooting in U.S. history at the time. This and 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 and quickly assembled a gun violence task force led by then-Vice President Joe Biden to collect ideas on how to curb gun violence and prevent future mass shootings. The task force presented their suggestions to President Obama in January 2013, who announced to implement 23 executive actions. These were aimed at expanding BGCs, addressing mental health issues and insurance

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coverage of treatment, as well as enhancing safety measures for schools and law enforcement officers responding to active shooter situations. Additionally, the task force proposed twelve congressional actions, including renewing the Federal Assault Weapons Ban, expanding criminal BGCs to private transactions, banning high-capacity magazines, and increasing funds for 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 to reinstate the Federal Assault Weapons Ban. While the bill passed the Senate Judiciary Committee in March 2013, it eventually was struck down on 17 April 2013 by the Senate 40-60 with all but one Republican and some Democrats opposing the bill. A bipartisan bill to be voted on that same day, introduced by Senators Joe Manchin and Pat Toomey, aimed at introducing universal BGCs, also failed to find the necessary three-fifths majority with 54-46, leaving federal legislation eventually unaffected.

Even though no new federal regulations followed, gun sales soared further in the months after the Sandy Hook shooting. Fear of stricter gun legislation and a higher perceived need for 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 surge after the shooting at Sandy Hook was unprecedented. The extreme demand shift even created supply problems for some dealers while others were hoping for 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 (The Intercept, 2015). In line with these anecdotes, Figure 1 shows a clear spike in gun sales starting in November/December 2012 after the Presidential election and the Sandy Hook shooting.

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While gun sales generally increase at the end of the year, this particular spike is far more pronounced and prolonged than in the years immediately before and after.

FIGURE 1 ABOUT HERE

3 Data

3.1 Handgun Purchases

One of the main challenges in our analysis is the absence of a central database of gun owners and firearm sales. To overcome this, researchers have often turned to proxy variables from surveys, vital statistics, crime data, and gun magazine subscriptions. While some of these indicators performed well in cross-sectional analyses, they have been found unsuitable for tracking gun ownership over time (Kleck, 2004). Since November 1998, Federal law dictates that an electronic NICS BGC be carried out for every firearm transaction through an FFL dealer. This publicly available data has the merit of being comparable across time, providing high coverage at a monthly frequency, and distinguishes between different types of transactions and firearms. The main variable in the first part of our analysis is NICS BGCs for handgun sales in a given state between November 2010 and October 2013, divided by the 2010 population in 100,000. In order to interpret our results as semi-elasticities and reduce the influence of outliers while keeping zero observations, we apply the *inverse hyperbolic sine* (IHS) transformation instead of taking natural logarithms.[10]

As pointed out in recent studies, the NICS data also exhibits significant drawbacks (Lang, 2013, 2016; Levine and McKnight, 2017). First, it can only measure flows of

[10]For convenience, we refer to the *IHS* transformation as *log* throughout the paper. We provide robustness checks in *levels* for our main specifications in the Appendix.

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weapons but does not allow inferring the stock of firearms or ownership levels. Second, flows might be substantially understated as about 22% of firearm sales are between private parties and occur in states which do not require BGCs for private transactions (Miller, Hepburn, and Azrael, 2017). Third, a BGC can occur for the purchase of multiple weapons, as well as an exchange of an old for a new firearm. Fourth, the data does not distinguish between approved and rejected BGCs, and even an approved check does not guarantee the sale of a firearm. Finally, some states require a BGC for a concealed carry permit application but not for a handgun purchase itself. Other states are running regular or irregular re-checks on existing permit holders and thereby inflate the counts or produce outliers.

We believe that our setup mitigates some of these problems. To start with, the aforementioned anecdotes, as well as findings from California by Studdert et al. (2017), indicate that many handgun purchases during the demand shock in late 2012 were made by new gun owners. With few sales to pre-existing gun owners, this should strengthen the correlation between handgun sale BGCs and changes in firearm ownership. Sales outside the NICS through private transactions and particularly gun shows are a concern but would only invalidate our results if they were more common in *NoDelay* states during the sales hike. Since many consumers were first-time buyers, we deem it more likely they were buying from a regular FFL dealer than privately.[11] Multiple purchases are unproblematic given our interest in the extensive margin of gun ownership. A boost in exchanges of old for new guns in *Delay* states could also overstate increases in firearm

[11] In Appendix Section B.5, we show that neither the supply of nor the demand for gun shows (the latter measured by Google Search results) witnessed a more substantial impact of the demand shock in *NoDelay* over *Delay* states, effectively showing that displacement to these states does not seem to be a cause for concern.

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ownership in those states. Since the likelihood of such exchanges should be correlated with pre-existing levels of gun ownership, we can control for this concern in additional robustness checks. Furthermore, work by Mueller and Frandsen (2017) has shown that only about 1.5% of BGCs across the U.S. are actually rejected, which severely limits the impact of this potential source of error. There is also no strong indication that the demand shock affected the rejection probability asymmetrically across *Delay* and *NoDelay* states. Finally, we add BGCs for permits to our measure of handgun sales to capture cases where buyers obtain a permit to purchase a handgun.[12]

A closer investigation of the NICS data revealed several outliers and reporting issues. We, therefore, removed Hawaii, Illinois, Kentucky, Massachusetts, Pennsylvania, and Utah, as well as parts of the series for Iowa, Maryland, and Wisconsin from the sample.[13] We also drop Connecticut and Michigan. Connecticut was host to the Sandy Hook shooting and thus may have potentially experienced lower gun sales after the shooting due to social pressure or psychological effects on residents. Michigan switched treatment status during our period of observation from requiring a permit to not requiring a permit. Performing the steps above yields our baseline sample consisting of 43 U.S. states for

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

[13]Outliers are mainly due to permit re-checks and law changes associated with large mechanic jumps in BGC activity. We provide explicit reasoning for these choices in Appendix Section A.2.

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investigating the effect of delay laws on handgun sales (*BL1*). While we prefer this restricted sample for our NICS analysis, robustness checks for our main results show that alternative (and less restrictive) sample definitions generate qualitatively similar results.

3.2 Homicide and Mortality

For our primary outcome of interest, homicides, there are two main statistical sources in the United States: death certificates from the *National Vital Statistics System* (NVSS) and police reports from the FBI's *Uniform Crime Reporting Program* (UCR). Despite the UCR data being widely used to study crime, they are known to suffer from reporting issues that need to be taken into account by removing areas with unreliable data from the sample (Targonski, 2011). Coverage is therefore not universal. The NVSS data, on the other hand, contains all U.S. death certificates in a given year. We obtained the data via the *Center for Disease Control and Prevention* (CDC) for the entire sample period between November 2010 and October 2013. The NVSS contains ICD-10 codes for the underlying cause of each death, as well as the victim's demographics, county of residence, and injury circumstances, such as location and date. The ICD-10 codes allow distinguishing not only between homicides, suicides, and fatal accidents but also whether these were inflicted through a handgun or not.[14] In order to increase the power of our statistical analysis, we use the detailed geographical information in the NVSS and collapse data at the county-month level. This provides us with a balanced panel of homicide counts for 3,047 counties which we normalize by their 2010 population in

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

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100,000. This second baseline sample, denoted as *BL2*, covers every U.S. state apart from Connecticut and Michigan for the same reasons as stated above, and we use it in all analyses based on non-NICS data. Figure 2 shows the counties in our NVSS sample BL2 and highlights the states excluded in the NICS sample BL1. In robustness checks, we show that applying more or less stringent sample restrictions yields very similar results.

FIGURE 2 ABOUT HERE

In order to cross-validate our results and delve deeper into homicide circumstances, we also use the *Supplementary Homicide Reports* (SHR) series from the aforementioned UCR data, bearing in mind the limitations of the data. These reports are compiled from voluntary submissions by individual law enforcement agencies to the FBI and contain detailed information such as demographics of victim and offender, the type of weapon used as well as murder circumstances (e.g., argument or gang-related crime). We clean the SHR data following the procedure described in Appendix A.4 and then collapse observations into a balanced monthly panel for 2,091 counties. Counts are normalized using the aggregate population in 100,000 covered by the reporting agencies within a specific county in 2010. Both UCR and NVSS crime rates are converted into logs using the same IHS transformation as for the NICS data.

3.3 Gun Interest and Controls

To assess whether consumers in states with and without handgun purchase delays have similar preferences, one needs to separate initial intentions to buy handguns from actual purchases. While we use NICS data to measure the latter, we rely on internet search data from *Google Trends* to proxy for people's intention to purchase firearms. We focus on searches for the term "gun store," which prior research has shown to be a good predictor

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of firearm purchasing intentions (Scott and Varian, 2014). Since the search data comes in relative numbers, we adopt a technique similar to that used by Durante and Zhuravskaya (2018) to construct a state-level panel of monthly Google searches for "gun store".[15]

In addition to this, we use several control variables to account for potential confounders as well as differences in socio-economic characteristics across counties and states. Our core set of covariates includes the log of population, the shares of the population living in rural areas and below the poverty line, as well as the percentages of Black and Hispanic inhabitants. All variables were obtained from the 2010 U.S. Decennial Census at the county level (and aggregated for state-level analyses). In addition, we collected state-level data on the percentage of households with internet access from the 2010 American Community Survey, which we include in regressions using Google search data. In selecting these control variables, we broadly followed the choices made in prior studies which have investigated the relationship between firearm prevalence and crime (e.g., Cook and Ludwig, 2006; Duggan, 2001). Further variables used only for robustness checks, such as measures of gun popularity, are introduced and explained where appropriate.[16]

4 Empirical Strategy

4.1 Difference in Differences Approach

To estimate the effect of delay laws on handgun purchases and mortality during the demand shock, we use a DiD regression model, which overlays the cross-sectional variation in pre-existing purchase delay laws with time-series variation from the six-month surge in

[15] Further details on this procedure are reported in Appendix Section A.5.

[16] Summary statistics of all variables can be found in Appendix Table 30. Appendix Table 31 performs mean difference tests on the primary outcome and control variables.

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firearm demand across the United States. To account for location-specific seasonality, all outcome variables are seasonally differenced by subtracting their 12-month lag (denoted as Δ_{12}). Seasonally differencing IHS-transformed variables approximate year-to-year growth rates. Coefficients can thus be interpreted as either changes in (nominal) growth rates or proportional changes in the outcome variable. Similar transformations of crime counts have, for instance, been applied in Draca, Machin, and Witt (2011). Our main specifications thus read as follows:

$$\begin{aligned}\Delta_{12}\, log(HandgunSales_{st}) &= \alpha + \beta_1(Delay_s \times Post1_t) + \beta_2(Delay_s \times Post2_t) \\ &\quad + \delta_{\mathbf{t}}\mathbf{X_s} + \lambda_t + \phi_s + \epsilon_{st} \qquad (1)\end{aligned}$$

$$\begin{aligned}\Delta_{12}\, log(Homicides_{ct}) &= \alpha + \beta_1(Delay_s \times Post1_t) + \beta_2(Delay_s \times Post2_t) \\ &\quad + \delta_{\mathbf{t}}\mathbf{X_c} + \lambda_t + \phi_c + \epsilon_{ct} \qquad (2)\end{aligned}$$

We use Equation 1 to estimate the effect of the demand surge on handgun sales in *Delay* over *NoDelay* states. Equation 2 is effectively the county-level analog of Equation 1 but instead uses homicide rates as outcome variables. In these equations, the specific effect of delay laws during the demand shock captured via $Delay_s \times Post1_t$ can be regarded as a shifter for new gun owners. $Delay_s$ is a dummy variable for states with delay laws as described in Section 2.1 and summarized in Table 27, i.e., California, Florida, Hawaii, Illinois, Iowa, Maryland, Massachusetts, Minnesota, Nebraska, New Jersey, New York, North Carolina, Rhode Island, Wisconsin, and the District of Columbia. $Post1_t$ is a dummy for time periods starting with President Obama's re-election in November 2012 and ending after April 2013 when the proposals for a renewed assault weapons ban and universal BGCs were defeated in the U.S. Senate. Our primary coefficient of interest is β_1 and captures the average proportionate difference in *HandgunSales* and *Homicides* between *Delay* and *NoDelay* states during the demand shock. We also include a second

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interaction using the time dummy $Post2_t$ for May 2013 to October 2013 to investigate effects beyond the initial six months. This also allows testing whether *Delay* states experience comparatively fewer handgun purchases over the entire time period or if this is compensated by more sales later on.

Apart from time fixed-effects λ_t, the DiD regressions also allow for location-specific linear trends ϕ_s and ϕ_c to account for the possibility that some areas may deviate from general trends in BGCs and homicides. Furthermore, our regression models each also feature a set of control variables $\mathbf{X}$. We avoid concerns about "bad controls" by using interactions of pre-determined, time-invariant factors and time fixed effects. The variables included in this way are % Hispanic, % Black, % rural, the log of population, and % poverty. ϵ denotes the residual. The standard errors used for inference are clustered by state as the level of treatment assignment to account for serial correlation in the error terms. Regressions are weighted by the state/county population to reduce the impact of less densely populated areas and to obtain U.S.-wide average effects.[17]

A potential alternative to our approach would be to estimate a gun owner-homicide elasticity using $Delay_s \times Post1_t$ as an instrument. Our preference for the somewhat cruder reduced-form relationship stems from two factors. The first is the limitations of the NICS data discussed above. BGCs do not allow to draw direct inference on changes in the existing population of gun owners, making an elasticity hardly comparable to other studies. This concern is compounded by issues of measurement error, as not all BGCs lead to gun purchases, and not all purchases are reflected in the BGC counts. Our second concern is that we do not expect the effect of gun owners on homicides to be overly large since the vast majority of gun owners are law-abiding citizens (Fabio et al., 2016). To

[17] Each of these estimation decisions is reassessed in sections 5.1 and 6.2, and we provide supplementary results in the Appendix.

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precisely estimate such a small effect, one would need a fairly large sample at the county level for which, however, no NICS data exists. We thus estimate the raw effect of handgun purchase frictions on sales and homicide rates during the demand shock but do not pin down a precise elasticity given the absence of reliable panel data on firearm ownership.

4.2 Validity of Identifying Assumptions

In order for our DiD design to yield causal effects, two assumptions need to be fulfilled. The first, commonly referred to as the parallel trends assumption, requires outcomes to have evolved similarly in the absence of treatment. This may create valid concerns as delay laws have not been exogenously assigned to states, and as such, any differential reaction to the shock could just be an expression of differences in unobservables. We take several measures to alleviate concerns that this assumption may be violated. First, we show that our outcome measures were following similar trends in *Delay* and *NoDelay* states prior to the demand shock to prevent that our estimates are simply picking up pre-treatment divergence. As we can see from Panels A and B in Figure 3, handgun sales and homicides in both groups of states are sharply diverging during the six-month window of increased firearm demand. There is also a slight divergence for handgun sales in preceding years which highlights the need for seasonal differencing.[18] Second, we report results with location-specific linear time trends for all our specifications as a first robustness check. In order to credibly identify pre-existing trends, our baseline sample length uses an asymmetric sample period 36 months before to 12 months after the 2012

[18]Appendix Figures 23/24 and 25/26 depict the evolution of both variables in levels and 12-month growth rates.

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election (November 2009 to October 2013) in the spirit of Wolfers (2006).[19] Finally, we also perform an event-study analysis to investigate concerns about non-linear pre-trends.

FIGURE 3 ABOUT HERE

The second prerequisite is the absence of correlated shocks, i.e., other events coinciding with the demand hike and being positively (negatively) correlated with the existence of delay laws but negatively (positively) with BGCs and homicide rates. As argued above, the outcome of the 2012 election, as well as the timing of the Sandy Hook shooting, are unrelated to any relevant outcome variables and were arguably the most notable events at that time. We tackle the remaining concerns in three ways: First, all regressions control for socio-demographic factors known to be correlated with both gun ownership and crime. Second, we corroborate the role of delay laws by running horserace regressions where we add interactions of time dummies with potential confounders related to political leanings as well as preferences for and supply of firearms. Finally, in Section 5.3, we use Google search data to show that the divergence in gun sales after the shock does not coincide with a similar divergence in the interest to purchase a firearm.

5 The Effect of Delay Laws on Firearm Purchases

5.1 Results

TABLE 1 ABOUT HERE

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

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In Table 1, we estimate the differential impact of the 6-month demand hike in *Delay* states on our handgun sale measure as well as total and non-handgun sale BGCs per capita. The main coefficient of interest is β_1 from Equation 1, which represents the percentage difference of the sales rate response to the demand shock in *Delay* states compared to *NoDelay* states. Column 1 shows a significant negative effect in the first six months after the Presidential election and a positive non-significant effect in the second period. This potential postponement effect, however, disappears when adding controls in column 2, while the coefficient for the *Post1* period remains marginally significant. After adding state-specific linear time trends in column 3 and accounting for potential pre-trends, the estimate for β_1 gains precision while β_2 decreases further. A very likely explanation for this result would be that this specification reduces noise from diverging trends in smaller states without significantly influencing the overall (weighted) coefficient.

Our preferred estimate is the more conservative specification in column 3.[20] The results imply that sales rates were 7.3% lower in *Delay* states during the first six months than in *NoDelay* states.[21] Columns 4 to 7 show that delay laws did not significantly affect overall BGCs or other gun-related transactions like long gun sales.

5.2 Robustness Checks

As highlighted in Section 4.2, our identification strategy hinges on the validity of the parallel trends assumption and the absence of correlated shocks. Even though our results

[20]Both specifications are informative, however, in our view. As we do not know whether the 'true' model exhibits trends, it is ex-ante unclear whether column 2 or 3 should be preferred. We, therefore, report specifications with and without trends for all results in order to provide a more complete picture.

[21]Note that for all results in logs (IHS), the interpretation of the coefficients is a change in percentages. In levels, the coefficients represent percentage point changes.

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in Table 1 are robust to the inclusion of state-specific linear trends, one may argue that this does not accurately capture non-linear pre-trends. We investigate this possibility using an event-study design based on column 2 in Table 1, in which we allow for quarterly treatment effects. The results are depicted in Panel A of Figure 4 and show no indication of non-linear pre-trends.[22] In the two years before November 2012, we do not observe a clear pattern of up- or downward trends in our estimation. In the quarter following the 2012 Presidential election, however, the effect of *Delay* states on handgun sales turns significantly negative. After that, the coefficients gradually move back to the pre-period level and remain insignificant for the entire *Post2* period. This also provides additional evidence against the possibility that firearm purchases were merely postponed.

FIGURE 4 ABOUT HERE

In Appendix Section B.1, we demonstrate that no other factors related to the existence of delay laws systematically affected handgun sales during the demand shock and provide a host of additional robustness and sensitivity checks. These additional analyses suggest that the omission of Texas reduces the effect, as the state's regression weights are redistributed to a large number of states. If each state suffers from measurement error with some probability, spreading the weights will increase the overall impact of mismeasurement. Furthermore, population weighting is necessary to correctly capture countrywide effects as the effect arises predominantly in urban areas. Placebo regressions,

[22]For this analysis, we aggregate the data into 3-month bins starting in November since "classic" quarters would result in one fully and two partially treated time periods. Appendix Figure 31 shows the same graph using monthly data. Appendix Figure 27 reports a similar event study graph without seasonal differencing extending over a longer period.

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different sample definitions, removing single states from the sample, results in levels and/or without seasonal differencing, weighting by the adult population, controlling for the economic environment, and using alternative clustering techniques confirm the robustness of our findings.

5.3 Mechanisms

Having established different reactions in handgun sales between *Delay* and *NoDelay* states, we proceed by evaluating whether our findings could be driven by impulsive consumers. The first appraoch to characterize impulsive agents is the potential divergence between plans and actions. In other words, impulsive consumers may decide to buy a firearm under the influence of transient emotions but eventually do not buy since these emotions have already passed. This should not be observed for regular, non-impulsive consumers if they make a perfectly rational purchase decision. However, a delay in receiving the gun makes the purchase also less attractive for non-impulsive consumers since it reduces the item's net present value. If, however, the decision not to buy is driven predominantly by standard exponential discounting, we should observe that longer delays reduce purchases substantially more than shorter delays. Impulsive agents, however, would be deterred by any delay since they cannot get hold of the firearm while being in a particular emotional state. The second key characteristic of impulsiveness would thus be that even very short delays should have a notable impact on the likelihood to buy.[23]

TABLE 2 ABOUT HERE

[23]These predictions can also be formally derived in a theoretical framework which is available on request but omitted here for the sake of space.

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We start by investigating the congruence between plans to buy firearms and actual sales. This analysis uses Google searches for the term "gun store," which serves as a proxy for public interest in buying a gun and has been identified as a strong predictor for firearm purchasing intentions in previous research by Scott and Varian (2014). Columns 1 and 2 in Table 2 repeat our preferred regression specifications using Google searches for "gun store" as the dependent variable. We do not detect large or significantly different changes in search results, which provides evidence that the different evolution of gun sales in the wake of the demand shock was not driven by different preferences for and intentions to buy firearms.[24] This is also additional evidence that our results are unlikely to be driven by unobserved state heterogeneity. More importantly, these findings indicate a mismatch between firearm purchase intentions and actual sales in *Delay* states. However, these results could also reflect that potential buyers do not know their state's firearm laws while searching for a gun store but only learn about delays at a later point and then deliberately decide not to buy. For such non-impulsive consumers, we should observe that decreasing delay lengths smoothly reduce the effect, which we test for next.

In columns 3 to 10 of Table 2, we use our two main specifications from Table 1 and gradually exclude states with delay lengths exceeding 30, 14, and 3 days. The table also features tests for coefficient equality of β_1 in the short-delay and the baseline sample. Overall, we do not detect strong variations in the estimated coefficients for β_1. In the most restrictive specifications 9 and 10, with only four treatment states and at most three days of delay, the estimates are still very close to the baseline in columns 3 and 4. The Wald tests can never confidently reject the null hypothesis of coefficient equality

[24]Figure 28 in the Appendix shows the development of Google searches between November 2009 and October 2013 graphically. A regression using levels and producing similar results can be found in Appendix Table 28.

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for β_1. The absence of a systematic decrease in the effect size suggests that gun buyers may, in fact, respond more to the presence of a delay per se rather than its length.[25] This evidence lends further support to the above conjecture that the difference in sales between the two groups of states is predominantly driven by impulsive consumers.

Another, competing explanation for the relative drop in handgun sales would be fear of tighter gun legislation. Such legislation would be particularly binding in *NoDelay* states which generally exhibit weaker gun legislation. The results in this and the previous section offer some insights into why this may not be the case. First, the Google search results in Table 2 favor impulsiveness as an explanation over rational, forward-looking behavior. Second, since firearm ownership is a constitutional right and handgun ownership, in particular, cannot easily be prohibited by the states, any belief in substantially more binding handgun ownership restrictions may be classified as distorted.[26] Holding such distorted beliefs makes further non-rational behavior conceivable. Third, the robustness checks in Table 7 and Appendix Sections B.1 and B.2 show that gun law strictness (or its absence) by and large does not explain away the effect of delay laws.

6 The Effect of Delay Laws on Homicides

6.1 Results

Having found that handgun sales increased significantly less in *Delay* states during the 2012 firearm demand shock, we investigate whether there was also a corresponding effect

[25]These findings are corroborated by a triple difference analysis presented in Appendix Table 35. In Appendix Table 29, we also show that including transaction costs from, e.g., gun licensing fees in our regressions does not qualitatively change our findings regarding the effect of purchase delay laws.

[26]This follows from the landmark ruling of D.C. v Heller, 554 U.S. 570 (2008).

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on homicide rates. Table 3 shows the results from Equation 2. Observations are now at the county-month level, and the sample includes all states previously omitted due to measurement error in the NICS data. Column 1 shows that *Delay* states saw a significant relative drop in gun homicide rates by 2.4% after the start of the firearm demand shock and an insignificant 1.4% relative decrease during *Post2*. Controlling for observables in column 2 yields a significant 2.2% relative drop in *Delay* states' handgun homicide rates during the treatment period *Post1* and an insignificant relative decline of 1.8% in *Post2*. The inclusion of county trends in column 3 mainly leads to a loss in precision but only slightly diminishes β_1 to −0.019, which is still significant at 5%.[27]

FIGURE 3 ABOUT HERE

Columns 4 and 5 show that the $Post_1$ effect for handgun homicides is also reflected in decreased aggregate homicide rates of similar magnitude. This effect is significant at the 5% level without trends but loses significance when these are included. Notably, there is virtually no impact of delay laws on overall homicides in the $Post_2$ period. The reason for this becomes apparent when looking at specifications 6 and 7, which show a significant *increase* during $Post_2$ for non-handgun homicides. A straightforward explanation could be that the reaction of *NoDelay* states reflects two different channels through which increased handgun ownership can affect homicides. One would be a *lethality effect* by which random acts of aggression or anger turn into the shooting and killing of another person. The second effect would be a *substitution effect* whereby homicides are simply carried out using handguns instead of other weapons with no aggregate effect. While the

[27]Our results thus imply an elasticity of homicide with respect to gun sales of between 0.23 and 0.3. This compares to an elasticity of 0.2 reported in Duggan (2001) or 0.1-0.3 in Cook and Ludwig (2006).

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former suggests an immediate, impulsive killing that would not have arisen without a gun, the latter constitutes a less specific crime that would have taken place in any case. Our results are indicative of both effects, with lethality being more prevalent during *Post1* and substitution dominating the *Post2* period (possibly because homicides are generally more prevalent in the months of the year also included in *Post2*). Since our main interest is delay laws' aggregate effects, the remainder of the paper focuses on the lethality effect and the impact of delay laws on handgun-related homicides during the *Post1* period.

Columns 8 and 9 use the violent crime rate (following the FBI's definition) as the dependent variable to investigate whether the decrease in homicides may have been counteracted by an increase in other types of violent crime and thus provide a test of the "more guns, less crime" hypothesis. The estimated coefficients, however, are insignificant, small in magnitude for the *Post1* period and point in the same direction as the coefficient for homicides. If anything, more handguns thus increased violent crime in our setting.[28]

6.2 Robustness Checks

We run similar checks as in Section 5.2 to establish the validity of our identification strategy. First, we investigate the possibility of non-linear pre-trends using the same event-study design as for the NICS data. Panel B of Figure 4 indeed does not show any systematic effect for handgun-induced homicides before the onset of the treatment.[29]

[28]Appendix Section B.3 decomposes the overall violent crime rate and shows that aggregation does not hide substantial effects on individual categories of violent crime. Appendix Section B.4 shows the effect on suicides and accidents.

[29]Appendix Figure 32 reports a similar event study graph using monthly data. Appendix Figure 29 reports a similar event study graph without seasonal differencing extending over a more extended period.

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During our treatment period *Post1*, however, there is a clear negative impact for the first quarter following the demand shock and a slightly smaller one for the second treatment quarter, which lines up with the patterns observed for handgun sales rates in Panel A of Figure 4.[30] In Appendix Section B.2, we show that our findings on handgun homicide rates are also not a by-product of underlying differences in political leanings, law stringency, and preferences for and supply of firearms, and we discuss and report placebo checks, state-level results, and sensitivity to alternative sample definitions, data transformation, and weighting choices. As with the BGC results, population weighting is necessary to correctly capture countrywide effects as the effect arises predominantly in urban areas. Placebo regressions, different sample definitions, removing individual states from the sample, results in levels and/or without seasonal differencing, weighting by the adult population, controlling for the economic environment, applying other clustering techniques, and using state-level aggregates confirm the robustness of our findings.

6.3 Mechanisms

Section 5.3 provided tentative evidence that impulsive consumers are likely to drive the differences in handgun sale BGCs between *Delay* and *NoDelay* states. In this section, we provide evidence that our results on homicides can also be traced back to impulsive behavior. We do so by taking a closer look at the type of additional handgun homicides in *NoDelay* states (or equivalently, which were "prevented" in *Delay* states).

[30]Appendix Figure 30 shows no systematic effect on non-handgun homicides before or during our treatment. The positive effect during *Post2* in the baseline regressions applies to all three-month periods but is only statistically significant for May to July 2013.

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Panel A of Table 4 presents the results split up by victim sex with a particular focus on the 20 to 29 age group, into which the majority of first-time buyers should fall.[31] The results show that men make up about 2/3 of the victims while women account for 1/3. The coefficients for female victims, however, are more precisely estimated. Both male and female victims are predominantly aged 20 to 29. These findings suggest that female victims are overrepresented, as less than 10% of overall homicide victims are women in our data.[32] Given this and our evidence on impulsive consumers, we investigate the role of domestic violence. To do so, we split the handgun homicide victims into those who were shot in their homes and those who were assaulted elsewhere. Panel B of Table 4 reports the corresponding results. For the male victims, we find that the entire effect is driven by attacks outside their homes. Female victims, on the other hand, are predominantly assaulted in their place of living, consistent with instances of domestic violence.

To further investigate the role of domestic violence and impulsive killings more generally, we present results using the UCR SHR data on homicide circumstances in Panel C of Table 4.[33] Columns 1 to 2 show the baseline specification for handgun homicides reported

[31]Appendix Table 32 shows corresponding results for all other age groups. We also report victim splits by race in Appendix Table 33 and show that, in line with the overall demographics of homicide victims in the United States, victims tend to be almost evenly categorized as ’White’ and ’Black.’

[32]To judge how common homicides of each category are, Panels A, B, and C of Table 4, as well as Appendix Tables 32 and 33, include an additional row reporting the mean of the non-differenced dependent variable in levels.

[33]As outlined in Section 3.2, this data exhibits a more restricted coverage. Appendix Table 34 shows that the UCR SHR data yield qualitatively similar estimates compared to the NVSS data in our *Post1* period of interest. A map illustrating the exact coverage for the UCR data is shown in Appendix Figure 12.

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in the UCR SHR and then split these into specific murder circumstances. The results for aggregate handgun homicides have the same sign as those using the NVSS data but are only about 2/3 in size and insignificant, likely due to the more limited coverage and data quality. The results in columns 3 and 4, however, indicate that deadly assaults related to arguments account for the main part of the additional handgun homicides in *NoDelay* states. Unlike the aggregate handgun murder rate, this effect is also highly significant. All other types of homicide circumstances such as brawls, (organized) crime, and defense, as well as other/undetermined, do not seem to be systematically affected during the *Post1* period. These findings lend further support to the hypothesis that impulsive consumers are driving the differences in handgun homicides during the demand shock.

TABLE 4 ABOUT HERE

Summarizing these findings, we observe that the additional homicides of females in *NoDelay* states primarily happened inside their home, predominantly to women between 20 and 29, and often as a result of arguments. Homicides of men, instead, happened primarily outside their home, but also primarily because of arguments. Similar to women, male victims are typically 20-29 years old. In terms of mechanisms, our findings suggest domestic violence and other *heat of the moment* murders as a possible explanation for the observed differences in homicides between *Delay* and *NoDelay* states. These interpretations are in line with insights by Tangney, Baumeister, and Boone (2004) that impulsiveness is correlated across domains.

7 Conclusion

In light of the persistently high rate of firearm homicides in the United States, understanding the consequences of legislation limiting access to guns is imperative. One of the main

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arguments used by proponents of *gun rights* is that gun laws do not substantially affect violent crime but impose excessive burdens on law-abiding gun owners. In this study, we focus on the effects of a specific type of policy measure, handgun purchase delay laws, and provide evidence that, while not infringing with Second Amendment rights, these laws can substantially reduce homicides by preventing impulsive purchases.

We present empirical evidence that states with delay laws in place saw comparatively lower handgun sales during a demand shock after the re-election of President Obama in 2012 and the shooting at Sandy Hook Elementary School. Further results show that purchase delays have strong effects even when they are very short and did not affect intentions to buy a firearm but only the likelihood of consumers making an actual handgun purchase. In the second part of our analysis, we investigate delay laws' effect on homicide rates. Using detailed micro-data on mortality, we find a significant effect of delay laws on handgun-related homicides during the period of the demand shock. The effect size is about 2%, which in turn implies that about 200 homicides could have been "prevented" during the six-month *Post1* period if all U.S. states had had some sort of purchase delay law in place. These additional homicides encompass both genders and indicate that arguments, as well as domestic violence, constitute some of the main channels through which handgun ownership by impulsive individuals may affect homicide rates.

We see our study as a good starting point for more nuanced investigations into the relationship between gun ownership and crime. First, additional direct evidence on the circumstances linking gun sales to violent crime is needed. While our results were able to point in the direction of arguments and domestic violence, the results are far from clear-cut. With increasing coverage of the FBI's National Incident-Based Reporting System (*NIBRS*), more detailed information on particular crime incidents could be utilized to study similar future firearm demand shocks. Second, given the absence of accurate data

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on how county-level gun ownership evolves over time, our study cannot pin down an exact gun-homicide elasticity. The NICS data is very noisy and makes cross-state comparison impossible at times. We thus stress the need for a more transparent, county-level version of handgun sales than what is currently available. Finally, we believe that more research is needed to evaluate the costs and benefits of specific gun laws. As shown in this study, the positive effects of purchase delays may be understated. Rigorous analyses of gun laws may therefore help foster a more informed debate on gun policy.

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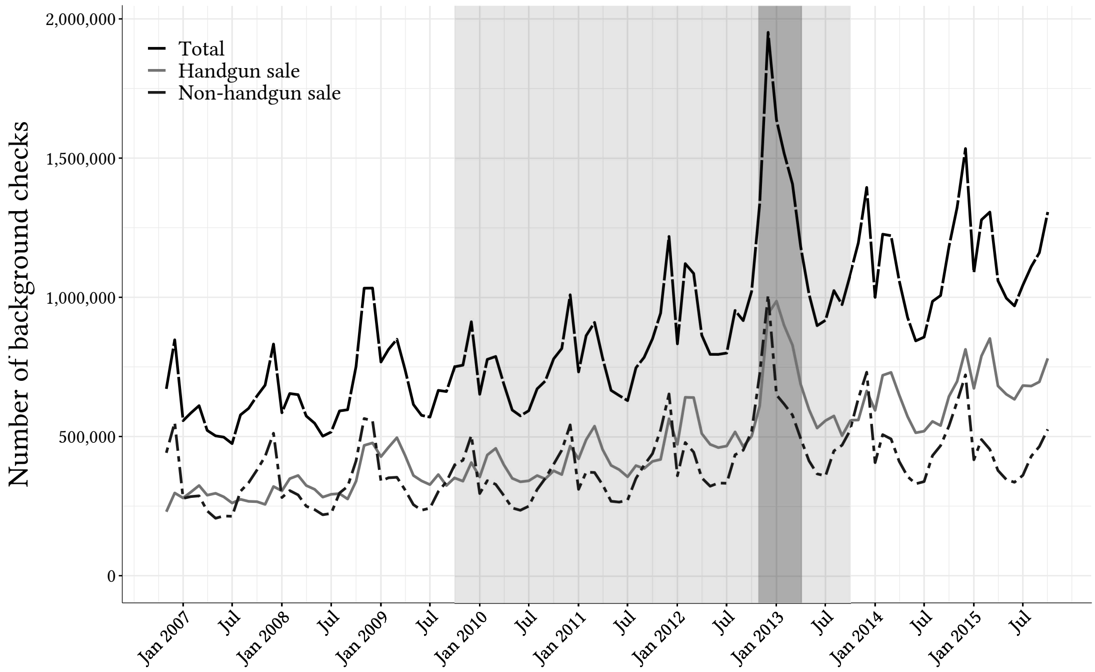

Figure 1: NICS BGCs

Notes: Monthly federal NICS BGCs between November 2007 and October 2015 in absolute numbers. The sample encompasses data for all states consistently included in our main specification as per Section 3.1. The light gray area is our sample window; the dark grey area depicts the six months after the 2012 election and the shooting at Sandy Hook. The gray line shows BGCs for handguns, the dashed black line all other firearm-related BGCs, and the solid black line displays the sum of the two.

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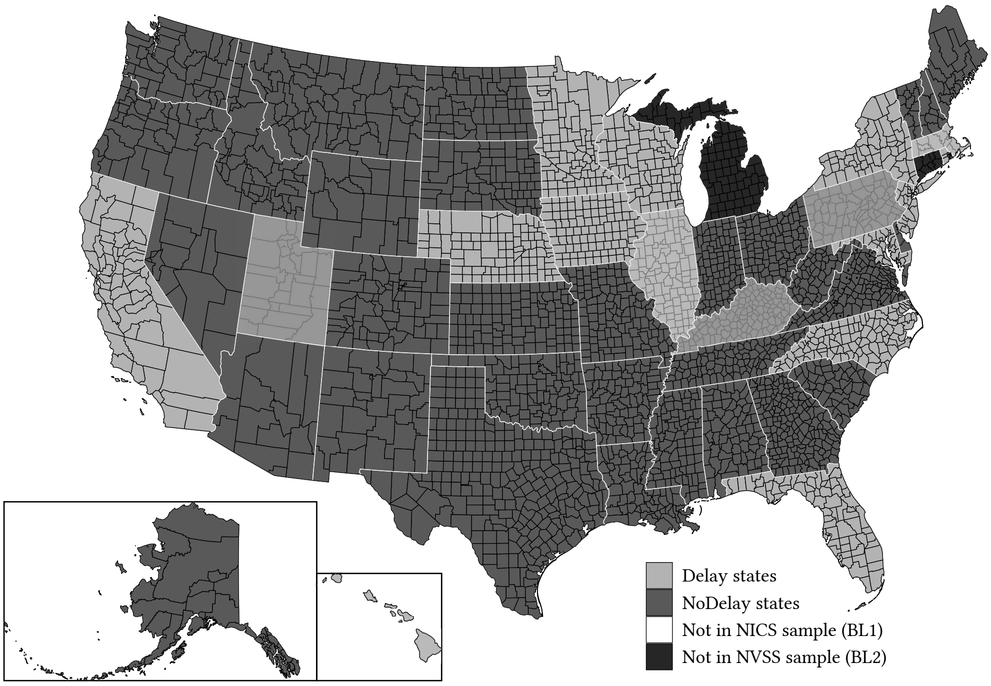

Figure 2: States and counties represented in the NICS and NVSS samples

Notes: Map of the United States showing the states contained in the NICS BGC data and counties contained in the NVSS homicide data. Dark gray counties are located in *NoDelay* states. Light gray counties are located in *Delay* states. Shaded states are dropped from the NICS sample. Near-black counties are not included in the NVSS sample.

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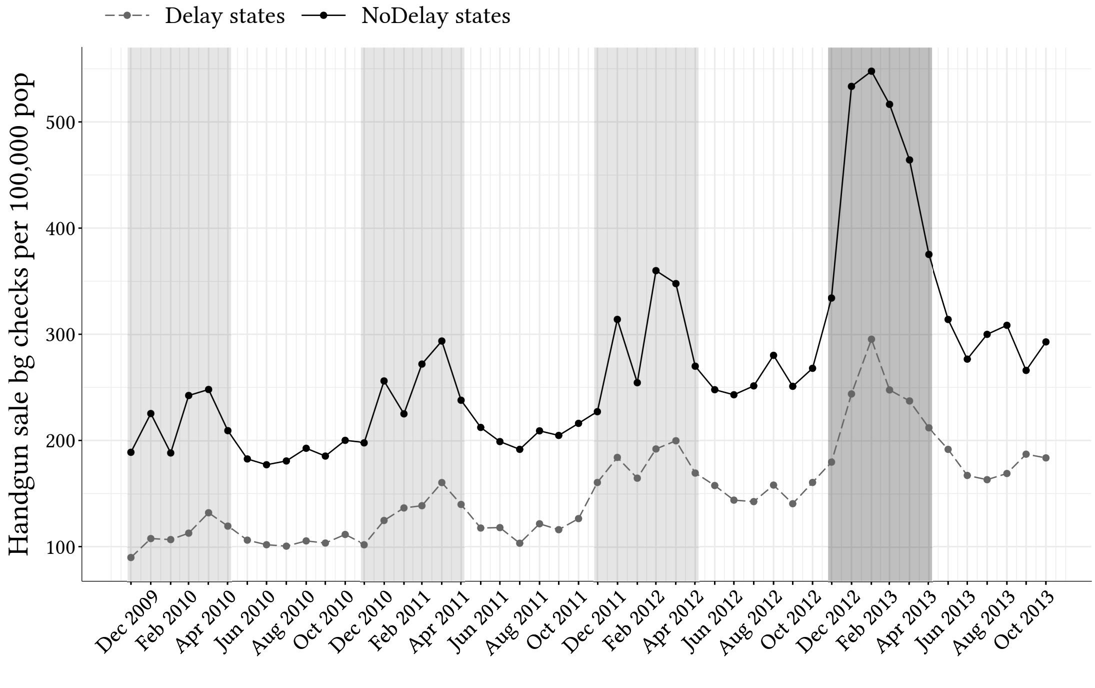
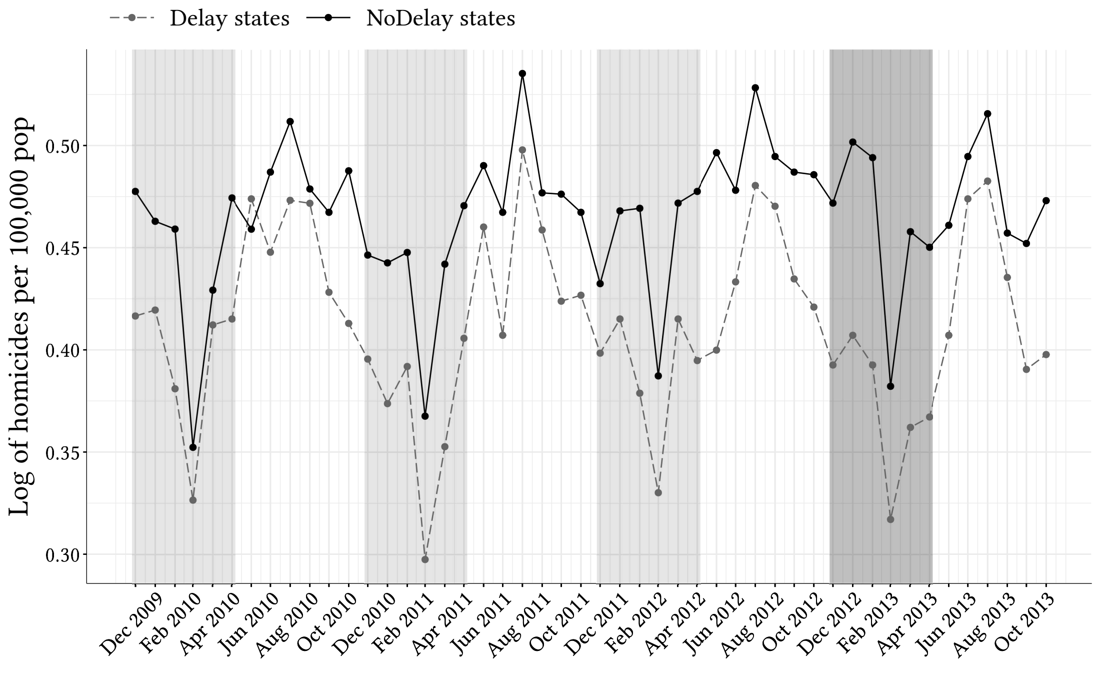


(A) Log BGC rate for handguns



(B) Log homicide rate

Figure 3: Evolution of outcome variables in *Delay* vs *NoDelay* states

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

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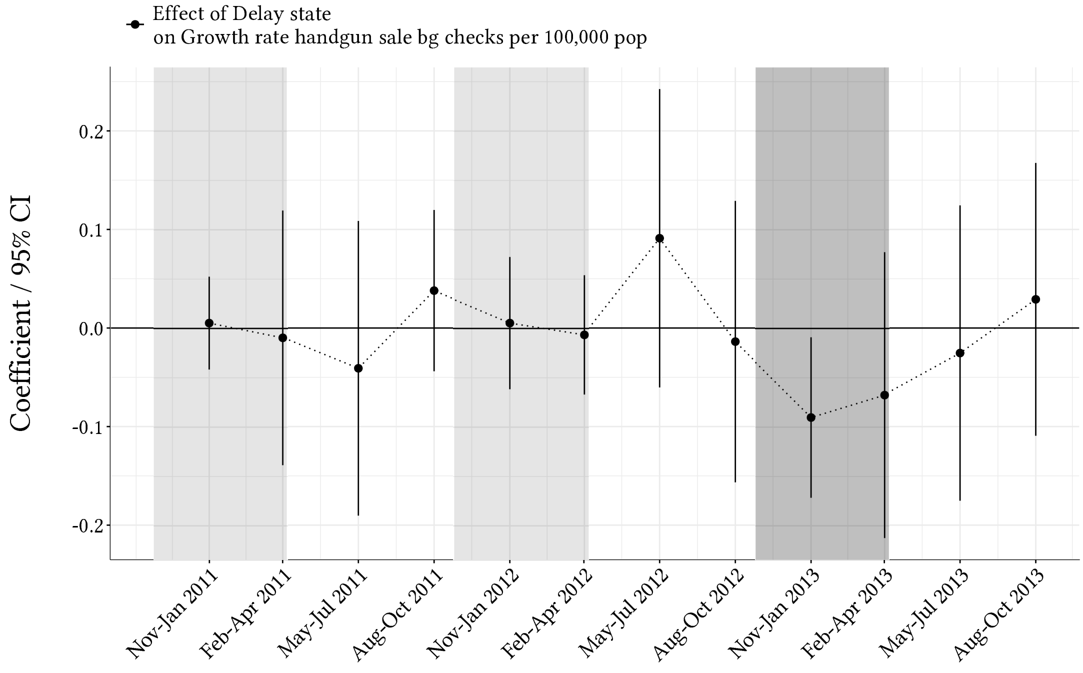
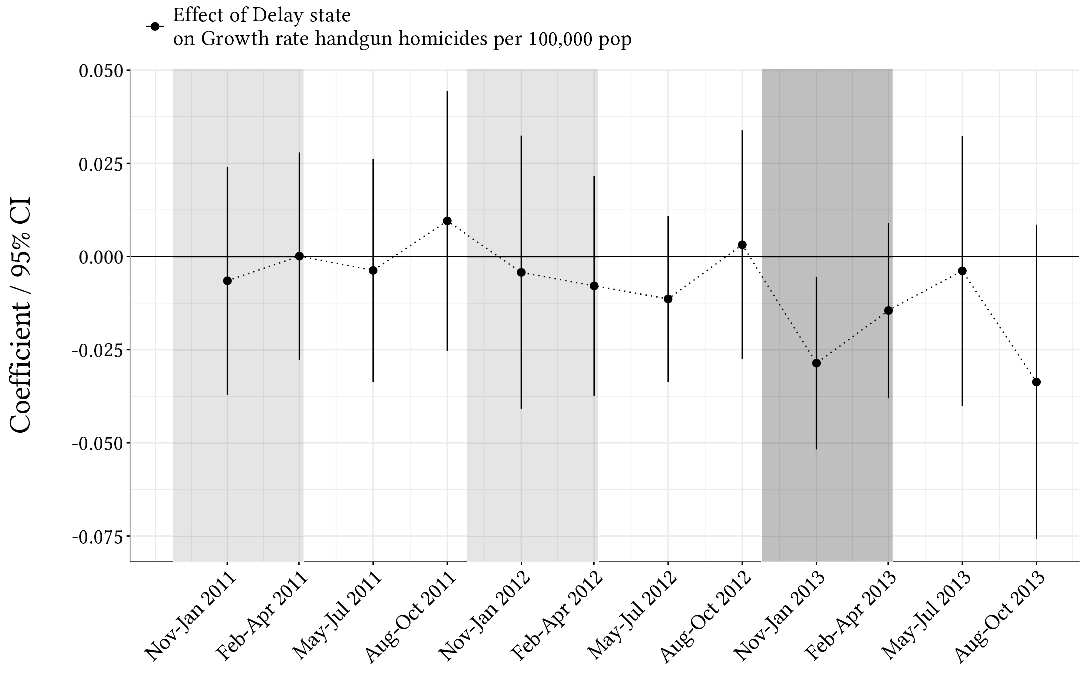


(A) NICS BGCs



(B) Handgun homicide rate

Figure 4: Event study graphs

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

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Table 1: Handgun sale BGCs

	Δ_{12} Log of BGCs per 100,000 inhabitants						
	Handgun Sale			Total		Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Delay×Post1	−0.112***	−0.081*	−0.073**	−0.036	−0.028	0.016	0.026
	(0.041)	(0.045)	(0.033)	(0.028)	(0.024)	(0.053)	(0.049)
Delay×Post2	0.057	0.008	0.005	0.048	0.053	0.113	0.127
	(0.062)	(0.066)	(0.086)	(0.054)	(0.062)	(0.097)	(0.095)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	Y	Y	Y	Y
State Trends	N	N	Y	N	Y	N	Y
States	43	43	43	43	43	43	43
Observations	1,516	1,516	1,516	1,516	1,516	1,516	1,516
R^2	0.446	0.538	0.591	0.685	0.721	0.676	0.756

Notes: Observations are at the state-month level. The sample period is November 2010 until October 2013, i.e., an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Standard errors clustered at the state level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % Black and % Hispanic. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the state population.

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Table 2: Online searches & Handgun BGCs (delay length)

	Δ_{12} Log std'zed share of Google searches for "gun store"		Δ_{12} Log of handgun BGCs per 100,000 inhabitants							
Maximum delay length D			Baseline (=12 delay states)		$D \leq 30$ Drop NY (=11)		$D \leq 14$ Drop MD, NC, NJ (=8)		$D \leq 3$ Drop CA, DC MN, RI (=4)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	0.043	−0.045	−0.081*	−0.073**	−0.074	−0.072**	−0.105**	−0.088**	−0.071**	−0.074*
	(0.072)	(0.080)	(0.045)	(0.033)	(0.049)	(0.035)	(0.052)	(0.041)	(0.035)	(0.038)
Delay×Post2	−0.029	−0.116	0.008	0.005	0.012	0.001	−0.001	−0.004	−0.131**	−0.173
	(0.104)	(0.138)	(0.066)	(0.086)	(0.072)	(0.094)	(0.080)	(0.116)	(0.058)	(0.119)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State Trends	N	Y	N	Y	N	Y	N	Y	N	Y
States	49	49	43	43	42	42	39	39	35	35
Observations	1,764	1,764	1,516	1,516	1,480	1,480	1,374	1,374	1,230	1,230
R^2	0.276	0.310	0.538	0.591	0.547	0.599	0.558	0.603	0.612	0.661
$p(\beta_1 = -0.073)$					0.98	0.99	0.54	0.7	0.97	0.98

Notes: Specifications as per Table 1. Google search results include % with internet access as additional controls.

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Table 3: Baseline: homicide rates

	Δ_{12} Log of ... per 100,000 inhabitants								
	Homicides							All violent crimes	
	Handgun			Any		Other			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Delay×Post1	−0.024***	−0.022***	−0.019**	−0.024**	−0.021	−0.001	−0.000	0.004	0.002
	(0.009)	(0.008)	(0.010)	(0.012)	(0.015)	(0.010)	(0.013)	(0.019)	(0.023)
Delay×Post2	−0.014	−0.018	−0.016	0.003	0.005	0.023***	0.024**	−0.018	−0.019
	(0.012)	(0.015)	(0.018)	(0.017)	(0.022)	(0.008)	(0.011)	(0.028)	(0.033)
Year-Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	Y	Y	Y	Y	Y	Y
County Trends	N	N	Y	N	Y	N	Y	N	Y
Counties	3,047	3,047	3,047	3,047	3,047	3,047	3,047	2,091	2,091
Observations	109,692	109,692	109,692	109,692	109,692	109,692	109,692	75,276	75,276
R^2	0.002	0.008	0.019	0.006	0.016	0.005	0.014	0.007	0.034

Notes: Observations are at the county-month level. The sample period is November 2010 until October 2013, i.e., an asymmetric 36-month window 2 years before and 1 year after the 2012 election. Standard errors clustered at the state level are in parentheses: *p<0.1; **p<0.05; ***p<0.01. Included control variables are log(population), % rural, % below poverty line, % Black and % Hispanic. All variables are as of 2010 and interacted with Month FE. Regressions are weighted by the county population.

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Table 4: Effect on homicide rates: mechanisms

Panel A:	Victim age									
	Δ_{12} Log of handgun homicides per 100,000 inhabitants									
Victim sex	Any		Male				Female			
Victim age	Any		Any		20-29		Any		20-29	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	−0.022***	−0.019**	−0.013	−0.011	−0.011*	−0.008	−0.008**	−0.008*	−0.006***	−0.006**
	(0.008)	(0.010)	(0.008)	(0.009)	(0.006)	(0.006)	(0.003)	(0.005)	(0.002)	(0.002)
Delay×Post2	−0.018	−0.016	−0.018	−0.016	0.002	0.005	0.002	0.002	−0.002	−0.002
	(0.015)	(0.018)	(0.013)	(0.015)	(0.007)	(0.008)	(0.005)	(0.007)	(0.002)	(0.003)
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
Mean DV levels	0.287	0.287	0.243	0.243	0.099	0.099	0.045	0.045	0.012	0.012
R^2	0.008	0.019	0.008	0.020	0.012	0.023	0.005	0.014	0.007	0.016
Panel B:	**Place of Assault**									
Victim sex	Any		Male				Female			
Place of assault	Any		Home		Not Home		Home		Not Home	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	−0.022***	−0.019**	0.006	0.004	−0.018*	−0.014	−0.007**	−0.008**	−0.001	0.000
	(0.008)	(0.010)	(0.008)	(0.008)	(0.009)	(0.011)	(0.003)	(0.004)	(0.002)	(0.003)
Delay×Post2	−0.018	−0.016	−0.012*	−0.014*	−0.008	−0.004	0.001	0.001	0.000	0.002
	(0.015)	(0.018)	(0.006)	(0.008)	(0.011)	(0.012)	(0.005)	(0.006)	(0.002)	(0.003)
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
Mean DV levels	0.287	0.287	0.083	0.083	0.159	0.159	0.027	0.027	0.018	0.018
R^2	0.008	0.019	0.006	0.017	0.011	0.022	0.004	0.014	0.006	0.016
Panel C:	**Circumstances**									
	Δ_{12} Log of handgun murders per 100,000 inhabitants									
Circumstances	Any		Arguments		Brawls		Gang, Felony, or Defense		All Other	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Delay×Post1	−0.015	−0.018	−0.011***	−0.018***	0.002	0.002	−0.001	−0.010	−0.007	0.003
	(0.011)	(0.013)	(0.004)	(0.006)	(0.001)	(0.001)	(0.009)	(0.010)	(0.012)	(0.010)
Delay×Post2	−0.008	−0.011	−0.009*	−0.015*	−0.000	−0.000	−0.011	−0.019	0.013	0.023
	(0.017)	(0.022)	(0.005)	(0.008)	(0.001)	(0.001)	(0.008)	(0.014)	(0.012)	(0.016)
County Trends	N	Y	N	Y	N	Y	N	Y	N	Y
Mean DV levels	0.253	0.253	0.049	0.049	0.003	0.003	0.080	0.080	0.121	0.121
R^2	0.011	0.022	0.009	0.021	0.013	0.024	0.022	0.043	0.010	0.026

Notes: Panels A and B: All regressions use 109,692 observations from 3,047 counties. Panel C: All regressions use 75,276 observations from 2,091 counties. Specifications as per Table 3.