More Crime, More Guns? The Effects of Long- and Short-Run Crime on Gun Demand

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Abstract

Although self-defense is the most commonly cited reason for gun ownership, there is little causal evidence of how gun demand responds to changes in crime rates. We investigate the effect of crime on gun demand using three approaches. First, we examine large, persistent changes in crime stemming from police hiring grants to U.S. cities in 2009. We use grant receipt as an instrument to estimate the effect of crime on firearm demand, employing three different measures of local gun demand. We find no evidence that crime affects firearm demand under any of these measures. Second, we analyze the effects of massive, short-run spikes in local crime. Using detailed data on crime and firearm sales in Massachusetts, we find that gun purchases increase following unusually large spikes in crime over two-week intervals. However, these spikes are extremely rare, and the effects are modest, with an implied elasticity of gun sales with respect to crime ranging from 0.08 to 0.13. Taken together, these findings indicate that local crime plays little role in gun demand. Finally, we present descriptive evidence suggesting that politics, culture, and perceived threats beyond crime may drive defensive gun ownership.

1 Introduction

The U.S. firearm homicide rate exceeds the total homicide rate of most developed nations (Grinshteyn and Hemenway, 2019). However, understanding the causal effect of guns on crime is complicated by the possibility that individuals purchase firearms in response to crime. Self-defense is the primary reason individuals report owning firearms (e.g., Alsan et al., 2026), and this motivation has become increasingly prevalent over time: the share of gun owners citing protection against crime as a reason for ownership rose from 67% in 2000 to 88% in 2021 (Gallup, Inc., n.d.b).

This growing emphasis on self-defense coincides with a substantial increase in firearm demand as per capita firearm production rose by 58% from 2000 to 2019. Yet crime rates declined dramatically over the same period, with a 43% reduction in violent crime according to the National Crime Victimization Survey. These opposing trends raise an important question: To what extent does firearm demand actually respond to changes in crime?

The goal of this paper is to better understand defensive gun ownership. We begin with a conceptual framework that explores three different perspectives on firearms and self-protection. Under standard expected utility theory, individuals acquire firearms to protect against crime, implying that local demand will respond to changes in crime. A second hypothesis concerns models of behavioral salience wherein demand responds primarily to highly salient crimes even when those events convey little information about the long-run risk of victimization. Finally, the self-defense motive may relate to using guns as a symbolic protection against broader threats to one's sense of control, stemming from uncertainties in one's personal life or societal threats of a chaotic world, rather than crime. Our empirical analysis focuses on the expected utility and crime salience stories, while providing some suggestive evidence on the third account.

To test the expected utility hypothesis, we require a credible source of exogenous variation in long-run local crime that is plausibly unrelated to unobserved determinants of firearm demand. We draw on the quasi-experimental framework of Mello (2019), who demonstrates that federal Community Oriented Policing Services (COPS) grants generate exogenous variation in local police staffing levels, which in turn affect local crime rates. Using the large expansion in COPS grants in 2009 as an instrument for crime, we estimate the causal effect of local crime on gun demand.[1]

Similar to Mello (2019), we find that COPS grant recipients enjoy a statistically significant drop in crime following the award of the grant. However, this decline in crime

[1]We focus on the 2009 COPS expansion, though research on other periods of COPS funding also finds substantial declines in crime (Evans and Owens, 2007; Mello, 2024).

does not translate into changes in local firearm demand or concealed carry permitting. We demonstrate this using multiple measures of firearm demand at different geographic levels: county-level concealed-carry permits, city-level concealed-carry permits, and a city-level proxy constructed from gun dealer locations and background checks. The point estimate from our main specification and data suggests that a 10 percent increase in crime raises gun demand by only 1.3 percent, and this estimate is statistically insignificant. Moreover, we show that this effect is largely driven by the year 2013, a period of heightened gun purchasing following the Sandy Hook Elementary School shooting, indicating that even this small point estimate is driven by political factors rather than by COPS grants or crime.

As an alternative to standard expected utility models, crime salience may affect gun sales. Specifically, we examine whether sudden, massive, and temporary spikes in local crime rates may induce firearm purchases. These spikes may heighten public awareness of crime and prompt purchases by the crime victims. To identify the effects of transitory crime shocks, we use daily city-level data on crime, gun sales, and concealed-carry weapon (CCW) permit applications in Massachusetts from 2009 to 2019. We identify unusually large, temporary increases in crime within towns over two-week intervals and estimate their effects on subsequent firearm demand.

We find that sudden spikes in local crime significantly increase firearm sales during and shortly after the crime wave. However, these spikes are exceptionally large (9 to 11 times the mean crime rate) and rare (less than 0.1% of observations), and their effects are modest. The implied short-run elasticity of gun sales with respect to crime ranges from 0.08 to 0.13, depending on the specification and shock threshold. The increased gun buying is entirely driven by handgun sales, consistent with a self-defense motive. We also find evidence that homicides have a small effect on purchases (4% compared to the mean). However, we find no evidence of changes in applications for CCW permits, potentially reflecting administrative frictions that delay the application process.

Taken together, these results suggest little meaningful effect of crime rates on the demand for firearms. We find causal evidence only in the case of unusually large, salient crime spikes, which explain only a tiny fraction of the variation in gun sales. Given the limited response of firearm demand to local crime, we conclude by examining descriptive evidence on the role of political, cultural, and non-criminal threats in motivating gun ownership.

Increases in national firearm production were highly correlated with lagged crime rates during the twentieth century, but a structural break in the relationship occurred in the late 2000s as the correlation flipped to strongly negative. We discuss the possibility that crime once meaningfully influenced firearm markets, but that its role diminished due to changes in gun culture and politics.

Fear of gun control can also spur firearm purchases, as illustrated by the surge following the Sandy Hook Elementary School shooting in December 2012. Across counties, the size of the Sandy Hook demand surge is unrelated to local crime, but is strongly associated with Republican presidential vote share. Also, counties more severely affected by the Great Recession experienced greater increases in gun demand in 2009, implying that economic insecurity may matter for firearm acquisitions.

Finally, we discuss the large increase in gun purchases during the COVID-19 pandemic, arguing that gun buying may have responded to the large increase in homicides and Black Lives Matter protests in the summer of 2020. This example highlights that crime may have a large role in firearm demand when violence is widespread, highly salient, and accompanied by broader social and political uncertainty. Together, these findings point to the importance of political, economic, and social factors in defensive gun ownership.

The small but positive role of crime salience in gun demand is consistent with prior research. Depew and Swensen (2019) examine the short-run effects of homicides on concealed-carry applications for cities in North Carolina. They find that a homicide increases applications one to two months after the incident, similar to our firearm sales results in Massachusetts. Furthermore, their results are entirely driven by applications in the census tract of the homicide. Although not the primary focus of his paper, Billings (2023) finds that individuals who are recent victims of crime are more likely to apply for permits, but that applications are unrelated to neighborhood crime rates.

Yamaya and Rodden (2025) leverage highly detailed data on California gun purchases. In panel fixed-effects regressions across law enforcement areas and census tracts, they find no clear relationship between firearm sales and measures of violent crime and homicide. This study is unable to directly address endogeneity concerns about the reverse effects of guns on crime. However, they strengthen their results with several descriptive facts pointing to the role of politics and culture rather than crime in gun demand, consistent with our own descriptive findings. We also complement their conclusion by using plausibly exogenous variation in local crime.

The limited role of crime is consistent with recent research on gun markets. Topaz and Johnson (2025) and Johnson and Topaz (2025) use principal component analysis to document the low-dimensional structure of firearm markets, finding that just three components explain almost all variation in gun dealers, concealed carry permits, and gun sales. Moreover, changes across counties over time account for only a small fraction of the variation, with most reflecting persistent differences across counties.[2] Additionally, Johnson et al. (2026) show

[2] "Low Dimensional" refers specifically to something closer to the *share* of gun dealers/concealed carry permits/NICS checks/gun sales relative to the national total in a given year. The correct interpretation is

that fluctuations in gun demand across states are synchronized: the changes in demand for one state are highly predictive of changes in other states.[3] Similar patterns are found in Yamaya and Rodden (2025) across law enforcement agencies in California. Overall, these findings suggest that firearm demand is shaped by persistent geographic differences and common national factors, leaving little role for changes in local crime. We supplement this literature by exploring demand during specific national events such as the Great Recession and Sandy Hook.

Finally, our findings have implications for the literature on the effects of gun ownership on crime, where reverse causality is a longstanding concern. Much of this literature focuses on homicides rather than overall crime rates. This research generally finds that lagged gun ownership is positively associated with more gun homicides, with no relationship for non-gun homicides (Duggan, 2001; Cook and Ludwig, 2006; Studdert et al., 2022; Chalak et al., 2022; Johnson and Robinson, 2024). While some recent research has explored exogenous changes in gun prevalence (Rosenberg, 2025a,b), concerns remain that the positive relationship is driven by self-defense purchases responding to crime. Our estimates focus on overall crime rates and cannot speak directly to self-defense purchases for homicides specifically. However, our results indicate that the reverse causality problem may not be a first-order concern.[4]

Our paper proceeds as follows. Section 2 presents the conceptual framework and reviews the literature on defensive gun ownership. Section 3 estimates the effects of persistent changes in crime on firearm demand using COPS grants. Section 4 explores the role of sudden crime spikes in firearm purchases and concealed-carry permit applications in Massachusetts. Section 5 presents descriptive evidence about the role of politics, culture, and fears of gun control. Section 6 concludes.

2 Conceptual Issues and Prior Literature

Self-defense has become a crucial feature of modern gun culture (Yamane, 2022). Understanding defensive firearm ownership requires addressing two questions: What exactly are

that the bulk of the variation in the short-to-medium-run deviations from a locality's mean is shared across localities.

[3]For example, changes in background checks in Maine over time are strongly correlated with those in California.

[4]We also note that our results are in some ways stronger than they might appear, since the reverse causality concern between homicide and gun buying may itself be weaker than commonly assumed. Homicide victims are deceased and therefore cannot purchase firearms in response to victimization — any gun buying triggered by homicide must operate through third parties. Our analysis avoids this issue entirely by focusing on non-fatal crime, including violent and property offenses, for which survivors can respond behaviorally. That we still find no meaningful response of gun demand to local crime makes our null result, if anything, even more compelling.

gun owners defending against, and how can this be incorporated into an economic model of demand? We provide an overview of these questions below.

2.1 Protection Against Criminal Threats

Under standard expected utility theory, risk-averse individuals are willing to pay a premium to reduce their exposure to risk. If firearms are used to reduce the harms of crime (self-insurance) or to lower the probability of victimization (self-protection), then higher crime risk will increase the demand for guns (Ehrlich and Becker, 1972).[5] A 2024 Gallup survey found that 89% of gun owners reported owning a firearm "for protection against crime" (Gallup, Inc., n.d.b), a finding common in many surveys. Our analysis using COPS grants as an instrument for the effects of long-run changes in crime, however, shows no evidence that crime impacts gun demand.

For a demand response, individuals must perceive the changes in their risk of victimization. Yet subjective perceptions of crime may differ substantially from objective crime conditions. As explained in Section 1, both gun sales and the importance of self-defense have increased over time despite falling crime rates. Moreover, while living in a high-crime area is weakly associated with lower perceived safety (e.g., Alsan et al., 2026), most individuals incorrectly believe crime (nationally and locally) is either increasing or remaining unchanged over time (Gallup, Inc., n.d.a). On the other hand, defensive ownership is associated with higher levels of perceived crime (Kleck et al., 2011; Stroebe et al., 2017). Thus, the perception and salience of crime may matter for gun demand.

Salience diverts attention toward stimuli that are contrasting, surprising, or prominent (Bordalo et al., 2022). Crime salience could arise from particularly heinous crimes (e.g., a school shooting), a town experiencing a sudden crime wave, or media coverage of violent acts. We consider two channels for changes in crime perception. First, prominent crimes may influence salience, even in the absence of changes in underlying crime rates. Second, salience may be shaped by public discourse about crime stemming from news media coverage, political rhetoric, or entertainment independently of actual changes in crime.[6]

Crime is likely more salient to recent victims of crime. Beyond financial losses and physical harm, victimization can produce persistent psychological and emotional harms (Resick,

[5]There is also evidence that the large decline in crime during the 1990s affected economic behavior by increasing housing prices (Pope and Pope, 2012). Similarly, housing prices fall when a registered sex offender moves into a neighborhood (Linden and Rockoff, 2008; Pope, 2008).

[6]These two channels relate to a broader discussion of crime salience in criminology. The "objectivist" account of salience asserts that salience is driven by actual changes in crime rates, whereas the "constructionist" model holds perception of crime is molded by societal rhetoric and messaging about crime (Beckett, 1994).

1987; Norris and Kaniasty, 1994). Consistent a relationship between with victimization and defensive gun demand, Billings (2023) found that applicants for concealed-carry permits are more likely to be recent victims of crime. Serious but uncommon crimes are plausibly more salient. Depew and Swensen (2019) find that a homicide in a municipality increases applications for concealed-carry permits in North Carolina, with effects highest for small towns. In Section 4.4.3, we document a similar finding in Massachusetts.

Large, temporary spikes in crime may have similar effects. Such a shock may increase residents' awareness of local crime through news/social media coverage, word of mouth, and proximity to the event. Under expected utility, a transitory spike in crime is unlikely to prompt costly gun purchases, but the heightened salience may drive some sales. Using data in Massachusetts, we find that when a town experiences a temporary spike in crime over a two-week interval, firearm sales increase.

Alternatively, salience may be shaped by public discourse about crime rather than by changes in criminal activity itself. Using a natural experiment in Italy, Mastrorocco and Minale (2018) find that exposure to media coverage about crime increases concerns about victimization. Baranauskas and Drakulich (2018) show that watching local news and TV crime dramas is associated with beliefs that crime is rising. More directly, recent studies indicate that news media coverage, particularly of violent crime, influences gun acquisition (Kreienkamp et al., 2021; Slote et al., 2025).

2.2 Protection Against Non-Criminal Threats

Defensive gun ownership likely reflects concerns beyond the fear of crime. For example, worries about government overreach color much of the contemporary discourse about firearms, and guns are often seen as "a bulwark against tyranny" among rights activists (Welna, 2013; Seto, 2025). Further, individuals who express lower trust in government are more likely to own firearms (Jiobu and Curry, 2001; Seto, 2025).[7] While we do not directly test the role of anti-government sentiment, we show that Republican-leaning counties are most responsive to threats of future gun control. In addition to worries about the ability to obtain a firearm in the future, this may also reflect political concerns about government overreach.

Protective gun ownership may serve deeper psychological needs beyond criminal safety. When asked how possessing a gun makes them feel, many owners describe feelings of safety, but also control, responsibility, confidence, and being valued and respected (Mencken and Froese, 2019; Alsan et al., 2026). Buttrick (2020) proposes that firearms serve as a coping

[7]This motive can, at least in principle, be incorporated into an expected utility framework, although it is unclear whether expected utility properly accounts for the underlying behavior.

mechanism to symbolically guard against perceived attacks on one's sense of safety, control, and belonging. These psychological threats can stem from a deep-seated "Belief in a Dangerous World" (BDW), a concept first developed by Altemeyer (1988). BDW encapsulates the perception that the world is disordered, chaotic, and filled with evil.[8]

In an influential paper, Stroebe et al. (2017) argue that both the risk of criminal victimization and BDW are key determinants of self-defense–motivated gun ownership: "the need for protection/self-defense is driven by more than just a *specific* threat—the perceived risk of lifetime assault victimization—but also by a *diffuse* threat emanating from the belief that the world is a dangerous and unpredictable place." Using survey data, they measure both respondents' perceived risk of criminal assault and their BDW, finding that both are independently associated with gun ownership.

These broader psychological threats can manifest in specific ways. For example, racial resentment is strongly associated with gun ownership (O'Brien et al., 2013) and opposition to gun control (Filindra and Kaplan, 2016). Racial animus can invoke fears of the dangerous "Other" and shape crime perceptions in terms of the threat of specific groups (Stroud, 2012; Chiricos et al., 2020), reinforcing the belief that the world as a whole is dangerous. Furthermore, guns may serve as psychological protection against perceived threats to one's identity, culture, and social status (Warner and Ratcliff, 2021; Warner and Steidley, 2022). These perceived threats may come from negative attitudes towards the poor, anti-immigrant and anti-minority sentiments, or challenges to traditional views of masculinity.[9]

3 Evidence from the COPS Grants

Expected utility theory predicts that a decrease in long-run crime should lower gun demand. To test this theory, we require an exogenous shift in local crime. In this section, we use increased police funding as a source of variation in crime rates.

3.1 Institutional Background

We exploit a large increase in federal police funding from the Office of Community Oriented Policing Services (COPS). Since 1994, COPS has awarded grants to selected police agencies

[8]Typical BDW scales ask respondents to state their level of agreement with statements such as, "Any day now, chaos and anarchy could erupt around us. All the signs are pointing to it."

[9]As argued in Wright (1995), "Since a 'sense of security' is inherently a psychological trait, it does no good to argue that the sense of security afforded by owning a gun is 'just an illusion.' " Even supposing that firearms lack practical value as a protection against psychological threats, the sense of security they provide may still improve individual welfare as a coping mechanism, assuming the coping mechanism is not maladaptive (Buttrick, 2020).

that partially cover the cost of hiring new officers. Evans and Owens (2007) analyzed the early years of the COPS program and showed that increases in police staffing reduce crime.

Federal funding for COPS declined steadily in the early 2000s, and by 2005 funding was virtually nonexistent. However, in 2009 the program received a $1 billion infusion through the American Recovery and Reinvestment Act. The 2009 grants were more generous than in previous years, covering 100% of the cost of hiring officers for three years. Moreover, each state was guaranteed at least 1.5% of the total funds, ensuring that funds were distributed nationwide.

The grants were designed to support agencies facing high crime rates but lacking the fiscal capacity to fund additional police officers. To apply for a COPS grant, police departments submitted applications with information on local crime rates, fiscal conditions, poverty rates, and unemployment rates. Each department was assigned one score for crime and one for the city's fiscal health. These scores were then added together, and departments above a certain cutoff were awarded funding. The selection process finished in July 2009, and 1,046 agencies received grants. Mello (2019) used an indicator for receipt of a 2009 COPS grant as an instrument to estimate the impacts of increased policing on crime rates, finding that increased police presence substantially reduces crime.

3.2 COPS Data

Our primary measure of gun demand is the number of concealed-carry weapons (CCW) permits issued per 1,000 residents at the county-year level. This data was gathered by Steidley (2021) and, in our sample, covers the period from 2004 to 2014 for eleven states: Colorado, Florida, Iowa, Nevada, New Mexico, North Carolina, Ohio, Oklahoma, Pennsylvania, Texas, and Utah. The data includes all CCW permits issued in a given year, including both new permits and renewals.[10]

For crime data, we use county-level information from the National Archive of Criminal Justice Data. This dataset is constructed from police agencies that report to the Uniform Crime Reporting (UCR) program. For our primary crime measure, we follow Mello (2019) and Autor and Pathak (2017) in computing a social-cost-weighted measure of violent and property crime per 1,000 residents:

$$\text{Cost}_{it} = \frac{\$67,794 \times \text{Violent}_{it} + \$4,064 \times \text{Property}_{it}}{\$67,794 + \$4,064}, \tag{1}$$

[10]Note that we use the number of issued permits rather than the stock of active permits held at a given time, primarily because of data limitations.

where the weights are taken from Cohen and Piquero (2009).

Importantly, not all police agencies report to the UCR program, and some report only for part of the year. This creates a potential problem if the crime rate for an entire county was imputed from a relatively small portion of the population (Maltz and Targonski, 2002). Thankfully, the coverage rates for our sample are high with an average of 98%, and we drop low-coverage counties as a robustness check.

Information on COPS grant applications, raw scores, and award status comes from Mello (2019), who obtained the data through a Freedom of Information Act request to the COPS office. Because grants were awarded to police agencies rather than counties, we construct a continuous county-level measure of treatment intensity. Specifically, we sum the populations covered by grant-recipient agencies within each county and divide by the county's total population. This yields a continuous treatment variable measuring the fraction of a county's population covered by a grant.[11]

We obtain county-level vote shares for the U.S. presidential elections in 2004, 2008, and 2012 from the MIT Election Data and Science Lab. Our main election variable is the average Republican vote share across these three elections, interacted with a linear time trend. Other covariates include the percentages of residents who are Black, Hispanic, or male aged 15 to 29, obtained from the National Institutes of Health's Surveillance, Epidemiology, and End Results (SEER) program. Finally, we use per capita income data from the Bureau of Economic Analysis and unemployment rates from the Bureau of Labor Statistics Local Area Unemployment Statistics program.

Table 1 gives summary statistics for the period 2004–2008, separately for control and treated counties. The differences between counties with and without grants are evident. Treated counties have higher crime rates and unemployment, as expected given the selection criteria for COPS grants, although treated counties have higher incomes. They exhibit demographic differences, including larger populations and higher shares of Black residents. Grant recipient counties also have lower permit rates and, crucially, lower Republican vote shares. If Republican-leaning areas respond more strongly to political events, such as the Sandy Hook shooting, differential trends in firearm demand may arise and bias estimates of the effects of COPS grants on CCW permits.

3.2.1 Alternate Gun Data

We employ two alternate measures of gun demand based on city-level data: a direct measure of CCW permits from two states and a proxy for firearm demand constructed for cities

[11] We also average this ratio over the sample period so that the treatment variable is constant within counties over time.

Table 1: Summary Statistics

	Control		Treated	
Gun and Treatment Variables				
Permits per 1,000	6.749	(7.885)	4.371	(3.404)
Percent Population Treated	0	(0)	0.274	(0.218)
Crime Variables				
Crime Cost per 1,000	3.724	(2.513)	6.553	(2.901)
Violent Crime per 1,000	2.612	(2.154)	4.699	(2.567)
Property Crime per 1,000	22.28	(12.51)	37.47	(13.14)
Percent Coverage	97.95	(7.715)	98.26	(6.185)
Control Variables				
Per Capita Income	30,182	(7,758)	32,855	(8,404)
Unemployment	4.839	(1.459)	5.467	(1.530)
Percent Black	0.0570	(0.0849)	0.140	(0.132)
Percent Hispanic	0.152	(0.190)	0.127	(0.168)
Percent Male Aged 15–29	0.102	(0.0247)	0.103	(0.0188)
Republican Vote Share	0.632	(0.134)	0.517	(0.124)
Population (10,000s)	6.272	(18.14)	32.09	(43.25)
Number of Counties	702		150	

Note: Summary statistics computed over the pre-treatment period 2004-2008. Means are given with standard deviations in parentheses.

nationwide. First, Massachusetts provides detailed firearm sales and permit data through the publicly available Firearms Records Bureau. We use the Firearms License Applications data for the years 2006–2014, which includes the date and city of each CCW permit applicant. Additionally, we use zip code-level data on CCW permits in Minnesota provided by the State of Minnesota.[12] We assign permits to cities by counting the number of permits issued in zip codes that intersect each city's boundaries. Pooling both states, we measure firearm demand as the annual number of CCW permits issued per 1,000 city residents.

For crime data, we use police agency data from UCR. We also construct city-level Republican presidential vote shares using election data from the Office of the Minnesota Secretary of State and the Secretary of the Commonwealth of Massachusetts Elections Division. The other covariates remain at the county level, as discussed in Section 3.2.

We also construct a nationwide proxy by allocating state-level firearm background checks across cities according to the geographic distribution of licensed firearm dealers, inspired by a similar approach in Johnson and Robinson (2024). The locations of gun dealers come from the Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF), which maintains records

[12]These reports, titled "Permit to Carry Report," are published by the Minnesota Department of Public Safety.

on all Federal Firearms License (FFL) holders, including their addresses.[13] Background check data comes from the National Instant Criminal Background Check System (NICS).

To construct this measure, we first count the number of gun dealers located in all zip codes that either intersect a city's border or are adjacent to those zip codes.[14] Let GD_{ist} represent the number of dealers for city i in state s at time t. We then allocate each state's annual NICS background checks to cities in proportion to their dealer counts:

$$\widehat{\text{NICS}}_{ist}^{City} = \frac{\text{GD}_{ist}}{\sum_{j \in s} \text{GD}_{jst}} \text{NICS}_{st}. \tag{2}$$

Finally, we use $\widehat{\text{NICS}}_{ist}^{City}$ per 1,000 residents in all of the intersecting and halo zip codes as our gun demand variable.

Comparing these alternative measures, the Massachusetts and Minnesota permit data provide reliable, direct measures of gun demand, but the sample includes only 19 grant-receiving cities. The gun dealer-background check proxy may induce additional measurement error, but it yields 800 treated cities. These measures offer complementary advantages alongside our primary county-level permit data. Summary statistics, found in Tables A1 and A2, document similar patterns to those in the county permit measure.

As a validation exercise, we compare the dealer-background check measure to the city-level permit data in Massachusetts and Minnesota, and show scatter plots in Figure A3. The dealer proxy is moderately correlated with permits, with correlation coefficients of 0.51 for Massachusetts and 0.34 for Minnesota. This provides evidence that the dealer proxy identifies meaningful variation in gun demand.

3.3 Methodology

3.3.1 Two-way Fixed Effects

We use grant receipt as an instrument for the effect of crime on gun demand. To do so, we estimate the first-stage (effect of grants on crime) and the reduced form (effect of grants on guns) using a two-way fixed effects (TWFE) framework. Both regressions take the following

[13] An FFL is required for "Each person intending to engage in business as an importer or manufacturer of firearms or ammunition," as outlined in 27 CFR 478.41a. The ATF posts FFL data publicly, but only starting in 2014. FFL data prior to 2014 was obtained by a FOIA request in Johnson and Robinson (2024). We restrict the sample to Type 1 and 2 FFLs because these license types primarily conduct retail firearm sales.

[14] Figure A1 provides an illustration of the zip codes for Baltimore, Maryland.

form:

$$y_{it} = \beta \text{Post Grant}_{it} + \alpha \text{Year}_t \cdot \text{GOP}_i + \gamma \text{IA2011}_{it} + X_{it}\delta + \phi_i + \lambda_t + \varepsilon_{it} \tag{3}$$

The outcome y_{it} denotes either the cost-weighted crime or the CCW permit rates, while ϕ_i and λ_t are county and year fixed effects, respectively, and X_{it} are covariates. Post Grant$_{it}$ is our treatment variable based on the average percentage of a county's population that received a grant. Republican presidential vote share interacted with a linear time trend is used to account for differential trends in firearm demand associated with political preferences. We also control for Iowa's adoption of a "shall-issue" concealed-carry policy in 2011 (IA2011$_{it}$), which made permits easier to obtain and generated a sharp but temporary increase in permit issuance, primarily among control counties (Figure A2).

Because COPS grants were awarded on the basis of local crime and fiscal conditions, there is clear selection into treatment status. Since we rely on a parallel trends assumption for the TWFE strategy, this selection does not pose a problem as long as there is no selection on trends. The specific parallel trends assumption requires that, for each level of treatment, the trends in outcomes for that treatment group would have equaled the trends for the untreated group in the absence of treatment. This assumption allows for identification of the conditional (on treatment intensity) average treatment on the treated effect (Callaway et al., 2025).[15] In support of this assumption, we use event-study plots to show that pre-treatment trends are similar for grant-receiving and non-grant-receiving counties. Also, the 2009 COPS application only asked about crime levels for the year 2008, rather than trends.[16]

A potential violation of parallel trends for the reduced-form estimates stems from differential responses to political events. Prior research demonstrates that gun sales increase in response to fears over future gun control, such as the election of President Obama (Depetris-Chauvin, 2015) and the political aftermath of high-profile mass shootings (Brock and Routon, 2020), with responses more concentrated in Republican areas.[17]

Particularly relevant in our case is the Sandy Hook Elementary School shooting in Newtown, Connecticut on 14 December 2012, which left 20 children and 6 adults dead.

[15] Many applications are interested in evaluating the average causal response of increasing the treatment dose by one unit. This requires a (generally) stronger parallel trends assumption to eliminate a selection bias term (Callaway et al., 2025). We do not impose this stronger assumption because (1) we are primarily interested in the effects of crime on guns rather than an evaluation of the COPS program itself; and (2) we also use city-level data where grant receipt is a binary treatment variable.

[16] In fact, most other application questions only asked about 2008. Some budget questions solicited information for the years 2007-2009, and unemployment required both 2008 and 2009 rates. The poverty rate provided was the average for the period 2005-2007.

[17] The passage of gun laws can also spur short-run gun purchases, such as occurred in Oregon immediately following the passage of their 2022 permit-to-purchase law (Bollman et al., 2026).

This tragedy sparked an intense national debate over gun control, especially after President Obama called for stronger gun regulations in the days following the shooting. He went on to propose specific legislation on 16 January 2013 and 12 February 2013, although Congress ultimately voted down the proposed legislation in April. Sandy Hook led to a large increase in gun demand with an estimated 3 million additional guns sold from December 2012 to April 2013 (Levine and McKnight, 2017).

As noted in Section 3.2, counties that received grants generally have lower GOP vote shares and are thus less responsive to political events such as Obama's election and Sandy Hook. Other high-profile shootings occurred during our sample period, such as the 2011 shooting of Arizona Representative Gabby Giffords and the 2012 Aurora theater shooting. A wide range of state-level firearms regulations were also proposed or enacted during this period. These may have contributed to generally higher trends in gun demand in Republican-leaning areas. To address the role of politics in creating differential trends in demand, we interact a linear time trend with the average GOP vote share from the 2004, 2008, and 2012 presidential elections.

3.3.2 Instrumental Variables

Using the grants as an instrument, we estimate the following equation:

$$\text{Guns}_{it} = \beta^{IV}\text{Crime}_{it} + \phi_i^{IV} + \lambda_t^{IV} + \alpha^{IV}\text{Year}_t \cdot \text{GOP}_i + \gamma^{IV}\text{Iowa2011}_{it} + X_{it}\delta^{IV} + \varepsilon_{it}^{IV} \quad (4)$$

Crime_{it} is instrumented by Post Grant_{it}, and all variables are the same as those in the prior section.

The relevance condition requires that the grants affect crime. This was found by Mello, and we confirm a strong first stage in our setting. Other studies find similar reductions in crime during other periods of the COPS program (Evans and Owens, 2007; Mello, 2024), and the broader literature finds that increased policing reduces crime (e.g., Levitt, 1997; Di Tella and Schargrodsky, 2004; Chalfin and McCrary, 2018; Fu and Wolpin, 2018).

The exclusion restriction requires that the grants influence demand only through their effects on crime. Exclusion could be violated if the increased police presence changed gun purchases independently of crime. For example, if observing more police makes civilians feel subjectively safer, then they may decide against buying a firearm. This would make the estimate for β^{IV} more negative, but we find a null effect of crime on guns regardless. Alternatively, a higher police presence may heighten perceived danger, by signaling that an area is unsafe or due to concerns about interactions with law enforcement. This would increase gun demand and could lead to the null effect we observe.

We discuss three lines of evidence to assess how increasing police presence affects feelings of safety. First, surveys generally indicate high public confidence in policing. For example, according to Gallup polling data over our sample period of 2004–2014, a majority of respondents indicated having high confidence in police, with only 8-18% reporting "little to no confidence." Other surveys find similar patterns (e.g., Ekins, 2016). Although confidence varies across demographic groups, including by race, political affiliation, age, and income, favorable assessments of law enforcement generally remain more common than unfavorable ones (Ekins, 2016; Wheelock et al., 2019; Pew Research Center, 2016, 2020).[18] Furthermore, confidence in police is associated with higher feelings of safety (Nofziger and Williams, 2005), suggesting that more police officers will not necessarily decrease perceived safety.

Second, studies of hot spot policing and problem-oriented policing (POP) programs provide direct evidence on the effects of law enforcement on perceived safety. In a meta-analysis of these programs, Hinkle et al. (2020) conclude that, while some findings are mixed, the literature generally finds no effects on perceived safety. Although Hinkle and Weisburd (2008) find that hot spot policing reduced feelings of safety, most studies find either decreased fear of crime or no significant effects (Stone, 1993; Shaw, 1995; Tuffin et al., 2006; Braga and Bond, 2009; Weisburd et al., 2011; Ratcliffe et al., 2015). [19]

One limitation of these studies is that perceived safety is affected both by lower crime and by more police in an area. To isolate the latter, some European studies have explored the effect of police on crime fears experimentally by showing participants images of locations with and without police. In the United Kingdom, Rowland and Coupe (2014) find that law enforcement presence increases feelings of safety. In the Netherlands, Van de Veer et al. (2012) find that police presence reduces perceived safety in locations that otherwise appear safe, particularly among men, but increases perceived safety in locations that appear unsafe. Using a similar design in Sweden, Doyle et al. (2016) find that uniformed police have no impact in apparently safe areas, but reduce fear of crime in places that appear unsafe.

In summary, the literature suggests that the rise in police presence from the COPS grants is unlikely to substantially decrease perceived safety. If anything, additional police presence appears more likely to increase overall perceived safety, although some of the evidence is mixed and may depend on specific conditions. The statistically insignificant effect of crime on guns we estimate is unlikely to be driven by police presence increasing perceived danger.

[18] It is also worth noting that, based on national surveys on reported firearm ownership (Smith and Son, 2015; Azrael et al., 2017) as well as individual purchase and CCW permit data (Studdert et al., 2017; Billings, 2023), these groups tend to have much lower firearm ownership than their counterparts. Thus, if law enforcement were to negatively impact feelings of safety and thereby increase gun purchases, the effect would most likely operate through groups with relatively low baseline gun demand.

[19] A few studies report mixed evidence. For example, Kochel and Weisburd (2017) found that hot spot policing in Missouri led to a slight, temporary increase in fear of crime that disappeared after six months.

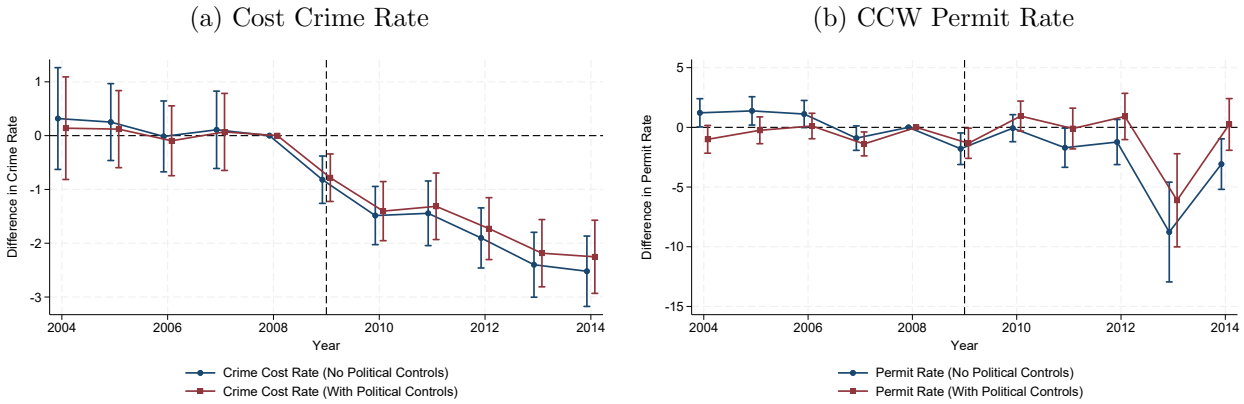
3.3.3 Methodology for the Alternate Data

The preceding discussions and regression specifications generally hold for our alternative data sources, with a few modifications. First, we do not include the dummy for Iowa in 2011. Because the Massachusetts and Minnesota permit data include only 19 treated cities, we also report p-values obtained from wild cluster bootstrapping (Cameron et al., 2008) for the first-stage, reduced-form, and OLS regressions. Finally, because the first-stage relationship is relatively weak in this sample, we also report p-values from the Anderson–Rubin test for the IV estimates.

3.4 COPS Results

We begin with event-study plots for the first-stage and reduced-form effects in Figure 1. To illustrate the importance of politics in our regressions, we display event studies both with and without the GOP times year control variable. Relative to control counties, crime rates remain stable before the grants and then decline substantially following treatment. Including the political covariate shrinks the estimates slightly, but not in an economically meaningful way.

Figure 1: Event Study Plots



Notes: Event study plot variants of Equation (3). We show the plots with and without the average GOP vote share times year covariate. Standard errors clustered at the county level.

Figure 1 shows no clear decline in CCW permits after the grants. Without the political control, the permit rate is consistently just below the pre-treatment levels, but this difference largely disappears once the covariate is included. The remaining differences are concentrated in 2013 after the Sandy Hook Elementary shooting, with a smaller decline in 2009 following the election of President Obama. The sharp decline in 2013 likely reflects differential re-

sponses to Sandy Hook that are not fully captured by the political control, rather than the effects of the grants. Thus, the event study provides little evidence that the grants decreased permit issuance.

Table 2: Main Regressions (County Data)

	Crime Rate	Guns	OLS: Guns	IV: Guns
	(1)	(2)	(3)	(4)
A. No GOP Time Trend				
Crime Rate			−0.0141	1.750***
			(0.0790)	(0.531)
Treat×Post	−1.883***	-3.296***		
	(0.299)	(0.893)		
Elasticity			−0.008	0.996
F-stat	39.787			
B. With GOP Time Trend				
Crime Rate			−0.0984	0.230
			(0.0877)	(0.494)
Treat×Post	−1.626***	−0.374		
	(0.304)	(0.802)		
Year × Pct GOP	0.185***	2.111***	2.149***	2.068***
	(0.0517)	(0.384)	(0.392)	(0.415)
Elasticity			−0.054	0.127
F-Stat	28.710			
Mean Outcome	3.724	6.749	6.749	6.749
Mean Treatment	0.274	0.274	0.274	0.274
Num Counties	852	852	852	852

Note: Column (1) shows the first-stage estimates of COPS on crime, with the Kleibergen-Paap rk Wald F-statistic. Column (2) shows the reduced form estimates of COPS on CCW permits. Column (3) shows OLS estimates of the effect of crime on guns. Column (4) shows the IV estimates of crime on guns using COPS. The reported means of the outcome variables come from the control group prior to treatment. Mean treatment is the average share of the population covered by COPS grants among treated counties. Standard errors in parentheses are clustered at the county level with *** p<0.01, ** p<0.05, * p<0.1.

We now present the main results in Table 2, first without the GOP controls in Panel A and then including them in Panel B. Column (1) gives the first-stage effects of the COPS grants on the cost-weighted crime rate. Both panels indicate large and statistically significant declines in crime following the grants. Because treatment intensity is continuous, we interpret the estimates by multiplying the coefficient by the average treatment intensity among treated counties (0.274). At this average level of treatment, the results in Panel B imply that COPS decreased crime by an average of 0.445 cost-weighted crimes per 1,000 residents, or about

12% relative to the untreated mean.[20] The estimated decline in Panel A is slightly larger than in B, but the difference is not statistically significant (p-value = 0.45). Finally, the relatively large Kleibergen–Paap first-stage F-statistics reduce concerns about weak instruments.

Column (2) shows the reduced-form effects of COPS grants on CCW permits. The difference in results between panels is striking: the estimated effect is nearly nine times larger in magnitude without the political control. At the mean treatment level, Panel B implies a decline of only 0.1 permits per 1,000 (1.5% of the control mean). Further, based on the event-study plot, even this small effect size is primarily driven by the differential response in 2013 rather than by the grants. The GOP time trend is large and significant. Counties with Republican vote shares 10 percentage points higher experienced an additional 0.2 permits per 1,000 residents per year over the sample period.

Columns (3) and (4) report OLS and IV estimates of the effect of crime on firearm demand, respectively. The OLS regressions show no statistically significant relationship between guns and crime. The IV results depend critically on the use of the GOP time trend. Without it, the estimated effect of crime on permit issuance is large, positive, and statistically significant. Including the control reduces the coefficient substantially, and the estimate is no longer significant. The implied elasticity of permits with respect to crime in Panel B is 0.13, meaning that a 10% increase in crime raises demand by 1.3%. As discussed above, even this small estimate is primarily driven by the year 2013. Given this fact and the lack of statistical significance, we find no evidence that crime impacts the demand for firearms.

3.4.1 Robustness Checks

We perform several robustness checks using the county-level data, reporting the results in Table 3 and the event study plots in Figure A4. We include the estimates from the baseline specification (Table 2, Panel B) for reference. Across specifications, the first-stage results in Column (3) are largely unchanged from the baseline, so we focus our discussion on the reduced-form and IV results. The first robustness check uses year dummies interacted with GOP vote share to more flexibly control for differential shocks to gun demand associated with partisanship. The coefficients increase in magnitude but remain fairly small and insignificant.

Our next two checks examine alternative ways of accounting for fixed effects and trends. First, we include state-by-year fixed effects. Both the IV and reduced-form estimates increase

[20] For convenience in interpreting results, we will multiply the coefficient by the treatment level mean for the TWFE regressions. However, as noted in Callaway et al. (2025), this interpretation implicitly uses a negative weighted average. As an alternative, one can simply ignore the continuous nature of our variable and instead use a binary indicator for whether a county received a grant. Doing so gives an estimated effect of -0.491 (p-value<0.001), just above the -0.445 cited above.

Table 3: Robustness Checks

	IV	RF	FS	Elasticity	F-Stat	Counties
	(1)	(2)	(3)	(4)	(5)	(6)
Baseline	0.230	−0.374	−1.626***	0.127	28.710	852
	(0.494)	(0.802)	(0.304)			
Year Dummies × Pct GOP	0.420	−0.684	−1.630***	0.232	26.748	852
	(0.500)	(0.806)	(0.315)			
State-Year FEs	1.161*	−1.647*	−1.755***	0.641	21.152	852
	(0.697)	(0.934)	(0.309)			
Extrapolate Pre-trends	−0.310	0.476	−1.532***	−0.171	29.684	852
	(0.547)	(0.840)	(0.281)			
Drop Low Coverage	0.836	−1.165	−1.392***	0.499	20.531	729
	(0.673)	(0.922)	(0.307)			
Drop Sandy Hook Outliers	−0.131	0.213	−1.631***	−0.072	28.686	846
	(0.384)	(0.625)	(0.304)			
Log(Permit Rate)	−0.121**	0.203**	−1.674***	−0.449	30.671	841
	(0.061)	(0.099)	(0.302)			
Log(Permit Rate) + State-Year FEs	0.075	−0.111	−1.472***	0.280	23.083	841
	(0.059)	(0.081)	(0.306)			

Note: Columns (1)-(3) show the IV, reduced form, and first stage estimates, respectively. Column (4) gives the implied elasticity of permits w.r.t. crime. (5) shows the first stage Kleibergen-Paap rk Wald F-statistics. (6) shows the number of counties. The specifications are the baseline, including year dummies times GOP vote share, including state-by-year fixed effects, including extrapolated county-specific pre-trends for the outcome variables, dropping low UCR coverage counties, dropping counties with a massive permit spike in 2013, using logged permit rates, using logged permit rates and state-year fixed effects. Standard errors in parentheses are clustered at the county level with *** p<0.01, ** p<0.05, * p<0.1.

in magnitude and become marginally significant. However, turning to the event-study plots in Figure A4 reveals that these larger effect sizes are entirely driven by a more pronounced decline in 2013.[21] Thus, these results do not change our interpretation of the main findings. Second, we estimate the county-specific pre-trends in CCW permits and crime, extrapolate these trends into the post-treatment period, and include the resulting variables as controls. This adjustment has little effect on the results, apart from reversing the signs of the reduced-form and IV estimates.

We next examine sensitivity to excluding certain counties from the sample. Because some police agencies do not report crime data to the UCR, county-level crime measures may suffer from incomplete coverage. We drop counties whose coverage rate falls below 70% in any year or whose average coverage rate is less than 90% over the sample period. The

[21] As will be discussed later in Section 5, the inclusion of state-year fixed effects increases the importance of GOP vote share in predicting the response to Sandy Hook. While county GOP vote share is positively correlated with a larger Sandy Hook response, counties in Republican-controlled states are negatively correlated. Thus, including state-year fixed effects increases the estimated difference between Republican- and Democrat-voting areas in their response to Sandy Hook.

first-stage estimate attenuates slightly, while the reduced-form and IV estimates increase in magnitude modestly but remain insignificant. Next, we exclude counties with an unusually large spike in permits in 2013. Specifically, we drop six counties whose 2013 permit rates were at least six times larger than the average of their 2012 and 2014 rates. The reduced-form and IV estimates decrease in magnitude, and their signs reverse.

Finally, we use the log permit rate as the outcome. Using this outcome suggests that COPS grants significantly increased permits. However, the event-study plot reveals substantial differential pre-trends, raising concerns about the parallel trends assumption. Including state-by-year fixed effects flattens the pre-period estimates, and the results are similar to the baseline specification.

3.4.2 Alternate Data Results

We now turn to our two alternative city-level measures. We start with a proxy that allocates state-level background checks across cities according to the number of firearm dealers located in or near each city. Results are presented in Panel A of Table 4 and event-study plots are shown in Figure A5. Column (1) indicates a strong and significant first stage, with crime falling by just over 13% relative to the comparison-group mean. However, the coefficients in columns (2) and (4) in Panel A are small and insignificant. The point estimate implies a 1.6% decline in gun demand and an elasticity of demand with respect to crime of 0.12.

Compared with the county permit data, this proxy offers two main advantages: treatment and outcomes are measured at the same geographic level, and the sample size is much larger. These facts are reflected in the first-stage results, which feature both a large point estimate and a high F-statistic. However, this proxy is an indirect measure of gun demand that relies heavily on gun dealer locations, which may slowly respond to changes in demand. A direct measure of city gun demand would shore up this weakness.

Panel B of Table 4 presents the results using city-level CCW permit data from Massachusetts and Minnesota, with event-study plots shown in Figure A6. Given the small number of treated cities, we also report wild cluster bootstrap p-values for Columns (1)–(3). The first-stage point estimate indicates a substantial decline in crime, although the F-statistic is small. Echoing the previous estimates, we find no evidence that crime influences firearm demand in Column (4).

To illustrate the role of politics in both samples, we report regression estimates without the GOP time-trend in Table A3. The reduced-form and IV results are sensitive to this control: both samples yield positive and statistically significant IV estimates when it is excluded. In the Massachusetts and Minnesota sample, for example, the implied elasticity

Table 4: Regressions for Alternate Data

	Crime Rate	Guns	OLS: Guns	IV: Guns
	(1)	(2)	(3)	(4)
A. Background Checks and Gun Dealer Data				
Crime Cost Rate			−0.0645	0.576
			(0.0441)	(0.572)
Treat×Post	−0.889***	−0.512		
	(0.121)	(0.503)		
Year × Pct GOP	0.151***	6.328***	6.355***	6.241***
	(0.0584)	(0.296)	(0.296)	(0.312)
Elasticity			−0.014	0.121
F-Stat	54.397			
Mean Outcome	6.827	32.365	32.365	32.365
Num Cities	4320	4320	4320	4320
B. MA and MN City CCW Permit Data				
Crime Cost Rate			−0.0429	−0.0476
			(0.0440)	(0.250)
Treat×Post	−0.912**	0.0434		
	(0.450)	(0.226)		
Year×Pct GOP	0.218	2.167***	2.176***	2.177***
	(0.154)	(0.248)	(0.238)	(0.280)
Elasticity			−0.086	−0.096
F-Stat	4.098			
Wild P-value	0.066	0.858	0.359	
AR P-value				0.848
Mean Outcome	4.717	2.345	2.345	2.345
Num Cities	295	295	295	295

Note: Column (1) shows the first-stage estimates of COPS on crime, with the Kleibergen-Paap rk Wald F-statistic. Column (2) shows the reduced form estimates of COPS on gun demand. Column (3) shows OLS estimates of the effect of crime on guns. Column (4) shows the IV estimates of crime on guns using COPS. The reported means of the outcome variables come from the control group prior to treatment. For Panel B, we also report the wild cluster bootstrap p-values for columns (1)–(3) and the Anderson-Rubin p-value for column (4). Standard errors in parentheses are clustered at the city-level with *** p<0.01, ** p<0.05, * p<0.1.

falls from 2.1 without the political control to −0.1 when it is included. Overall, the results from both alternative measures of firearm demand are qualitatively consistent with our main county-level findings.

4 Short-Run Crime Spikes

We now consider whether sudden increases in local crime rates generate short-run changes in gun demand.[22] Under expected utility theory, temporary increases in crime should have little effect on firearm purchases unless they signal a persistent change in the risk of victimization. It is not clear that consumers with rational expectations would interpret temporary fluctuations as informative about long-run trends or, even if they did, that the belief update would be strong enough to induce purchases of a relatively expensive good. Thus, if a crime shock increases gun sales, it would likely do so through its effects on crime salience.

There are two broad mechanisms through which a crime spike could raise the demand for firearms. First, an unusually large number of offenses may increase the salience of crime through news coverage or word-of-mouth discussions, raising public fears about crime and prompting self-defense purchases. Alternatively, a crime shock may induce purchases among victims themselves, even without affecting perceptions of crime among the broader public. Under the first mechanism, public sentiment and salience are drivers of demand rather than crime itself. The second mechanism implies that any change in crime rates will alter purchases, but the effect on aggregate demand may be too small to detect without large spikes.

4.1 Crime Spikes Data

To estimate these short-run local effects, we require detailed data on both gun purchases and crime. Massachusetts provides both. We obtain transaction-level firearm sales data from the Massachusetts Firearms Records Bureau (FRB), covering the universe of firearms sold by licensed dealers in the state. These records include the buyer's city and zip code, the purchase date, and the type of firearm. We supplement these data with FRB records of concealed-carry permit applications, which include the application date and the applicant's location. Our sample spans 2009–2019.

For crime data, we use incident-level records from the National Incident-Based Reporting System (NIBRS) for Massachusetts. NIBRS provides detailed information on individual

[22] We are deeply grateful to Alex Whitefield for providing an idea that ultimately developed into this section of the paper.

crime incidents, including the occurrence date, the offenses committed, and the number of victims. Because not all agencies report to NIBRS, our sample includes 254 of the 351 municipalities in Massachusetts.[23]

Municipalities are the primary geographic unit in our analysis. Geographic units must be small enough to capture the effects of local crime, a condition that many Massachusetts towns satisfy given their relatively small populations and land areas. [24] We aggregate the data into two-week intervals to estimate short-run effects.

We define a crime spike as a large, sudden, and temporary deviation from a town's expected crime rate. To estimate expected crime, we regress the crime rate in municipality i at time t on lagged crime rates, time fixed effects, and municipality fixed effects:

$$\text{Crime}_{it} = \sum_{l=1}^{L} \rho_l \text{Crime}_{it-l} + \lambda_t + \phi_i + \varepsilon_{it}. \tag{5}$$

Here, L denotes the number of lags included in the regressions, which we set $L = 6$ because this value maximizes the adjusted R^2 of Equation (5).[25] We calculate the residuals from this regression, $\hat{u}_{it}^{\text{crime}} = \text{Crime}_{it} - \widehat{\text{Crime}}_{it}$, and classify a crime spike in town i at time t as a period where $\hat{u}_{it}^{\text{crime}} > S$. The threshold S represents the minimum number of additional crimes per 10,000 residents compared to the expected crime rate. If crime spikes induce gun buying, then higher thresholds should yield larger point estimates, although standard errors may also increase as the sample of spikes falls.

Table 5 presents summary statistics for a spike threshold of $S = 40$. Compared to other municipalities in our sample, towns that experienced a shock tend to have lower crime rates and higher gun demand. They also tend to be smaller, poorer, more rural, and more White. One point to note is the relatively high standard deviations for the firearm variables. This likely reflects small population sizes and could mean our results are driven by noise. We address this possibility using placebo tests, as discussed below.

Table 5 also reports the average crime rate during the spike period. The cutoff value of $S = 40$ implies at least 40 additional crimes per 10,000 residents compared to what is predicted by Equation (5). With an average crime rate of 69.4, these spikes are about 8.0 times the average crime rate for treated municipalities and correspond to an additional 41.2 crime victims on average.

[23] We drop the particularly small town of Aquinnah, which only has around 300 residents.

[24] However, focusing solely on individual municipalities may understate the effects of a crime spike if nearby communities are also affected. Accordingly, we also examine responses to crime shocks in neighboring municipalities.

[25] The results are nearly identical when choosing other values for L. Additionally, the findings remain unchanged when using deviations from a simple rolling average.

Table 5: Summary Statistics (S=40)

	Data	**Treated**		**Comparison**	
	Source	Mean	Sd	Mean	Sd
Crime Rate	NIBRS	7.70	(9.16)	7.13	(5.99)
Sale Rate	FRB	9.54	(8.83)	6.65	(5.69)
Permit Rate	FRB	7.25	(6.57)	4.81	(4.76)
Population (1,000s)	NIBRS	6.68	(4.24)	24.79	(24.83)
Percent Rural	Census	0.48	(0.34)	0.15	(0.23)
Percent White	Census	0.90	(0.05)	0.83	(0.13)
Percent Male 25-64	Census	0.25	(0.03)	0.25	(0.03)
Percent GOP	CMED	0.38	(0.12)	0.40	(0.10)
Median HH Income (1,000s)	ACS	82.76	(25.25)	88.76	(28.67)
Shock Size		69.36	(19.63)		
Number of Cities		40		213	

Note: Statistics for NIBRS and FRB data are for the period 2009–2019. Census data is from 2010 and 2020, ACS from 2010 and 2019, and election data is from the 2012 and 2016 presidential elections.

4.2 Methodology

Our main method to estimate the effects of crime shocks is a standard two-way fixed effects approach with time (λ_t) and place (ϕ_i) fixed effects.

$$\text{Gun}_{it} = \alpha + \delta \text{Post Spike}_{it} + \lambda_t + \phi_i + \varepsilon_{it} \tag{6}$$

When a shock occurs in municipality i at time t, Post Spike$_{it}$ will equal 1 for periods t to $t+3$. However, because municipalities may experience multiple overlapping spikes, Post Spike$_{it}$ is not a binary variable. Instead, it counts the number of crime spikes whose treatment windows include a given period. For example, if a spike occurs at period t and $t+2$, then Post Spike$_{it} = 2$ during the periods $t+2$ and $t+3$.[26]

In our main specification, we estimate Equation (6) using all municipalities in our sample as the comparison group. We also consider two other subsamples. First, we only include towns that experience a shock during the sample period, so that the comparison group consists only of ever-treated towns. Second, we construct a nearest-neighbor comparison group by matching on the characteristics described below. [27]

Given the high-frequency data and small towns in our sample, our results may be driven

[26] As a robustness check, we estimate the regression in which Post Spike$_{it}$ is redefined as a binary indicator even in the presence of overlapping shocks. This increases the point estimate, as expected if spikes increase gun demand.

[27] We will also examine the possibility of spillovers to nearby municipalities, which could violate SUTVA.

by random fluctuations. To assess this concern, we conduct 10,000 placebo tests by randomly assigning crime spike dates in the treated sample and re-estimating the regression on each placebo sample.[28] If large point estimates are rare in the placebo distribution, then our main results are unlikely to be caused by random noise in demand.

One disadvantage of the approach above is that focusing on narrowly defined crime spikes exploits only a tiny fraction of the overall variation in crime rates. To use the full variation in crime, we nonparametrically estimate the short-run relationship between deviations from expected crime and firearms. We begin by separately regressing crime rates, firearm sales rates, and permit rates on time and municipality-year-quarter fixed effects. We then obtain the residuals from each regression:

$$\mathrm{y}_{it} = \phi_i \times \gamma_y \times \tau_q + \lambda_t + \varepsilon_{it} \Longrightarrow \hat{\mu}^y_{it} = y_{it} - \hat{y}_{it}, \tag{7}$$

where y_{it} is either the crime, sale, or permit rate.[29]

With these residuals, we nonparametrically estimate how unexpected deviations in crime impact subsequent gun demand. Specifically, we estimate

$$\sum_{s=0}^{s=3} \hat{\mu}^g_{it+s} = m_g(\hat{\mu}^c_{it}) + \varepsilon^g_{it}, \ \sum_{s=0}^{s=3} \hat{\mu}^p_{it+s} = m_p(\hat{\mu}^c_{it}) + \varepsilon^p_{it}, \tag{8}$$

where g, p, and c represent guns, permits, and crime, respectively, while m_g and m_p are the functions to be estimated. To capture cumulative responses over time, we use the sum of residual gun demand from t to $t+3$. We estimate these functions with a local polynomial regression and bootstrap the standard errors to account for the uncertainty introduced by using estimated residuals.

4.3 Main Crime Spike Results

4.3.1 Two-way Fixed Effects

We first present event-study plots for crime, gun sales, and CCW permits for a shock threshold value of $S = 40$ in Figure 2, while event-study plots for other thresholds are found in Figure A7. As expected, the crime rate starts off flat, spikes at the time of the event, and then immediately falls. The average spike corresponds to over 59 additional crimes per

[28] We exclude dates in the treatment window when assigning the placebo dates.

[29] Unlike Equation (5), which we used to identify spikes, the above regression does not include lagged terms. If we included lagged terms, then the periods immediately following a surge in crime would have large negative residuals even if the crime rate is not low. In other words, we would mistakenly characterize some normal or even elevated crime periods as low-crime deviations.

10,000 residents relative to the baseline level. Crime rates are somewhat elevated in the next period but quickly return to their pre-shock levels.

Gun sales substantially increase during the spike before gradually declining toward the pre-spike baseline. Although the confidence intervals are wide, the point estimates suggest that sales remain elevated for several weeks after the shock. In contrast, CCW permit applications exhibit no clear response to crime spikes.

Table 6 gives regression estimates across shock thresholds. Beginning with firearm sales results in Columns (1)–(4), we find statistically significant evidence that gun sales increase after the shock for the largest thresholds. At $S = 40$, Column (3) indicates an average of 1.3 more guns sold per 10,000 residents in each period following the shock, an 18% increase relative to the mean. The effect at $S = 50$ is even larger, as shown in Column (4). Interestingly, the implied elasticity of firearm sales with respect to crime also increases from 0.09 to 0.12 across these thresholds, suggesting a potentially nonlinear response, although the difference is not statistically significant. [30] Finally, the relatively small placebo-based p-values in Columns (3) and (4) indicate that these effects are unlikely to arise from random noise.

Table 6: Regression Results for Crime Spikes

	Gun Sales				**CCW Permits**			
	S=20	S=30	S=40	S=50	S=20	S=30	S=40	S=50
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Shock	-0.082	0.314	1.266***	2.255**	0.041	-0.307*	-0.480	-0.747
	(0.201)	(0.276)	(0.482)	(0.873)	(0.153)	(0.175)	(0.330)	(0.651)
Placebo P-Value	[0.583]	[0.377]	[0.021]	[0.057]	[0.492]	[0.299]	[0.379]	[0.409]
Elasticity	-0.009	0.027	0.086	0.123	0.006	-0.035	-0.044	-0.056
Outcome Mean	7.088	7.097	7.097	7.099	5.167	5.190	5.192	5.194
Crime Mean	6.977	7.091	7.148	7.183	6.977	7.091	7.148	7.183
Spike Size	34.14	47.42	59.52	74.37	34.14	47.42	59.52	74.37
Num Shocks	350	133	63	29	350	133	63	29

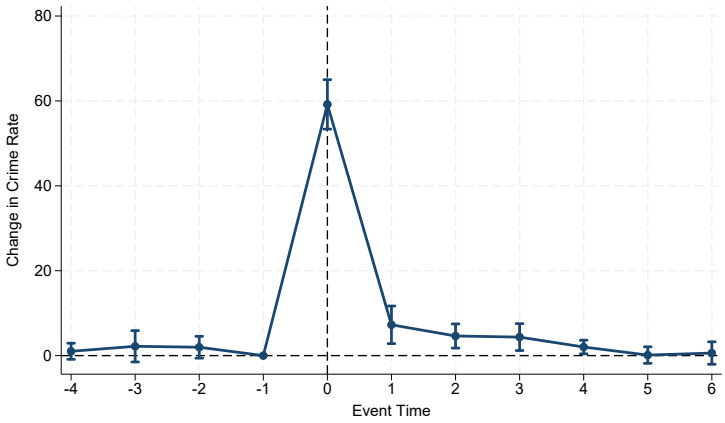
Note: Regression estimates for Equation (6). Columns use shock thresholds of either $S = 20, 30, 40$, or 50. Outcome means come from the comparison group. The elasticity is for the outcome variable with respect to the crime shock. Standard errors are clustered at the town level, with *** p<0.01, ** p<0.05, * p<0.1.

The results for CCW permits, presented in Columns (5)–(8), provide no evidence that crime shocks increase applications, with most estimates being negative. One explanation is that applicants must first complete a basic firearms safety course, which may delay their

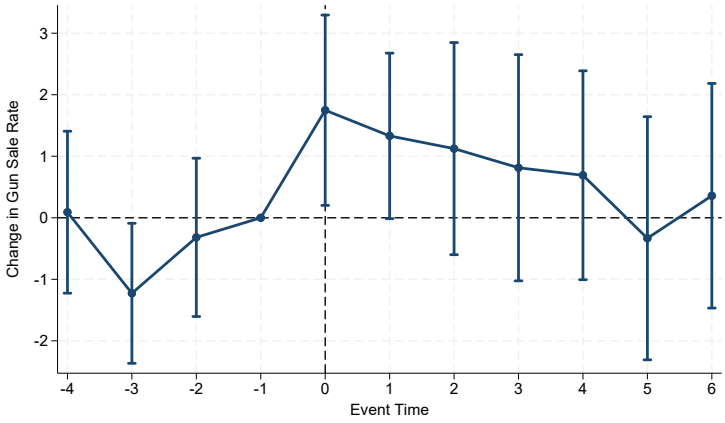
[30] Specifically, we calculate the elasticity by dividing the coefficient $\hat{\delta}$ by the mean of the outcome variable and multiplying by the ratio of the average pre-shock crime rate to the average crime rate at event time 0.

Figure 2: Event Study Plots for MA Shocks (S=40)

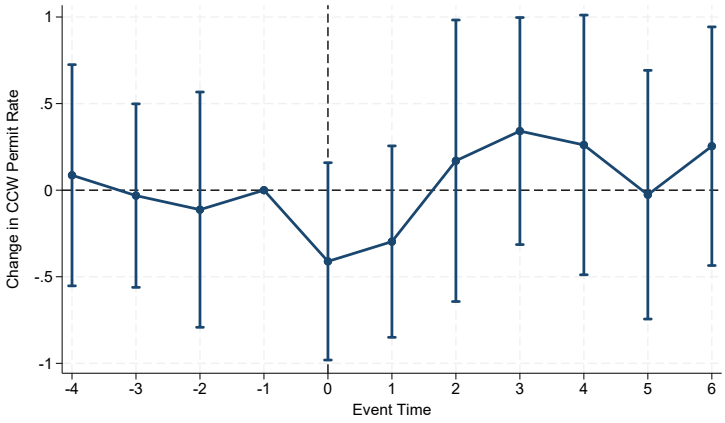
(a) Crime Rates



(b) Gun Sales



(c) CCW Permits



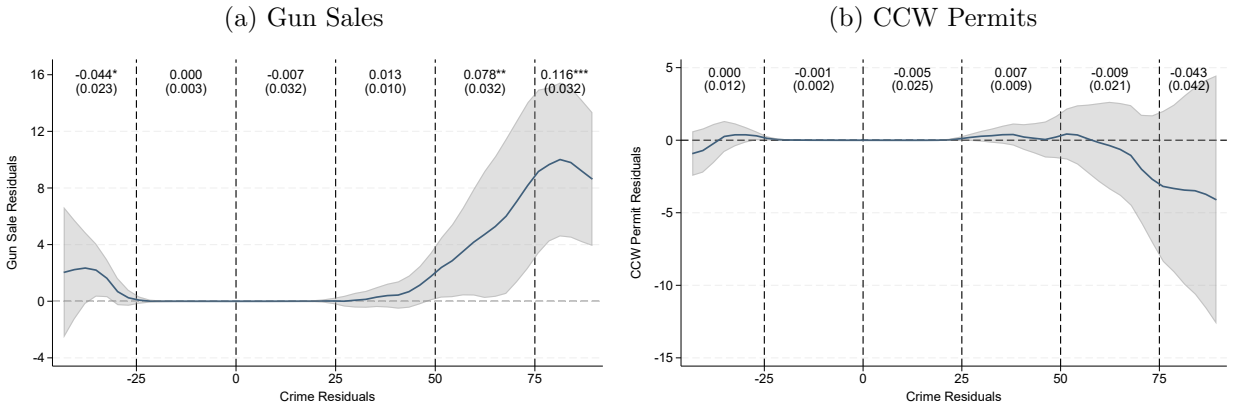
Notes: Event study plot variants of Equation (6) for crime rates, gun sales, and CCW permits at a shock cutoff level of $S = 40$.

response to crime spikes. These courses typically last several hours, and as of March 2026, only 26 locations offered such courses, most of which held just one session per month.[31]

4.3.2 Nonparametric Regressions

Our preceding analysis identifies crime shocks using discrete thresholds. We now use local polynomial regression to nonparametrically estimate the relationship between deviations from expected crime and cumulative residuals in gun demand. The results are displayed in Figure 3. In Panel (a), gun sales increase when crime is unexpectedly high. At the estimated peak, around 10 additional guns per 10,000 residents are sold. These effects diminish with smaller crime deviations, and the relationship is nearly flat for residuals between -25 and 25, the range where 99.8% of all observations fall. Sales are slightly elevated on the opposite end of the distribution, although the estimates are generally insignificant.

Figure 3: Nonparametric Regressions



Notes: Estimates of Equation (8) with a bandwidth of 10. Standard errors are bootstrapped using 399 replications.

To facilitate comparison with the TWFE estimates, we also calculate average short-run elasticities over different ranges of the crime residuals. Specifically, at each of the 50 grid points used in the local polynomial regression, we divide the estimated effect by the corresponding crime residual and scale the result using the mean levels of sales and crime. We then average these elasticities over several intervals (e.g., -50 to -25 and -25 to 0) and report the corresponding bootstrapped standard errors. For large positive deviations, the elasticities range between 0.08 and 0.12, similar to the TWFE estimates. Thus, both

[31] Another possibility is that crime spikes affect only the intensive margin of gun demand, meaning that no impacts on CCW permits could be found because an individual can only have a single permit whereas people can own multiple firearms. Unfortunately, our data does not allow us to identify first-time gun buyers, preventing us from distinguishing between extensive- and intensive-margin responses.

approaches suggest that unusually large increases in crime lead to moderate increases in firearm sales.

We find no evidence of changes in CCW permits, however. Although the relationship is negative at the extreme right tail of the distribution, it never becomes significant. The average elasticity is very small, -0.04, and is statistically indistinguishable from zero.

4.4 Robustness Checks and Other Results

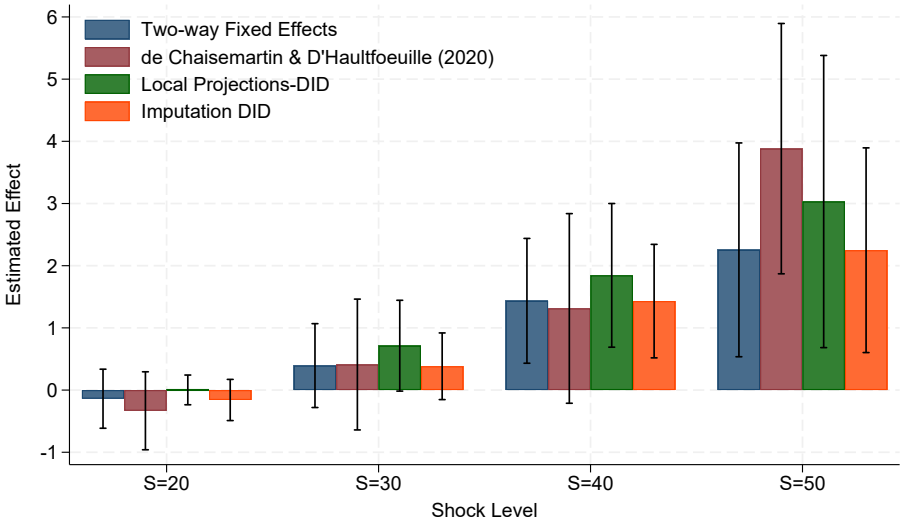
4.4.1 Alternate Comparison Groups and Estimators

Our main sample consists of all municipalities in our data. As a robustness check, Figure A8 reports the point estimates using the baseline sample (all municipalities), only towns that experience a spike, and treated towns combined with a nearest-neighbors sample created by propensity score matching on the characteristics shown in Table 5. The effect sizes are very similar across control groups, with the major differences being the width of the confidence intervals. The estimate is statistically significant for the nearest neighbor group at $S = 30$, although the coefficient for the treated-only sample is insignificant at $S = 50$. Overall, our findings are robust to the choice of comparison group.

Recent literature has developed alternatives to TWFE estimators that address potential biases arising from staggered treatment timing and heterogeneous treatment effects (see Baker et al., 2026, for a review). Many of these estimators work when treatment is absorbing, meaning that treated units remain treated throughout the sample period, making them inapplicable in our setting where treatment periods are brief. However, we can use three estimators that accommodate treatment turning on and off: the instantaneous treatment effect estimator of De Chaisemartin and d'Haultfoeuille (2020), the local projections estimator of Dube et al. (2025), and the imputation estimator of Borusyak et al. (2024).

The results from these estimators, along with TWFE, are given in Figure 4. The estimated increases in gun sales are similar to, if not larger than, those obtained using TWFE. Two results are particularly noteworthy. While TWFE finds no effect at a shock threshold of $S = 30$, the local projections estimator yields a marginally significant 10% increase in sales. The estimator of De Chaisemartin and d'Haultfoeuille (2020) produces the largest estimate at $S = 50$, indicating a 55% rise in sales. Permits, however, continue to lack any discernible response across estimators.

Figure 4: Effects of Crime Shocks Across Estimators (Gun Sales)



Notes: Estimates obtained using TWFE, De Chaisemartin and d'Haultfoeuille (2020), Dube et al. (2025), and Borusyak et al. (2024). Standard errors for the imputation method are bootstrapped with 399 replications.

4.4.2 Spillovers

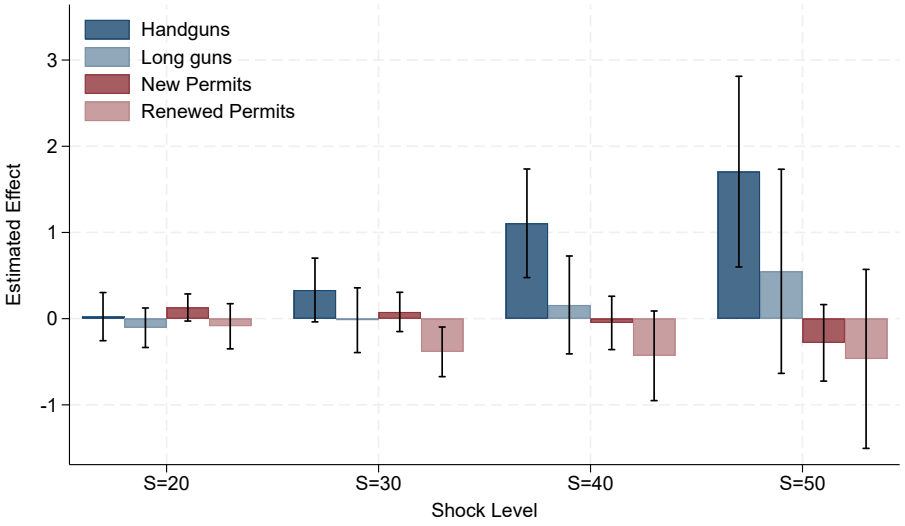
We next examine whether crime spikes affect firearm demand in neighboring municipalities. Focusing on thresholds of $S = 40, 50$, we estimate effects using the TWFE specification in Equation (6). Recall that our main sample does not include all municipalities in Massachusetts because not all police agencies report to NIBRS. However, the FRB has transactions throughout the state, allowing us to use the full set of bordering municipalities.

While our primary interest is in sales and permit applications, we first note that we find no changes in crime rates in neighboring towns. At a threshold of $S = 40$, the point estimate for neighboring crime is 0.40 (p-value = 0.34), with similar results at $S = 50$. We report estimates for firearm sales in Table A4. Column (1) uses cities in the NIBRS data for consistency with the main sample. Column (2) includes all towns from the FRB data. In both columns and panels, the effects are near zero and insignificant.

Crime spikes may be more salient for bordering towns that are relatively small or geographically near the shock. Column (3) restricts the FRB sample to neighboring municipalities with populations below the median, while Column (4) restricts the sample to those within the median distance between municipal centroids.[32] There is no evidence of increased purchases in these small, neighboring towns. Finally, we replicate these analyses using CCW

[32]The median population is about 6,650 residents for Panel A and 7,700 for Panel B. The median distance is about 6 miles for both panels. Distance is calculated using municipal centroids.

Figure 5: Regression Estimates by Gun and Permit Type



Notes: Estimates of Equation (6) split between handgun and long gun sales, and new and renewed permits.

permits as the outcome and report the results in Table A5. We again find small and insignificant point estimates.

4.4.3 Heterogeneity in Responses to Crime Spikes

We next examine heterogeneity, starting with gun and permit type. Figure 5 shows that the increases in firearm sales are entirely driven by handguns, and the estimate at $S = 30$ is now marginally significant for handguns. The effects on handgun purchases are consistent with a self-defense motive. Turning to CCW permits, we would expect crime spikes to affect new applications rather than renewals. However, the point estimates for new permits are very close to zero. Also, the negative coefficients for permits reported in Table 6 are primarily driven by renewals.

Another source of heterogeneity is the type of crime. The crime spikes analyzed thus far are driven almost entirely by property crimes. While the violent crime rate is somewhat higher during these spike periods (2.3 versus 1.3 at baseline), approximately 97% of the crimes during the spikes are property crimes.[33] Plausibly, however, violent crimes are more salient and may generate greater public fear. To test this possibility, we repeat our TWFE analysis using periods with unusually high violent crime.

Table A6 gives the results. The threshold values are much lower than those used for

[33]This largely reflects the greater prevalence of property crime, which occurs approximately seven to eight times as frequently as violent crime.

overall crime, but the spikes are similarly rare and are several times the average level of violent crime. We find no evidence that sales or permit applications increase in response to periods of unusually high violent crime. Indeed, most coefficients are negative, only one estimate is statistically significant at the 5% level, and all imply elasticities close to zero. These results do not mean that violent crime is less salient than property crime. All else equal, an event with 100 violent crimes may spur more gun buying than one with 100 property crimes. However, violent-crime spikes of this magnitude are not observed in our data.

Homicides may be particularly salient to local residents. We now repeat the analysis using an indicator for whether a homicide occurred during a given period. Because homicides are likely more salient in smaller towns, we estimate effects separately for towns above and below the median population in Table 7. We find significant increases in firearm sales following homicides, with larger effects in smaller towns. Depew and Swensen (2019) observe similar patterns using CCW permits in North Carolina. Although we don't observe a statistically significant increase in permits (Table A7), our results on firearm sales also suggest that demand is spurred by local homicides.

Table 7: Effects of Homicides on Gun Sales

	Below Median Pop	Above Median Pop	All Towns
	(1)	(2)	(3)
Post	0.911**	0.158***	0.293***
	(0.364)	(0.050)	(0.071)
Outcome Mean	9.127	5.163	7.209
Num Homicide Events	77	858	935

Note: Regression estimates for Equation (6) using homicides as the crime shock. The median population threshold is calculated using each town's average population over the period 2009–2019. Standard errors are clustered at the town level with *** p<0.01, ** p<0.05, * p<0.1.

5 Politics, Gun Control, and Crime

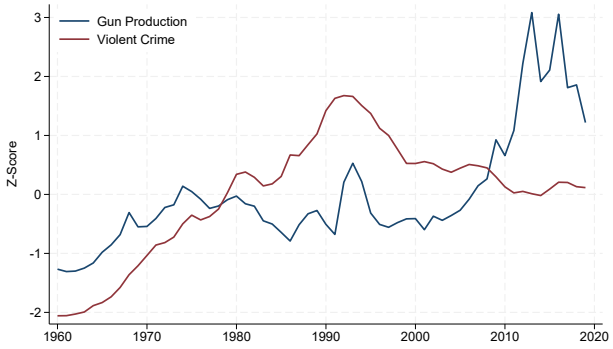
So far, we have found evidence that crime plays only a limited role in gun demand. This raises a broader question: What other factors drive firearm acquisition? We explore three sources of evidence based on long-run trends in gun production and crime, large-scale shocks associated with heightened fears of gun control and economic uncertainty, and the surge in gun buying during the COVID-19 pandemic. While the following analyses are descriptive, they highlight potentially important channels of firearm demand and identify directions for future research.

5.1 Guns and Crime in the Long-run

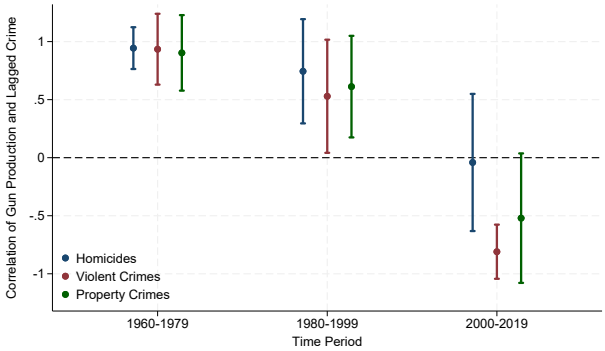
We began this paper by noting that firearm production has increased substantially over recent decades despite large declines in crime. Historically, however, these trends have not always diverged. Figure 6a illustrates this relationship by plotting national gun production and violent crimes reported in the UCR. While recent decades have seen an explosion in gun production accompanied by a steady decline in crime, the two series followed similar trajectories prior to the 2000s.

Figure 6: Guns and Crime Trends (1960-2019)

(a) Time Series Plots



(b) Correlation Coefficients



Notes: Part (a) shows Z-scores of gun production and violent crime from 1960-2019. Part (b) gives correlation coefficients between gun production and lagged homicides, violent crime, and property crime. Confidence intervals were created using Newey-West standard errors.

Figure 6b makes this point more formally using homicide data from the CDC, and violent and property crimes data from the UCR. We split the sample into three periods (1960–1979, 1980–1999, and 2000–2019) and compute the correlation between guns and each lagged measure of crime. In the earlier periods, the correlation was extremely high (ranging from 0.90 to 0.94), and remained high during the 1980s and 1990s (between 0.53 and 0.74). After 2000, the correlation with homicides fell to nearly zero, whereas it became strongly negative for the other crime types.[34] Testing for structural breaks, we find breaks at around 2007 for violent crime, and 2009 for homicides and property crime.[35]

Although firm conclusions cannot be drawn from this simple time series analysis, it raises the possibility that crime historically influenced gun sales. This idea is supported by Duggan (2001), who found a positive relationship between county gun sales and lagged

[34] One reason why the correlation with homicides may be zero rather than negative is if the general decline in crime decreased homicides, but the surge in gun purchases increased homicides. Indeed, splitting by type of homicide, the correlation with gun homicides is slightly positive (0.27) and negative for non-gun homicides (-0.65).

[35] The Wald test statistics all exceed 200 and have p-values nearly equal to zero.

homicide rates over the period 1982 to 1998. If crime previously played a greater role in firearm demand, what explains the apparent change in this relationship around 2009? We focus on two hypotheses: changes in American gun culture and changes in the news media over time.[36]

While self-defensive gun ownership has always been deeply rooted in the United States, gun culture throughout the early twentieth century was primarily oriented toward hunting and sporting rather than personal protection (Hofstadter, 1970). During the 1960s, amid political unrest, riots, and assassinations, gun buying surged as a response to concerns about crime (Newton and Zimring, 1969), marking a shift towards a self-defense-based gun culture.

Political elements also influenced the new culture. The National Rifle Association (NRA) was originally focused on promoting competitive shooting and hunting. While generally opposed to gun control, the organization was willing to propose compromises with lawmakers seeking to regulate firearms. This changed after the 1977 "Revolt at Cincinnati," when the NRA's relatively apolitical leadership was replaced by a faction more focused on gun rights, "effectively transforming the NRA into the primary defender of the Second Amendment" (Melzer, 2009).

The growing focus on self-defense can also be seen in other ways. Firearm advertisements increasingly emphasize self-protection (Yamane et al., 2018, 2020), handgun purchases and rates of concealed carry have risen, and more people give defense as a major reason to own a gun. Although defensive gun ownership may have initially increased in response to substantial fears of crime and political unrest, it has continued to increase over the last 25 years despite large declines in crime.

Changes in the news media may have further contributed to the disconnect between perceived and actual crime rates. Over recent decades, news consumption has shifted from local to national sources (Hopkins, 2018; Angelucci et al., 2024), partly reflecting a decline in local news coverage (Greene et al., 2024). Even surviving local news sources have sometimes increased their coverage of national topics (Martin and McCrain, 2019). Meanwhile, sensationalism in news coverage has steadily increased since the 1970s (Slattery and Hakanen, 1994; Slattery et al., 2001; Rozado et al., 2022). These trends may have led to distorted perceptions of crime, contributing to the increase in self-defensive gun ownership despite decreases in crime.

[36] A detailed discussion of the history of guns in the United States is beyond the scope of this work. For more discussion of the gun culture and its history, see the articles by Yamane (2022) and Kopel (2011).

5.2 Politics and Fears of Gun Control

The preceding discussion of changes in gun culture and the news may help explain the long-run shift in the relationship between guns and crime. However, these gradual developments do not fully explain the structural breaks observed around 2009. In this section, we focus on fears of gun control during the presidency of Barack Obama and discuss the role of the Great Recession.

5.2.1 Sandy Hook and the Fear of Future Gun Control

As explained earlier, gun purchases and permit issuances increased in the wake of the Sandy Hook shooting. This demand shock was caused by fears over future gun control (Levine and McKnight, 2017), and the magnitude of the response varied across counties. We measure the county-level percentage increase in 2013 relative to 2012 and 2014 as a proxy for the response to Sandy Hook.

$$\text{Sandy Hook}_i = \frac{\text{Gun}_{i,2013}}{(\text{Gun}_{i,2012} + \text{Gun}_{i,2014})/2} - 1. \tag{9}$$

Figure A10 plots the distribution of Sandy Hook$_i$, dropping six outlier counties with a percentage increase exceeding 700%. Nearly every county issued more CCW permits in 2013, with an average increase of 51% relative to 2012 and 2014. The magnitude of this response varied considerably across counties, with a standard deviation of 44 percentage points. We investigate the role of politics and crime in explaining this variation. If crime is a determinant of gun demand and concerns about future gun control accelerate firearm acquisition, then the Sandy Hook proxy should be positively associated with crime.

We test this hypothesis with the following regression:

$$\text{Sandy Hook}_i = \delta_1 \text{Pct GOP}_{i,2012} + \delta_2 \text{Pct Rural}_{i,2010} + \delta_3 \overline{\text{Crime}}_i + \overline{X}_i \beta + \varepsilon_i. \tag{10}$$

We measure crime rates and other county characteristics using averages from 2004 to 2012. We also include GOP vote share from the 2012 election and the percentage rural in the 2010 Census. All explanatory variables are standardized to aid in comparing effect sizes.

Table 8 presents the results. Column (1) only includes covariates, Column (2) adds an indicator for unified Republican control of the state government, and Column (3) uses state dummies to account for the possible impact of state laws on gun demand after Sandy Hook (see Koenig and Schindler, 2023). Across the specifications, the coefficient on crime rate is small and insignificant, while the estimate for Republican vote share is always large and

Table 8: Sandy Hook Proxy Regressions

	(1)	(2)	(3)	(4)	(5)
Pct GOP 2012	0.052***	0.105***	0.121***	0.122***	0.120***
	(0.017)	(0.021)	(0.027)	(0.027)	(0.027)
Pct Rural	0.013	0.004	0.040*	0.046**	0.039**
	(0.021)	(0.021)	(0.022)	(0.020)	(0.019)
Crime Rate	−0.010	−0.006	−0.005		
	(0.019)	(0.019)	(0.020)		
GOP Govt		−0.211***			
		(0.054)			
Crime Rate 2012				0.007	
				(0.018)	
Crime Trend 2012					0.011
					(0.011)
Covariates	X	X	X	X	X
State FEs			X	X	X
Mean	0.506	0.506	0.506	0.506	0.506
R2	0.092	0.117	0.303	0.304	0.304
N	846	846	846	846	846

Note: Regression estimates for Equation (10). All explanatory variables are standardized, except the indicator for GOP control of the state government in column (2). Columns (3)–(5) include state dummies. Column (4) uses the 2012 crime rate. Column (5) uses the trend in crime from 2004 to 2012. Standard errors are robust to heteroskedasticity with *** p<0.01, ** p<0.05, * p<0.1.

significant. In Column (3), a one-standard-deviation increase in GOP vote share is associated with a 12-percentage-point (24%) larger increase in CCW permits. Column (2) also indicates that counties in states with unified Republican governments exhibited smaller responses to Sandy Hook, potentially reflecting lower concerns about future state-level gun restrictions. However, given that our sample includes only 11 states, this finding should be interpreted cautiously. Finally, rural counties had a stronger Sandy Hook response, although the point estimates are small.

Column (4) replaces the long-run average crime rate with the 2012 rate and yields similar results. Column (5) examines crime trends, which may matter more than levels if individuals are concerned that gun control will prevent purchases in the future when crime may be higher. We compute each county's crime trend from 2004 to 2012 and find no significant relationship between crime trends and the Sandy Hook response.

5.2.2 Obama and the Great Recession

We next study whether the Great Recession contributed to the surge in firearm demand around 2009. If firearms are a normal good, a reduction in income would decrease gun demand. On the other hand, sociological research suggests that the anxieties generated by economic insecurity may drive firearm purchases to restore a sense of control (see, e.g., Mencken and Froese, 2019; Warner and Steidley, 2022). To understand the 2009 demand increase, we create an equivalent proxy and run a regression similar to those in Equations (9) and (10), respectively. We use three measures of economic conditions from 2007 to 2010: the percentage-point change in unemployment, the change in log per-capita income, and the change in the Federal Housing Finance Agency's Housing Price Index. We present results in Table A8. [37]

We first find that increased Republican vote share is associated with the 2009 demand surge, consistent with concerns about future gun control during the Obama presidency (Depetris-Chauvin, 2015). Turning to economic conditions, a one-standard-deviation larger increase in unemployment (approximately 2 pp.) is associated with a 5.7 pp. larger increase in CCW permits. We find similar, albeit smaller and only marginally significant, effects for changes in log income: greater income declines are related to more CCW permits. The coefficient on housing prices, however, is insignificant. Taken together, these results provide some evidence that areas more impacted by the recession had higher gun demand and support the hypothesis that economic insecurity can fuel firearm purchases.

5.3 Gun Buying During COVID

Finally, we discuss the surge in gun demand during COVID using NICS background checks from 2018-2021. Gun buying spiked in March 2020 as the pandemic began in the United States, followed shortly by another surge in June amid nationwide protests and riots after the murder of George Floyd. Purchases rose in early 2021 after the inauguration of President Joe Biden and then again in March after renewed calls for an assault weapons ban following a mass shooting in Boulder, Colorado. We have focused on the spikes, but even the troughs indicate elevated gun buying during the pandemic relative to pre-COVID times.

These events reflect several of the themes of this paper. The pandemic was characterized by heightened uncertainty, fear, and social and political instability, consistent with research in psychology suggesting that guns are a coping mechanism to restore a sense of safety and control (Buttrick, 2020). The protests and riots during the summer of 2020 involved highly

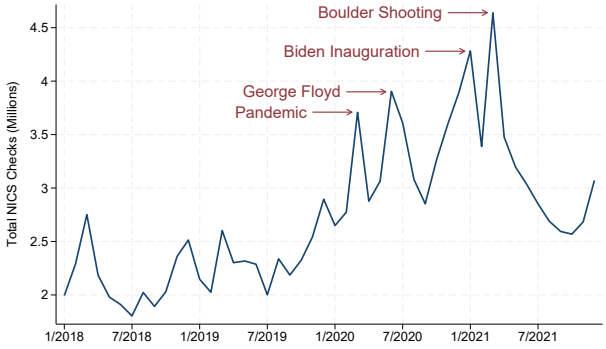
[37] Unlike the regressions in Table 8, the regressions in this table include only demographic covariates, excluding mean unemployment and mean log-income.

salient, widespread, and politically charged crimes, perhaps echoing the gun buying during the civil unrest of the 1960s. The fears of gun control and the government in early 2021 resemble those during the Obama presidency. More broadly, these episodes highlight the importance of national events and factors in gun demand (Johnson et al., 2026).

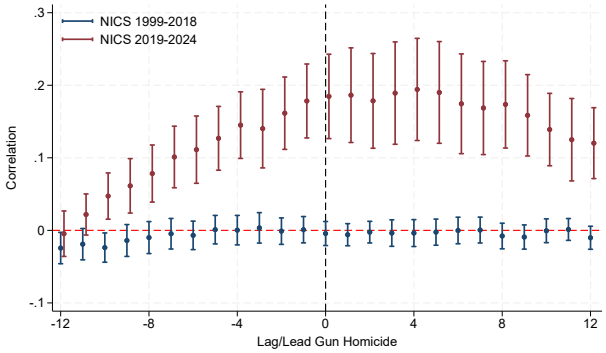
At the same time, the homicide rate increased by 30% in 2020 and remained elevated throughout the pandemic, raising questions about the relationship between homicides and firearms during this period. As a first pass, we regress standardized state-level NICS background checks on standardized leads/lags of homicides, controlling for state-specific linear time trends and state-specific month indicators. Figure 7b shows the results for the periods 1999–2018 and 2019–2024. Prior to 2019, the monthly correlations between background checks and homicides were near zero for all leads and lags. In contrast, there is a strong relationship between the two variables during COVID. Consistent with this change, the panel structural break test of Baltagi et al. (2016) identifies a substantial break in March 2020.

Figure 7: NICS and Homicides

(a) Time Series Plots



(b) Correlation Coefficients



Notes: Part (a) number of NICS firearm background checks from 2018 to 2021. Part (b) shows correlation coefficients between NICS and lags/leads of homicides, controlling for state-specific time trends and month indicators. Standard errors clustered at the state level.

As discussed in Section 1, empirical evidence indicates that greater gun prevalence increases homicides. During the pandemic in particular, the rise in gun purchases (and increased gun carrying) likely contributed to the homicide spike (Ludwig, 2026), consistent with the right side of Figure 7b. At the same time, the association of background checks with lagged homicides could indicate the presence of reverse causality, whereby increases in homicides stimulate subsequent firearm demand.

Of course, this analysis is very simple, the COVID surges in homicides and guns are multifaceted, and we cannot rule out serial correlation as the explanation of Figure 7b. Nevertheless, if rising homicides contributed to gun buying during COVID, then this would

illustrate the extreme circumstances that may be necessary for crime to play a large role in overall firearm demand: widespread, salient, and politically charged violence during a period of social turmoil.

6 Conclusion

We examine three sources of evidence on the effect of crime on gun demand. First, we exploit variation in COPS grants to estimate the effects of crime on gun demand, finding no evidence that either grants or crime affects demand. This finding is difficult to reconcile with a standard expected utility model in which firearms serve as protection against crime risk.

Second, we examine whether short-run spikes in crime could stimulate firearm purchases. Using city-level data from Massachusetts, we find that gun sales increase in response to crime shocks, suggesting that crime salience influences gun demand. However, the shocks are rare, the effect sizes are small, and they explain very little variation in firearm sales.

Third, we consider broad descriptive patterns about firearm markets, which point to politics, economic insecurity, and social unrest as important determinants of self-defensive gun ownership. While crime possibly affected purchases during the twentieth century, in the current political era it may take highly salient crime during periods of great societal instability, such as the pandemic, to substantially influence gun demand. Overall, our results suggest that crime, at best, plays only a minor role in gun demand.[38]

A key question is what these results imply for reverse causality when estimating the effects of guns on crime. Our findings suggest that the reverse causality problem is unlikely to pose a major threat to identifying the impact of firearm ownership on crime rates. However, there is an important caveat: most of the gun literature focuses specifically on the effects of firearms on homicides, while we focus on crime rates in general. When examining homicides, we find only small and temporary increases in sales, primarily concentrated in small towns. These results mirror those in Depew and Swensen (2019) in North Carolina, who even go a

[38]These results are not particularly surprising given how firearm markets operate. Gun markets have a large number of frictions that make large responses to local crime spikes unlikely. For instance, obtaining a Type 1 FFL (Dealer license) requires completing Form 7/7CR, which is actually composed of three separate forms: the 7/7CR itself, the Part B – Responsible Person Questionnaire, and the FD-258 Fingerprint Card. Applicants must mail these to the ATF Licensing Center along with the application fee (currently $200) and provide copies to the Chief Law Enforcement Officer (CLEO) in the locality where the business will be located. The ATF then assigns an Industry Operations Investigator to interview the applicant, inspect the business, and ensure compliance with federal, state, and local requirements. Additionally, firearms themselves are not inexpensive; common Glock 9mm pistols typically cost over $400, while cheaper alternatives like the Taurus GX2 run over $200.

step further by showing that the effects on CCW permits are concentrated entirely within the census tract of the homicide.

One possible takeaway is that reverse causality in this setting is primarily a localized, short-lived phenomenon and is therefore most apparent in high-frequency, geographically narrow (e.g., zip code, census tract, city block) data. Such responses may be more difficult to detect in aggregated data because homicides explain a small share of variation in gun purchases. More broadly, endogeneity should probably not be thought of as binary but rather as a continuum, with the degree of concern depending on how much of the identifying variation it can explain.

Our findings also have implications for public policy. Some policymakers may worry that increases in crime will also raise gun ownership and (assuming guns increase crime, or at least homicides) thus lead to more crime. Such feedback loops are central to models of arms races and self-reinforcing cycles of firearms and violence (Ehrlich and Saito, 2010; Evans and Kotowski, 2024). Our results indicate that this concern is likely unwarranted unless the increase in crime is massive or particularly salient. Likewise, arms race models likely have limited relevance for general crime rates, although they may apply in certain high-homicide areas (Evans and Kotowski, 2024).

Finally, a crucial problem is reconciling our findings with the stated preference for self-defensive gun ownership. The perception of crime may be more important than crime itself. News coverage of crime influences the fear of crime (Romer et al., 2003) and potentially the acquisition of firearms. Social media and word-of-mouth discourse, particularly through neighborhood-based platforms such as Nextdoor, may heighten concerns about local crime. This mechanism may help explain our findings for crime spikes, although more analysis is needed to directly test the importance of media.

Another explanation is that self-defense encompasses more than protection against specific crimes. Anti-government sentiment is an important component of modern American gun culture, including the belief that firearms are necessary to resist tyranny (Jiobu and Curry, 2001; Cramer, 2009; Seto, 2025). Concerns about future gun control led to massive spikes in gun purchases following the Sandy Hook school shooting. While demand increased across the country, Republican vote share is a crucial predictor of the magnitude of these increases. Concerns with future firearms restrictions and government overreach likely contribute to gun ownership.

Finally, defensive gun ownership may reflect broader, diffuse anxieties about the world, personal uncertainties, and one's social standing (Stroebe et al., 2017; Buttrick, 2020). Research in psychology and sociology, primarily relying on survey data, indicates that these psychological factors are critical for defensive ownership. Our analysis of gun demand dur-

ing the Great Recession and COVID-19 is consistent with these accounts. At the same time, survey-based evidence should be interpreted cautiously, as people sometimes misreport on surveys, particularly when questions involve sensitive topics (Marquis et al., 1986; Tourangeau and Yan, 2007; Gosse, 2014). Surveys about guns may not be any different. For example, Cook and Ludwig (1998) find evidence consistent with overreporting of defensive gun use, and Miller et al. (2022) find that patients hospitalized for gunshot-related injuries sometimes misreport the cause of their injury. As such, it is possible that some gun owners misreport the motivations underlying their ownership status. Further research using causal designs will help evaluate the importance of these psychological mechanisms in shaping firearm demand.

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7 Appendix

7.1 Figures

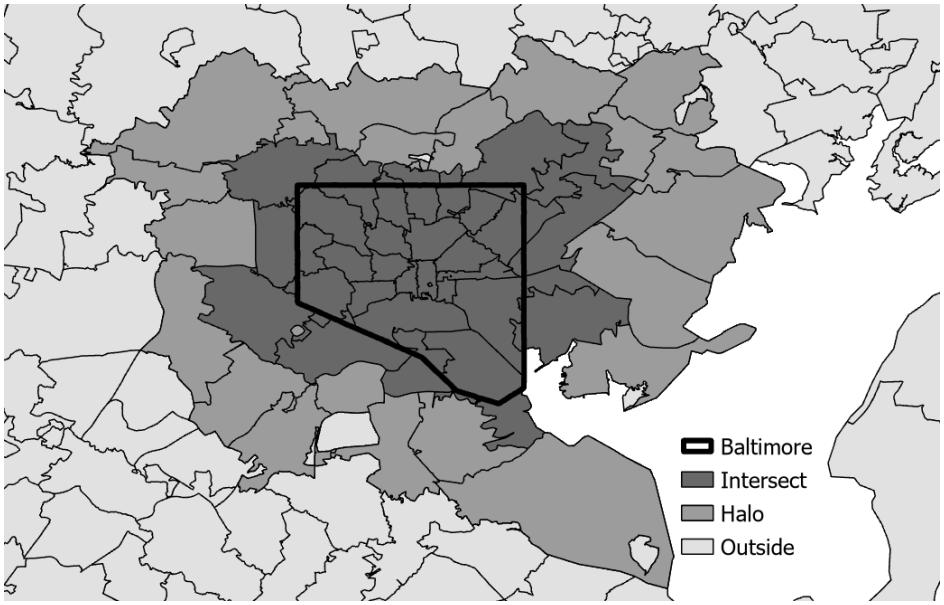
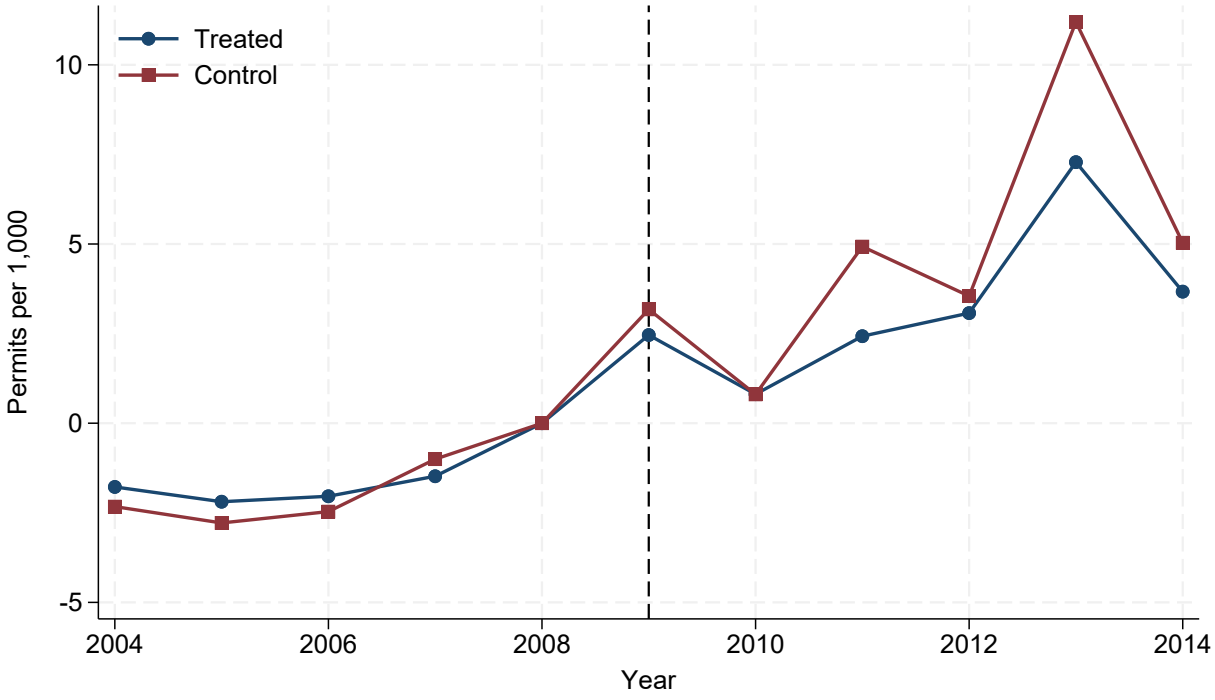


Figure A1: Example of the Gun Dealer Measure for Baltimore, MD

Figure A2: Average CCW Permit Rates (County Data)

(a) Treated vs. Control



(b) Iowa

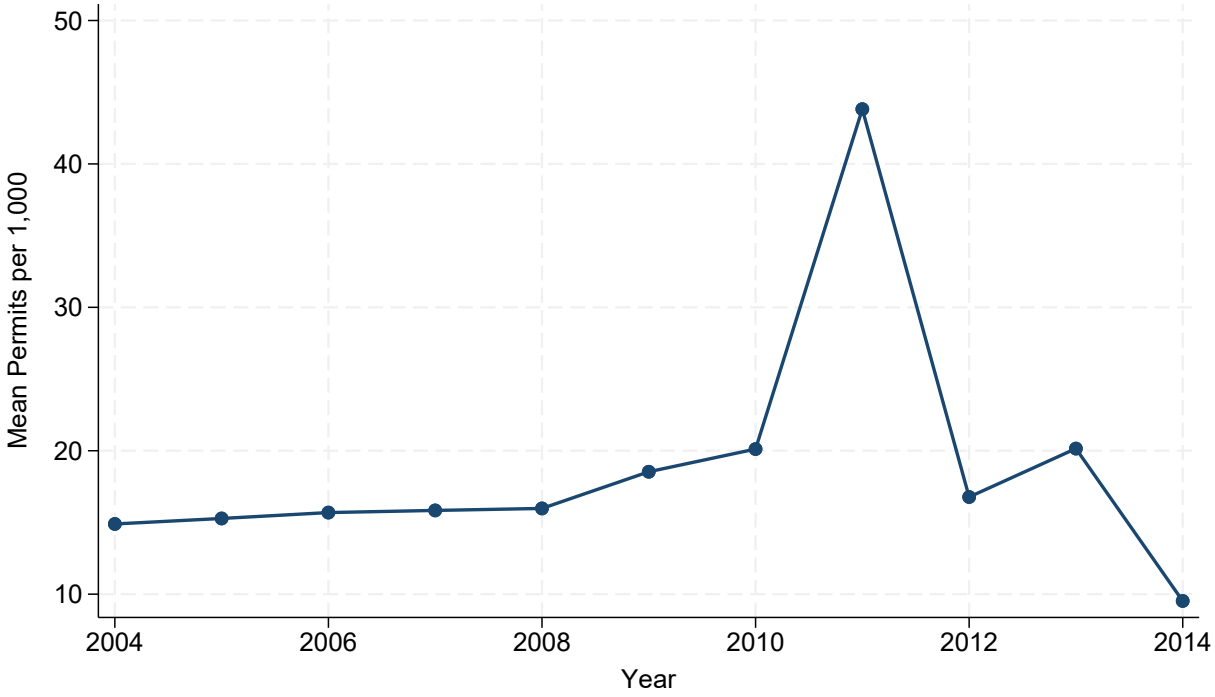
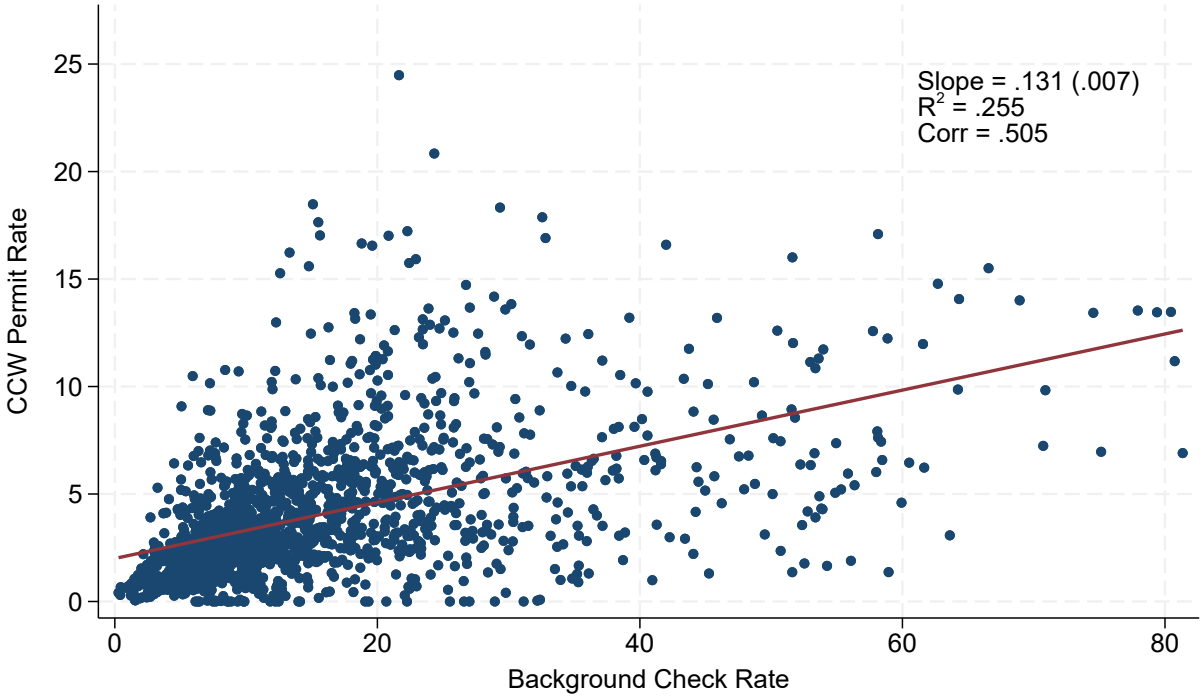
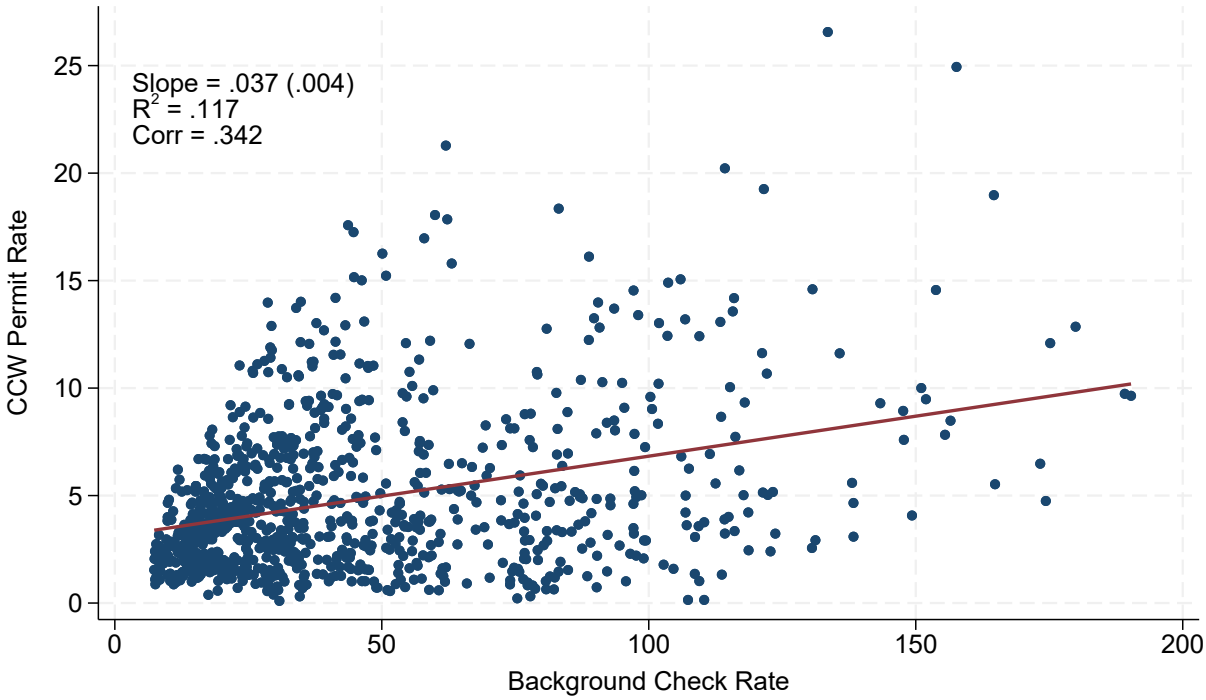


Figure A3: Dealer-Background Check Measure vs. City-Level Permit Data

(a) Massachusetts



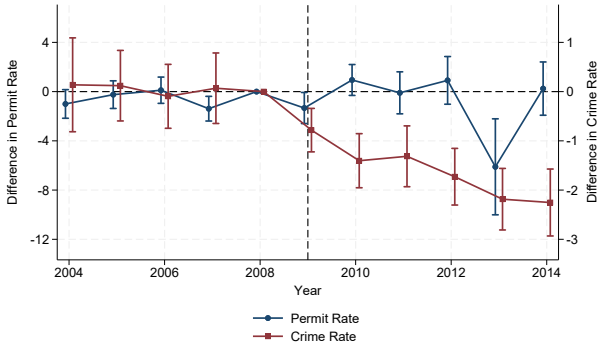
(b) Minnesota



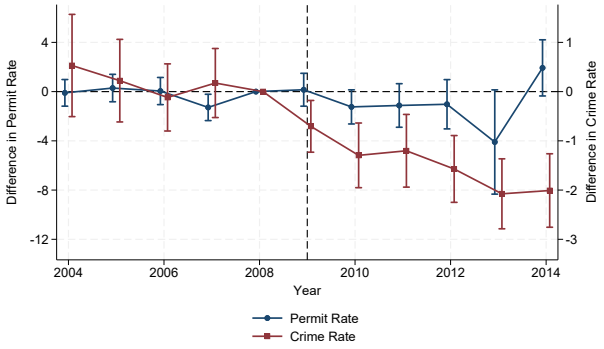
Notes: Illustration of the geographic area used to construct the gun dealer measure for Baltimore, Maryland. The measure includes firearm dealers located in zip codes that intersect the city's boundaries, as well as dealers in adjacent zip codes.

Figure A4: Event Study Plots (County Robustness Checks)

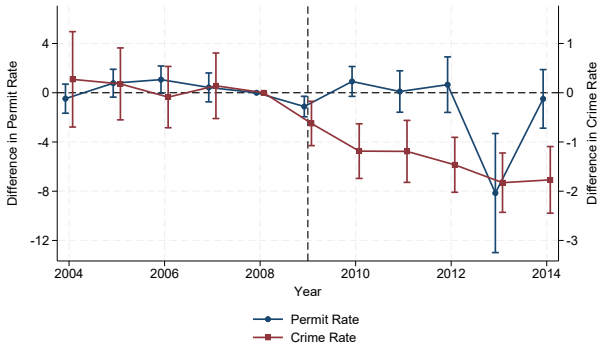
(a) Baseline



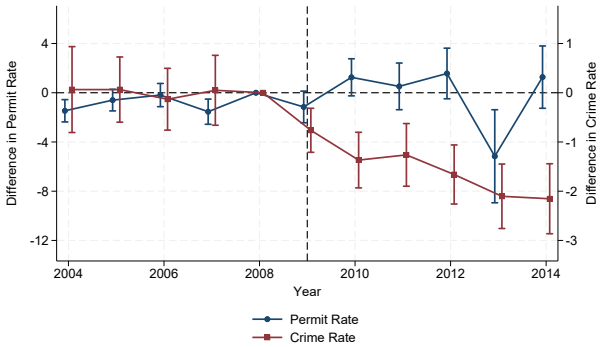
(b) Year Dummies × GOP



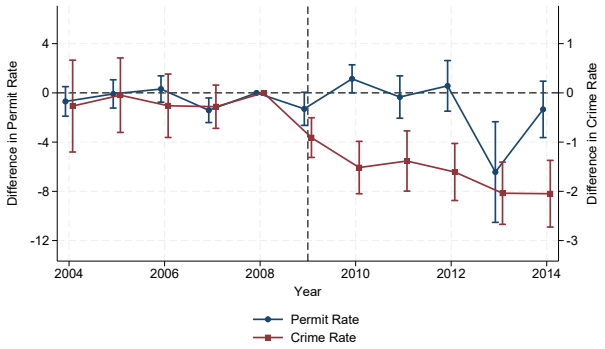
(c) State-Year FEs



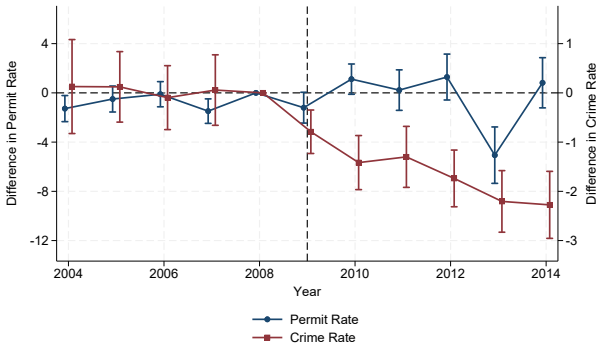
(d) Extrapolate Pre-Trends



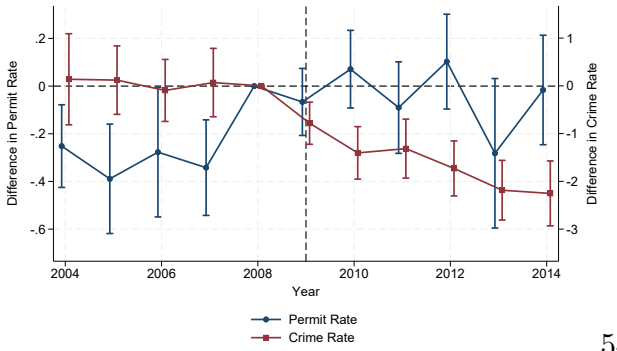
(e) Drop Low Coverage



(f) Drop Sandy Hook Outliers



(g) Log(Permit rate)



(h) Log(Permit rate) + State-Year FEs

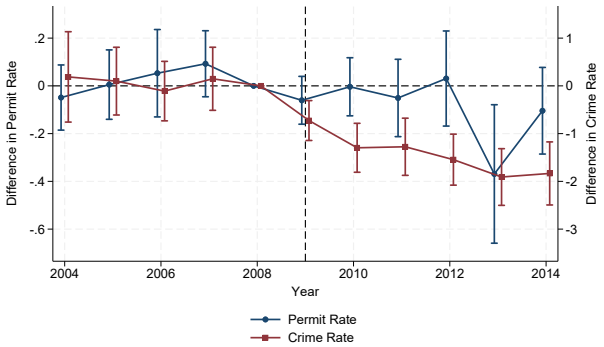
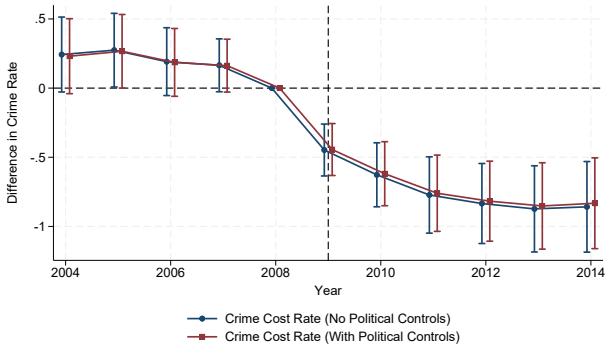
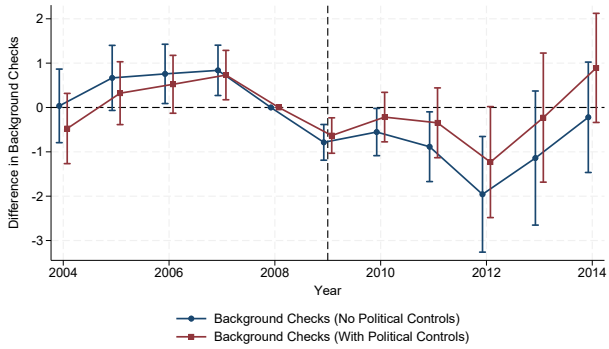


Figure A5: Event Study Plots (Background Checks and Gun Dealer Data)

(a) Cost Crime Rate



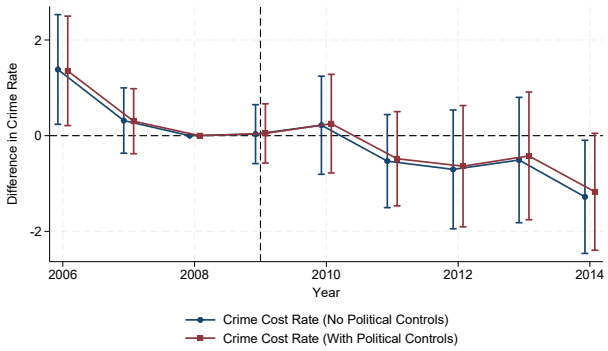
(b) Background Checks Rate



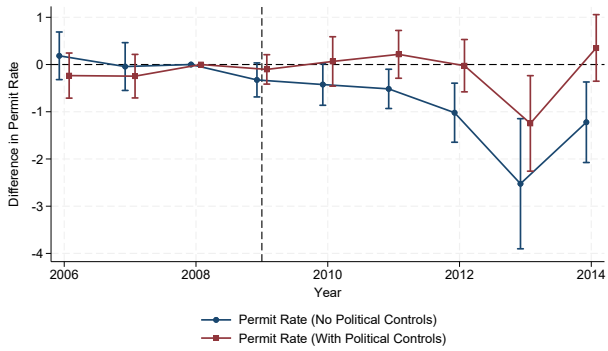
Notes: Event study plot variants of Equation (3). We show the plots with and without the average GOP vote share times year covariate. Standard errors clustered at the city level.

Figure A6: Event Study Plots (MA and MN Permit Data)

(a) Cost Crime Rate



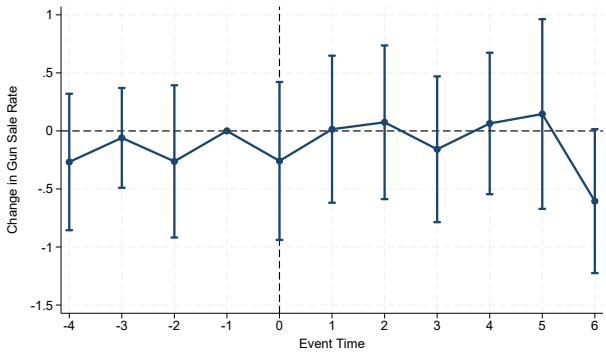
(b) CCW Permit Rate



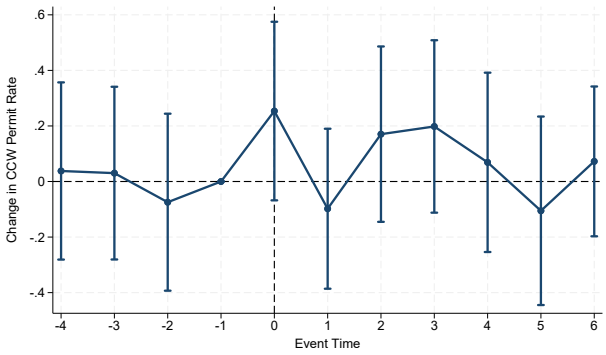
Notes: Event study plot variants of Equation (3). We show the plots with and without the average GOP vote share times year covariate. Standard errors clustered at the city level.

Figure A7: Event Study Plots for Other Thresholds

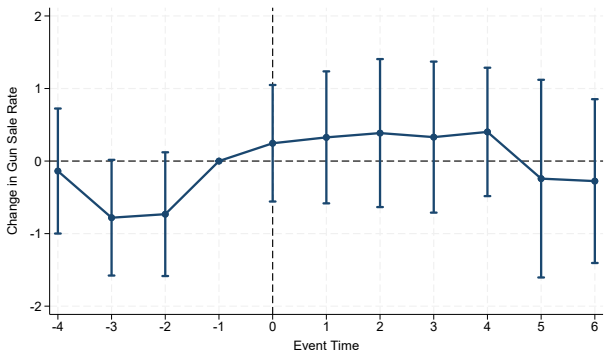
(a) Gun Sales (S=20)



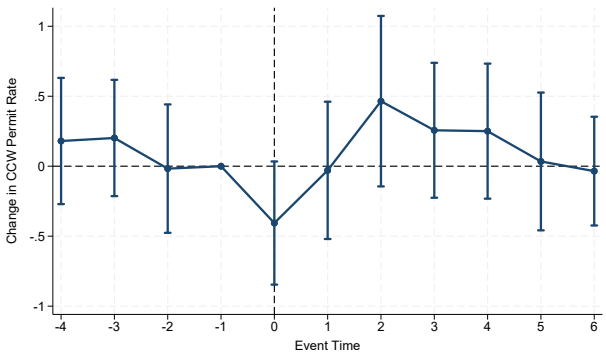
(b) CCW Permits (S=20)



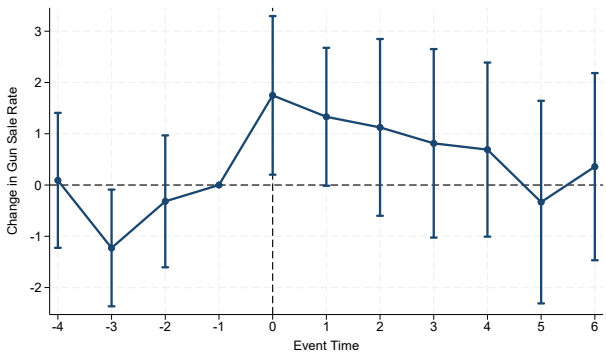
(c) Gun Sales (S=30)



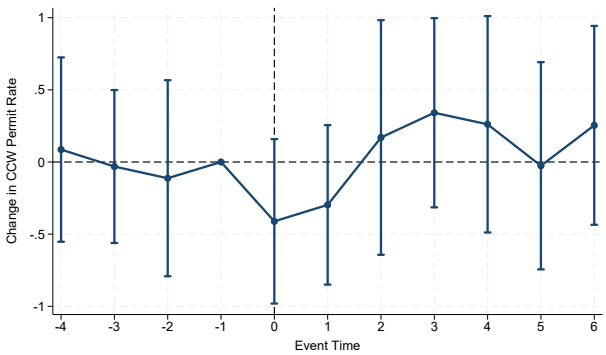
(d) CCW Permits (S=30)



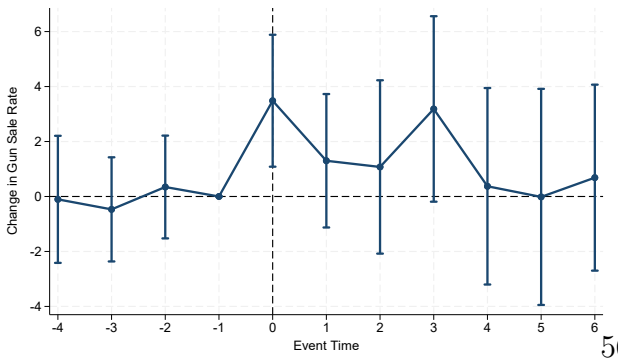
(e) Gun Sales (S=40)



(f) CCW Permits (S=40)



(g) Gun Sales (S=50)



(h) CCW Permits (S=50)

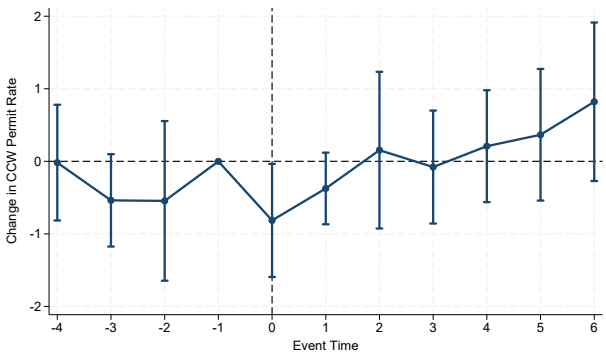
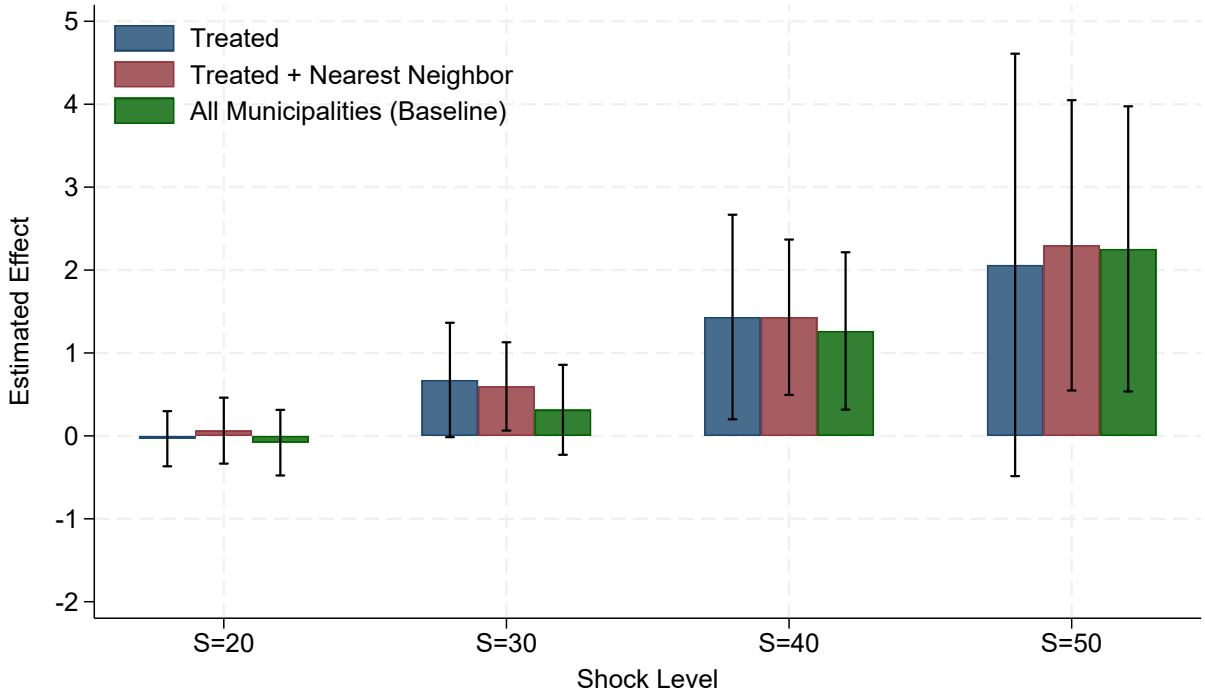


Figure A8: Effects of Crime Shocks Across Control Groups

(a) Gun Sales



(b) CCW Permits

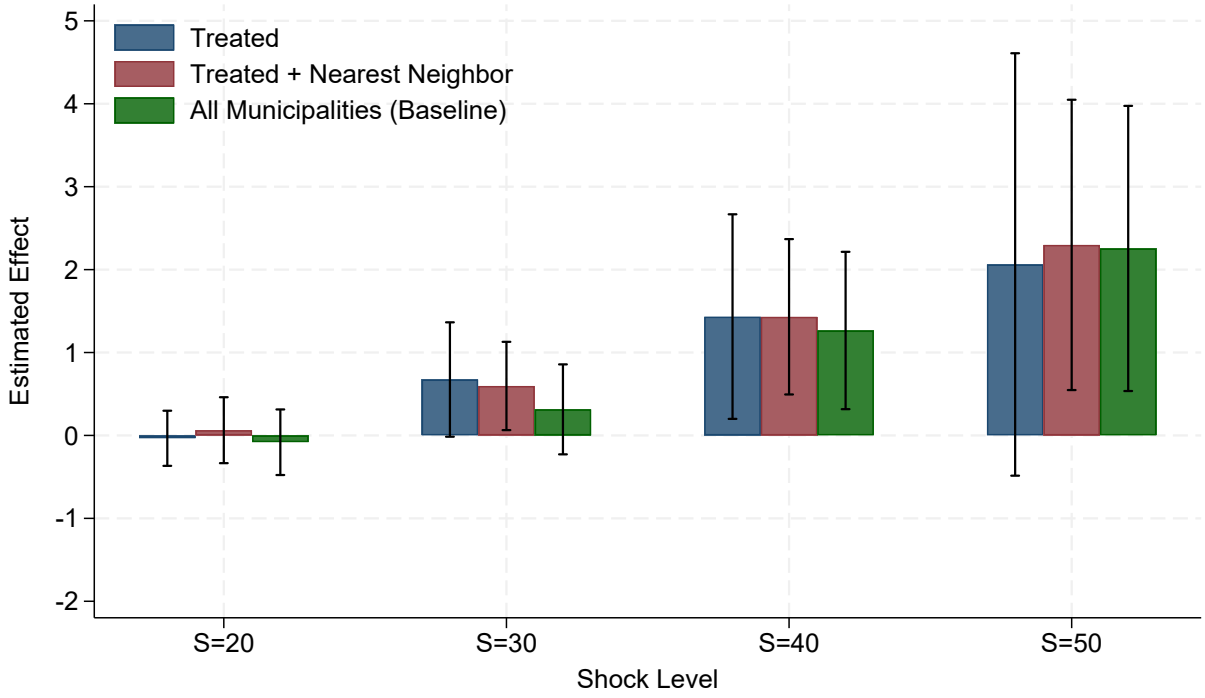
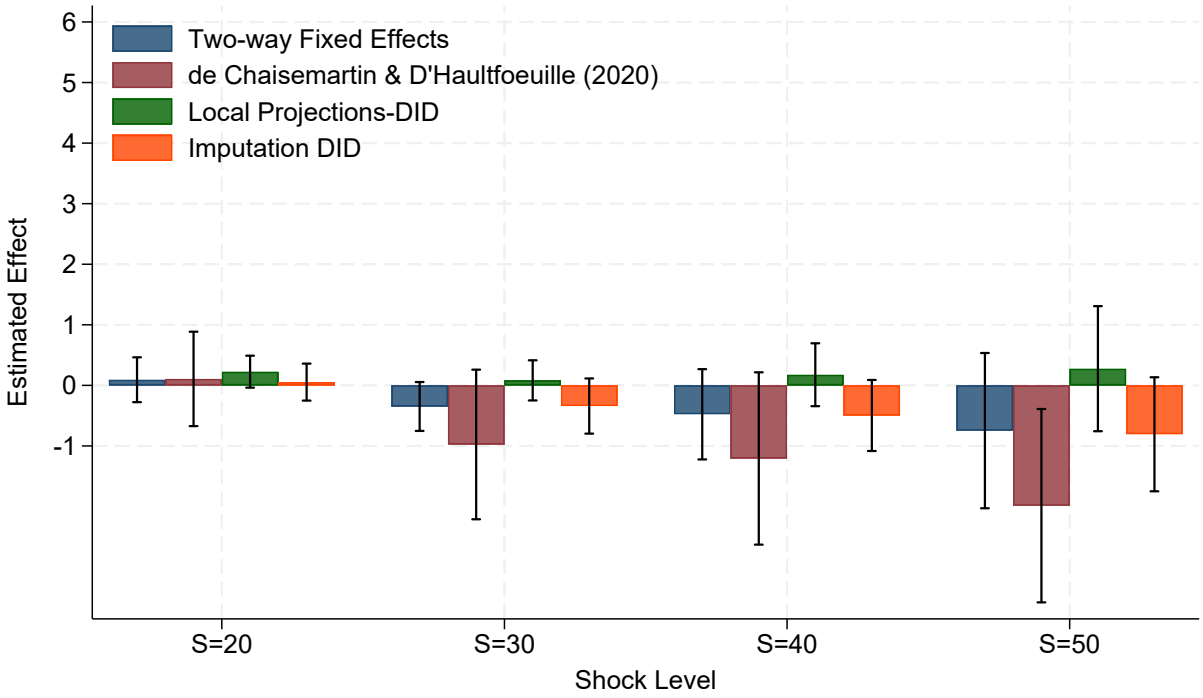
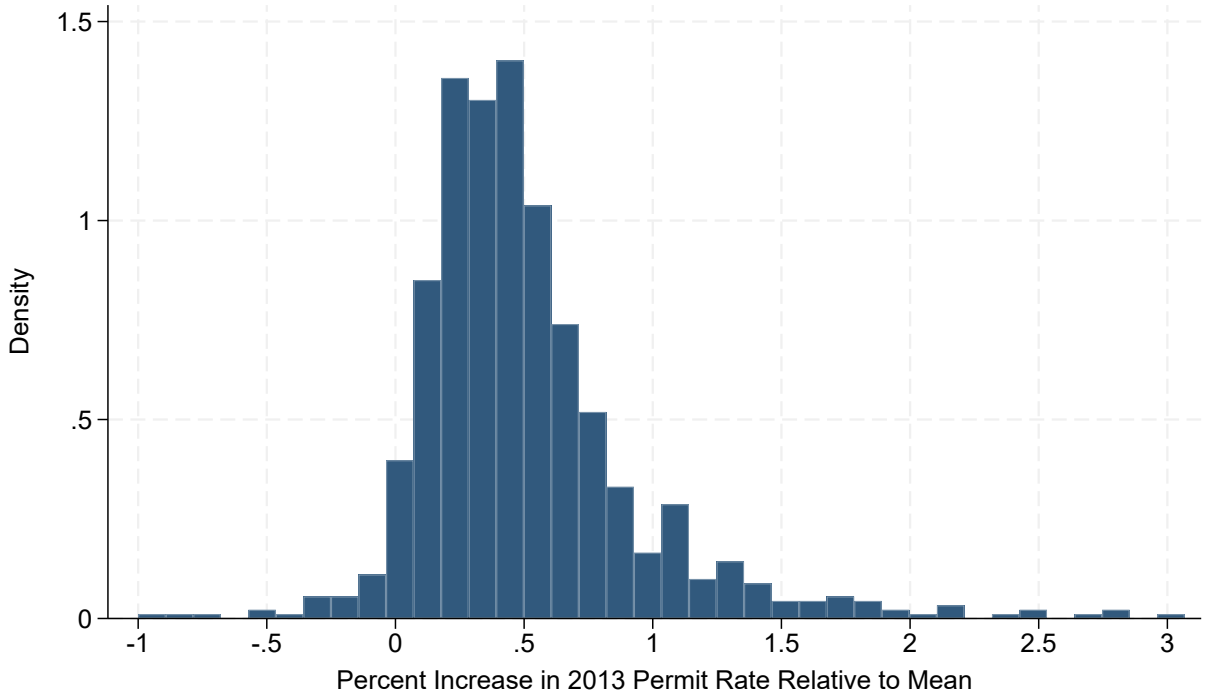


Figure A9: Effects of Crime Shocks Across Estimators (CCW Permits)



Notes: Estimates obtained using TWFE, De Chaisemartin and d'Haultfoeuille (2020), Dube et al. (2025), and Borusyak et al. (2024). Standard errors for the imputation method are bootstrapped with 399 replications.

Figure A10: Relative Permit Increases in 2013



Notes: Distribution of the Sandy Hook proxy described in Equation (9).

7.2 Tables

Table A1: Summary Statistics: Massachusetts and Minnesota Data

	Control		Treated	
Gun Variables				
Permits Issued per 1,000	2.295	(1.820)	1.194	(0.804)
Crime Variables				
Crime Cost per 1,000	4.431	(2.367)	11.89	(3.986)
Violent Crime per 1,000	3.731	(2.225)	10.43	(3.885)
Property Crime per 1,000	16.11	(9.205)	36.18	(8.903)
Control Variables				
Per Capita Income	47245.1	(9377.9)	46117.8	(8174.1)
Unemployment	4.845	(0.800)	5.116	(0.826)
Percent Black	0.0560	(0.0309)	0.0908	(0.0803)
Percent Hispanic	0.0750	(0.0491)	0.0974	(0.0610)
Percent Young Male	0.0997	(0.0135)	0.108	(0.0219)
Republican Vote Share	0.412	(0.0956)	0.283	(0.0713)
Population (10,000s)	2.009	(2.049)	10.44	(15.29)
Number of Cities	276		19	

Note: Summary statistics computed over the pre-treatment period 2006–2008. Means are given with standard deviations in parentheses.

Table A2: Summary Statistics: Background Checks and Gun Dealers

	Control		Treated	
Gun Variables				
Background Checks per 1,000	32.54	(26.53)	32.05	(24.75)
FFLs per 1,000	0.256	(0.218)	0.267	(0.262)
Crime Variables				
Crime Cost per 1,000	6.868	(4.777)	11.81	(5.598)
Violent Crime per 1,000	5.594	(4.514)	9.394	(5.253)
Property Crime per 1,000	28.11	(17.99)	52.05	(22.65)
Control Variables				
Per Capita Income	36333.1	(10652.8)	33810.6	(9486.9)
Unemployment	5.179	(1.427)	5.695	(1.945)
Percent Black	0.0949	(0.112)	0.148	(0.156)
Percent Hispanic	0.0990	(0.129)	0.122	(0.159)
Percent Young Male	0.103	(0.0194)	0.106	(0.0183)
Republican Vote Share	0.517	(0.134)	0.490	(0.134)
Population (10,000s)	2.411	(15.35)	6.940	(21.87)
Number of Cities	3531		789	

Note: Summary statistics computed over the pre-treatment period 2004–2008. Means are given with standard deviations in parentheses.

Table A3: Regressions for Alternate Data (No GOP Vote Share)

	Crime Rate	Guns	OLS: Guns	IV: Guns
	(1)	(2)	(3)	(4)
A. Background Checks and Gun Dealer Data				
Crime Cost Rate			-0.0272	1.517**
			(0.0480)	(0.612)
Treat×Post	-0.910***	-1.380***		
	(0.120)	(0.524)		
Elasticity			-0.006	0.320
F-stat	56.995			
Mean Outcome	6.827	32.365	32.365	32.365
Num Cities	4320	4320	4320	4320
B. MA and MN City CCW Permit Data				
Crime Cost Rate			-0.00436	1.023**
			(0.0501)	(0.425)
Treat×Post	-1.021**	-1.045***		
	(0.455)	(0.327)		
Elasticity			-0.009	2.058
F-stat	5.031			
Wild P-value	0.042	0.007	0.931	
AR P-value				0.003
Mean Outcome	4.717	2.345	2.345	2.345
Num Cities	295	295	295	295

Note: Column (1) shows the first-stage estimates of COPS on crime, with the Kleibergen-Paap rk Wald F-statistic. Column (2) shows the reduced form estimates of COPS on gun demand. Column (3) shows OLS estimates of the effect of crime on guns. Column (4) shows the IV estimates of crime on guns. The reported means of the outcome variables come from the control group prior to treatment. For Panel B, we also report the wild cluster bootstrap p-values for columns (1)–(3) and the Anderson-Rubin p-value for column (4). Standard errors in parentheses are clustered at the city level with *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Regressions for Bordering Towns: Gun Sales

	NIBRS Sample (1)	FRB Sample (2)	Below Median Pop (3)	Below Median Dist (4)
A. $S = 40$ **Cut Off**				
Post Spike	0.186	0.371	0.097	-0.219
	(0.256)	(0.346)	(0.650)	(0.520)
B. $S = 50$ **Cut Off**				
Post Spike	-0.055	0.112	-0.162	0.397
	(0.366)	(0.458)	(0.858)	(0.678)
Sample	NIBRS	FRB	FRB	FRB

Note: Regression estimates for Equation (6) applied to the municipalities that border the towns with a crime spike for a cut off of $S = 40$ and $S = 50$. Column (1) uses the sample of NIBRS towns; (2) uses the full set of towns in MA from the FRB data; (3) restricts the sample to bordering towns below the median population; (4) restricts the sample to bordering towns below the median distance to the shocked towns. Standard errors are clustered at the shock town level with *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Regressions for Bordering Towns: CCW Permits

	NIBRS Sample (1)	FRB Sample (2)	Below Median Pop (3)	Below Median Dist (4)
A. S = 40 Cut Off				
Post Spike	0.222	0.018	-0.394	-0.138
	(0.228)	(0.279)	(0.517)	(0.427)
B. S = 50 Cut Off				
Post Spike	-0.016	0.034	-0.078	0.342
	(0.344)	(0.429)	(0.825)	(0.567)
Sample	NIBRS	FRB	FRB	FRB

Note: Regression estimates for Equation (6) applied to the municipalities that border the towns with a crime spike for a cut off of $S = 40$ and $S = 50$. Column (1) uses the sample of NIBRS towns; (2) uses the full set of towns in MA from the FRB data; (3) restricts the sample to bordering towns below the median population; (4) restricts the sample to bordering towns below the median distance to the shocked towns. Standard errors are clustered at the shock town level with *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Regression Results for Violent Crime Spikes

	Gun Sales				**CCW Permits**			
	S=5	S=7	S=9	S=11	S=5	S=7	S=9	S=11
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post Shock	-0.369**	-0.638*	-0.183	-0.623	-0.140	-0.419	0.165	0.835
	(0.164)	(0.346)	(0.566)	(1.030)	(0.228)	(0.377)	(0.590)	(1.183)
Elasticity	-0.024	-0.031	-0.008	-0.022	-0.012	-0.028	0.010	0.040
Outcome Mean	7.092	7.102	7.103	7.105	5.176	5.190	5.190	5.192
Crime Mean	0.864	0.884	0.893	0.900	0.864	0.884	0.893	0.900
Spike Size	7.564	10.10	11.76	14.35	7.564	10.10	11.76	14.35
Num Shocks	360	135	69	27	360	135	69	27

Note: Regression estimates for Equation (6). Columns use shock thresholds of either $S = 5, 7, 9$, or 11. Outcome means come from the comparison group. The elasticity is for the outcome variable with respect to the crime shock. Standard errors are clustered at the town level, with *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Effects of Homicides on CCW Permits

	Below Median Pop	Above Median Pop	All Towns
	(1)	(2)	(3)
Post	0.411	0.027	0.097
	(0.274)	(0.039)	(0.062)
Outcome Mean	6.722	3.719	5.269
Num Homicide Events	77	858	935

Note: Regression estimates for Equation (6) using homicides as the crime shock. The median population cutoff is calculated using the average population of towns in the sample from 2009 to 2019. Standard errors clustered at the town level with *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Great Recession Proxy Regressions

	(1)	(2)	(3)	(4)	(5)
Unemployment Shock		0.055**			0.054**
		(0.022)			(0.022)
Log Income Shock			-0.028*		-0.026*
			(0.015)		(0.015)
Housing Price Shock				0.002	0.018
				(0.016)	(0.015)
Pct GOP 2008	0.065***	0.072***	0.072***	0.065***	0.074***
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Pct Rural	-0.017	-0.021	-0.011	-0.018	-0.020
	(0.016)	(0.015)	(0.017)	(0.016)	(0.016)
Crime Rate	-0.003	-0.011	-0.006	-0.003	-0.013
	(0.018)	(0.017)	(0.018)	(0.018)	(0.017)
Covariates	X	X	X	X	X
State FEs	X	X	X	X	X
Mean	0.383	0.383	0.383	0.383	0.383
R2	0.460	0.469	0.464	0.460	0.472
N	744	744	744	744	744

Note: Regression estimates for the equivalent of Equation (10) applied to the 2009 gun demand increase. All explanatory variables are standardized. The additional covariates include only demographic characteristics. Only demographic covariates are included. Standard errors are robust to heteroskedasticity with *** p<0.01, ** p<0.05, * p<0.1.