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The Effect of Municipal Gun Laws on Firearm Fatalities

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Abstract

In this article, I present a novel dataset of the universe of municipal gun ordinances between 2008 and 2019 to estimate the effects of gun ordinances on firearm fatalities. I implement a 2SLS identification approach using exogenous variation in turnout to the 2018 Nation School Walkout to isolate exogenous variation in county level find no discernible effect of ordinances on firearm deaths at the county level. These estimates are tightly bound to zero, so I can rule out large effects. State preemption prevents most municipalities from passing or enforcing laws, which may be a driving force of the null results. I present suggestive evidence that effects may be stronger in California where local communities actively combat gun violence.

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1. Introduction

Over 680,000 people have died in firearm-related fatalities in the United States since 2000 (Pew Research Center, 2023). The estimated annual economic impact of gun violence exceeds \$550 billion, encompassing immediate expenses, long-term costs, and the loss of quality of life (Everytown Research & Policy, 2022). This problem is uniquely American among developed countries, as the firearm death rate in the US is 5 times higher than the second highest county (Gumas et al., 2023). While a majority of Americans support increased gun control, legislation regularly gets gridlocked in federal and state legislatures (Brenan, 2024). In response, numerous grassroots movements have collaborated with mayors, city council members, and other community leaders to combat firearm violence locally (Everytown for Gun Safety, 2024; Blannelberry, 2018). Simultaneously, the number of gun ordinances rose sharply from approximately 700 to 7,400 between 2016 and 2019. Figure 1 shows how this increase deviated from a decade-long period of stability. While the purpose of the ordinances is to make communities safer and improve citizens' overall well-being, little work has been done to study if they decrease violence. In this paper, I examine the effect of municipal firearm ordinances on firearm fatalities.

I construct a novel longitudinal dataset of municipal firearm ordinances covering the entire United States. The Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF) must compile state laws and municipal ordinances to help citizens comply with all laws governing their geographic area (Bureau of Alcohol, Tobacco, Firearms and Explosives, 2023). Through a Freedom of Information Act request with the ATF, I construct a longitudinal history of municipal ordinances in all states. I obtained state-year-level documents listing all municipalities within that state with ordinances and the subject matter. I create a dataset with the number of ordinances in each municipality and the subject of each ordinance. To the best of my knowledge, this is the first paper to study the complete longitudinal set of ordinances in the United States. This comprehensive approach provides a holistic framework to study ordinances. It allows me to study how ordinances have changed over time.

In an OLS framework, I estimate that an additional ordinance is associated with a decrease of 0.04 firearm deaths per year at the county level after controlling for county-level characteristics and state-fixed effects. The average number of firearm ordinances in all US counties is 2.3, so this point estimate implies an average 0.1 decrease in deaths. This point estimate is statistically insignificant with a standard error of 0.04, so I can rule out any large relationships between total gun deaths and ordinances in this framework. I also separate the outcome by suicides and homicides. Suicides are associated with a marginally significant 0.19 decrease per additional ordinance, and homicides are associated with a statistically insignificant 0.15 increase per additional ordinance. The results are similar in regressions where the outcome is the number of deaths within subgroups, including combinations of the age, education, and gender of the deceased. I find that more gun ordinances are associated with fewer White suicide victims, but more Black homicide victims. Point estimates are also similar when the sample is limited to urban/metro areas. To situate the magnitude of the point estimates, a one standard deviation increase in median household income ($12,000) is associated with a -0.64 decrease in homicides and a 0.83 increase in suicides.

The ordinary least squares estimates are likely biased. Ordinances are intended to decrease firearm violence and fatalities. However, municipalities with firearm violence are the areas that pass ordinances. The reverse causation will bias the estimates upwards. So plausibly, ordinances have a stronger, negative effect on firearm fatalities than the ordinary least squares estimates suggest.

I exploit plausibly exogenous variation in ordinances to estimate the causal effect of ordinances on firearm fatalities. The Stoneman Douglas shooting put firearm violence and gun control at the forefront of American politics (Nass, 2018). Exactly one month later, the student led group Youth EMPOWER organized a single-day walkout of school to bring awareness to gun violence and encourage local activism. On March 14, 2018, over one million students participated in a walkout to advocate for increased gun control according to estimates from the Crowd Counting Consortium. This movement coincided with a drastic increase in the number of firearm ordinances. Following established single-day protest literature (Madestam et al., 2013; Hungerman and Moorthy, 2023; Madestam and Yanagizawa-Drott, 2012), I use day-of-protest rainfall to generate exogenous variation in the number of municipal ordinances. The first stage in this analysis is the number of ordinances on day of protest rainfall, which operates solely through the protest channel. The validity of the first stage requires rainfall to decrease protest turnout and protest turnout to increase ordinances. Using this 2SLS framework, I find that an additional ordinance, instrumented with day-of-protest rainfall, increases the number of deaths by 0.07. The estimate is a precise zero with a standard error of 0.12, so I rule out decreases larger in magnitude than 0.18 deaths per ordinance using a 95% confidence interval. A county would need five ordinances to prevent one firearm death using the lower bound of the CI, which only 7% of counties had in 2019. An effect of -0.18 deaths for an additional ordinance is roughly a 1% decrease from the average firearm deaths per county. The statistically insignificant point estimates on suicides and homicides are similar to total deaths. The 2SLS estimates are also similar when studied by characteristics of victim subgroups and geographic sample restrictions. Under my identifying assumptions, these results indicate ordinances do not have a detectable effect on gun deaths, and I rule out large effects.

The null effects in this paper may partly be attributed to preemption around firearm ordinances. In 2018, 41 states prevented municipalities from enacting and enforcing firearm legislation that exceeded the state's measures (Pomeranz et al., 2021). Even though municipalities subject to state preemption have passed more ordinances, there are still obstacles to enforcement. Gun rights groups, largely the National Rifle Association, can sue local governments via costly lawsuits (Mascia and Brownlee, 2018a). These decisions are left to judicial discretion; some judges strike down ordinances (Donohue, 2023) while others allow ordinances to stand in the interest of public health (Mascia and Brownlee, 2023). Municipalities may pause enforcement while in litigation, making it difficult to know which laws are enforced.

Another explanation for the low effects is that gun violence is a difficult outcome to change as its causes are deeply rooted and multifaceted. Makarios and Pratt (2012) summarizes the results of various types of policies implemented at different levels of government by organizing numerous studies into one of four broad categories: information and training campaigns; gun buy-back programs; gun laws; and law enforcement campaigns. Papers in the first three categories show weak and mixed relationships between policies being passed and firearm deaths. The types of interventions that did yield encouraging yet limited effects are those where multiple community organizations collaborated and worked with local youths (Sheppard et al., 2000; Braga et al., 2001). Even though communities have put a good deal of resources towards stopping gun violence, the problem is complex to address.

If gun violence is a complex and structural issue, municipalities may need the support of local governments for the ordinances to have any detectable effect. Of the 608 counties that experienced a change in the number of ordinances between 2016 and 2019, the median change was a 3 ordinance increase. Communities in California generally have been more sympathetic towards gun control causes, as they were the first communities to pass gun control measures. There is also a history of grassroots organizations that work with municipal governments to reduce gun violence (SPUR, 2005; Godwin and Schroedel, 2000). Additionally, some of the largest ordinance increases were in California counties including Los Angeles, Riverside, San Diego, Santa Cruz, and Santa Clara, all of which had over 80 new ordinances. I conduct another analysis by limiting the sample to counties in California. Using 2SLS, I find that an additional ordinance decreases the number of deaths by 0.45, which is 5.7 times greater in magnitude than the full sample 2SLS. This estimate is imprecise with a standard error of 0.95, partly due to there only being 58 observations in the regression. Given the median[1] county has 5 ordinances, the median county would experience a 2 death decrease in total deaths for 2019. The median number of gun deaths is 15, so this equates to approximately a 14% decrease from the median. Even though a California-only analysis lacks the power to

[1] I use the median in this analysis because Los Angeles County is an extreme outlier in the data and heavily skews the data

reject the null hypothesis, I present it as suggestive evidence that ordinances may function in communities that tend to act in support of gun control.

This article is situated in a small literature on the relationship between city firearm ordinances and firearm violence. O'Carroll et al. (1991) finds that a Detroit 1987 ordinance mandating a 30-90 day jail sentence for illegal weapon possession had no relation to changes in firearm-related homicides due to a lack of enforcement since only 2.2 percent of offenders received jail time due to judicial discretion. Loftin et al. (1991) finds that the requirement for a permit to carry a handgun in Washington D.C. resulted in a 25 percent decrease in both suicides and homicides. This study differs from these studies in some important ways. The previous studies use interrupted time series techniques to estimate the relationship between one ordinance being passed in a single city and gun violence in that city. This analysis uses longitudinal data and observes the entire United States. I observe all US municipalities, so I estimate the relationships between ordinances and deaths by comparing areas with ordinances with otherwise similar counties with fewer ordinances. My results provide an average effect of ordinances, not specific to a single city. Communities face different challenges such as legal battles over preemption or baseline levels of firearm deaths, so the effect of an ordinance could widely differ. This method also allows me to test heterogeneity in the number of deaths by race, gender, age, and education demographics. Second, I exploit plausibly exogenous variation in the number of ordinances in a county to estimate the causal effect of ordinances on firearm violence, not just a descriptive relationship. The amount of gun ordinances in a municipality is self-selected and correlated with other relevant characteristics, so the variation in OLS is not reasonably as good as random. Later, I present results that suggest rainfall, conditional on county characteristics, is good as random variation. Therefore, my reduced form and 2SLS estimates have a causal interpretation.

Publicized events regarding firearms can have unclear consequences. The purpose of the ordinances is to decrease gun violence, but gun support advocates are politically active and may respond in countervailing ways. For example, mass shootings can lead to the loosening of state gun laws if Republicans control the legislature (Luca et al., 2020). Also, mass shootings can lead to increased gun sales (Levine and McKnigh, 2017; Brock and Routon, 2020) which can lead to increased crime (Kleck, 2015; Duggan, 2001). In the context of ordinances, a municipality raising the age to purchase a gun may cause people to buy firearms before the ordinance is passed, which then can lead to increased crime and violence. This counter-activist effect may cause ordinances to have ambiguous effects.

The result of the first stage relates to a single-day protest literature. The connection between rainfall on March 14, 2018, and the number of municipal ordinances is driven by turnout for the National School Walkout. Even though turnout does not enter an equation, my results assume that areas that participated more in the movement went to pass more ordinances. Most participants in the National School Walkout could not directly participate in formal political institutions, so they had to inspire others to engage with the movement. There has been a growing concern that young Americans are uninterested in politics (Zhang, 2022). Under my 2SLS assumptions, my first stage implies that the spike in ordinances was in part due to the turnout at the walkout. Even though I do not show that ordinances fulfill their intended goals, this paper provides evidence that the student-organized movement led to changes in the legal landscape. Guns are an issue that 59% of Americans identify as "extremely important". Teenagers were able to move the needle on a central issue of American politics, yet has seen limited success in getting state and federal law to reflect the preferences of the American public's opinion (Goldberg, 2022). Also, this paper highlights an example where youths reveal they care, or can be convinced to care, about politics and actively engage with political institutions. González (2020) provides evidence for teenage activism, showing that Chilean students shifted votes to elect board members in line with their demands. This paper is consistent with González (2020) in showing that teenagers can initiate change when politically activated.

The rest of the paper is as follows: Section 2 gives more background to firearms and firearm legislation in the United States; Section 3 describes the data; Section 4 states the empirical model; Section 5 presents results; Section 6 concludes.

2. Background

2.1. Local Legislation

The home rule doctrine allows cities to enact and enforce laws to address local concerns. States either make this explicit in the state constitution or a separate statute. Common examples include fire maintenance or street safety (National League of Cities, 2020). These issues are generally too specific to be decided at higher levels of government and must be handled locally. In other instances, a municipality wants to further the law of the federal or state government, such as increasing the minimum wage. Cities in the United States have governed firearms dating back to colonial times, with the first record in Albany, New York when they restricted the sale of certain firearms to Native Americans in 1686 (Common Council of the City of Albany, 1838). Since then, many more cities have enacted firearm legislation. According to ATF's "State Laws and Published Ordinances - Firearms", 33rd edition, there were 1,358 distinct municipalities with at least one ordinance and a total of 7,485 ordinances on the books. Firearm ordinances have become much more common in the late 2010s. For approximately a decade, the number of firearm ordinances was relatively stable with between 650 and 800 ordinances at a given time (Figure 1).

Ordinances can be grouped into common general categories. The most common type of ordinance is a licensing ordinance. These ordinances often set forth the criteria for eligibility, the application process, background checks, and any training or safety courses required. Such ordinances aim to regulate who can legally own or carry a firearm in the community. These are meant to increase public safety by ensuring that only people with a low propensity to commit crimes obtain firearms. Table 1 lists the most common categories of firearm ordinances. In 2019, 1,537 ordinances referenced the need for a license. This reflects how licenses are governed at that state level. In 2018, only 10 states required prospective purchasers first to obtain a license or permit from law enforcement (Pomeranz et al., 2021)[2]. Many local governments see the lack of legislation around firearms as a gap in the gun laws and choose to supplement it with their laws. Other types of laws include age restrictions that raise the age to buy certain types of firearms, establishing additional penalties for breaking laws, defining prohibited actions, and establishing where firearm-related businesses can operate via zoning laws.

There are multiple ways that municipal governments organize to pass laws. In 2018, the International City/County Management Association conducted a survey of cities with a population of over 2,500 citizens. They found that 38 percent of cities nationwide used mayor-council government, 40 percent of cities used the council-manager government system, 11.9 percent of cities used the city commission system, 9.6 percent used the town meeting system, and another 0.5 percent used the representative town meeting (International City/County Management Association, 2020). The two most common types of systems have similar structures where a city council acts as a traditional legislative body that drafts deliberates, and votes on ordinances. In a mayor-council government system, the mayor approves or vetoes the laws. In a council-manager government system, a mayor serves on the city council but does not hold any legal privilege to distinguish themselves from the rest of the council. The mayor represents the city at the state and federal level (Ballotpedia, 2024).

2.2. State Preemption

States can revoke a municipality's home rule authority by declaring preemption in that legal domain. In the context of firearms, a state can invalidate a municipal firearm ordinance. Firearm preemption laws started in response to a 1981 ordinance passed by Morton Grove, Illinois to ban handguns. Many states developed a similar law as the NRA became more prominent and politically active (Mascia and Brownlee, 2018b). Pomeranz et al. (2021) describe the policy environment of state preemption in 2018. By then only 9 states had little

[2]Supplemental Appendix A, Table A.3

to no preemption laws. These states have larger populations. Combining this data with the Census's 2018 National and State Population Estimates, I estimate 29% of Americans live under no state preemption. The 41 remaining states have claimed legal authority to invalidate municipal firearm laws. These remaining states are categorized into either moderate or punitive preemption. In 2018, 15 states had punitive preemption measures, which means that the governments that pass the law can be held financially responsible for any legal fees in striking down the law. This number has grown, as only Kentucky and Florida had punitive preemption in 2010 (Pomeranz et al., 2021). In more strict cases, the member(s) who passed the ordinance can be removed from office. Moderate preemption gives the state government authority to strike down laws but does not grant action to be taken against the municipality that passed the ordinance. Figure 4 is a state-level map of the United States that shows the 2018 level of preemption using data from Pomeranz et al. (2021).

Cities subjected to preemption have passed additional ordinances and are typically sued by a gun rights group to remove the ordinance. Most notably, a municipal ordinance in Washington D.C. was invalidated (District of Columbia v. Heller, 2008) and again in Chicago (McDonald v. City of Chicago, 2010). Most gun ordinance cases are settled in lower courts, but controversy over ordinances has driven the two largest firearm Supreme Court cases. A more common example of these laws going to court is a 2010 Ohio Supreme Court decision that invalidated nearly 100 municipalities' firearm ordinances. The rationale behind this decision is that every city having its own set of firearm ordinances creates a "confusing patchwork" of licensing requirements and possession restrictions that citizens must navigate (Tobias, 2010). Sometimes even the threat of these lawsuits can stop an ordinance from ever being enacted. Cities wanted to pass more firearm laws after Parkland, but local policymakers decided not to push the ordinance any further after threats of NRA lawsuits (Mascia and Brownlee, 2018b).

Moderate preemption measures affect how ordinances are passed and enforced in various ways. Most states have exceptions for preemption and allow municipalities to pass ordinances that meet the criteria. These are narrow in scope and primarily concern the regulation of firearms on municipal government property, zoning firearm-related business, and the discharge of firearms. Additionally, 12 states allow municipalities to pass ordinances identical in scope and punishment to a standing state law (Bureau of Alcohol, Tobacco, Firearms and Explosives, 2022). These laws allow local resources to go towards enforcement of laws, and not solely rely on the state. Lastly, preemption may cause ordinances to primarily serve a symbolic purpose. Lincoln, Nebraska, outlawed bump stocks, which were already federally banned after the 2017 Las Vegas shooting. The city council members who passed it called the move "largely symbolic" and meant to signal their commitment to reducing gun violence (Young, 2018). Some ordinances are passed not to be enforced, but to express the community's attitude towards firearms and reveal preferences to leaders in state government (Sieger, 2015; Effingham, 2021).

2.3. Grassroots Activism

The 2010s were plagued with a series of public shootings. On February 14th, 2018, the gun control debate reached a shifting point after the school shooting at Parkland High School in Parkland, Florida. On that day, 17 people died and 17 more people were injured. The 2018 Parkland shooting garnered drastically more media coverage compared to other mass shootings in the 2010s. In the three months following the tragedy, news articles mentioning gun control-related terms appeared 2.5 times more often than after previous shootings. For the following week, stories that covered gun control accounted for 4% of all news stories from mainstream media outlets (Nass, 2018). Additionally, the Parkland shooting stayed in the news cycle for longer than other recent mass shootings. The increased news coverage was partly due to the activism of gun control advocates. (LaRose et al., 2021; Nass, 2018).

2.3.1. Women's March Youth EMPOWER

Exactly one month after the shooting, Women's March Youth EMPOWER (WMYE) organized the National School Walkout at 10:00 am local time. WMYE was formed as an extension of the Women's March after the leaders recognized the value of youth engagement in social movements. Their responsibility was to engage younger women and get them politically active in the women's movement.[3] WMYE is an intersectional organization that advocates for the well-being of women in a variety of ways. The primary focus of the Women's March was areas that directly affect women such as reproductive rights. However, the agenda states that women have "intersecting identities and are therefore impacted by a multitude of social justice and human rights issues." Their focus on intersectionality drives their interest in firearm violence, as they state "Women have the right to live full and healthy lives, free of all forms of violence against our bodies" (Women's March, 2018). Therefore, organizing the walkout aligns with WMYE's overall mission to better the living conditions of women.

To prepare for the protest, WMYE launched a website where students could register their event and download a two-part tool kit that gives guidelines for a successful demonstration. The tool kit provided a unifying message for students to rally behind. This consisted of recommended ways to organize and a list of topics to discuss with each other and community leaders. The protest was driven by frustration of inaction in the aftermath of the Parkland school shooting. However, WMYE claims that the movement "aims to highlight the need to prevent all acts of gun violence including those that happen on our city streets, in our homes, in our places of worship, and in our schools" (Women's March, 2018), so the movement aimed to address gun violence as a broader public health issue.

WMYE did not want the walkout to be a one-day event where people lost their drive to initiate change once the day ended. To keep the momentum strong, WMYE partnered

[3] The URL for their website is https://www.womensmarch.com/enough. This link is longer active, but the web page can be accessed using the Wayback Machine. The sources in this section were taken from the March 14, 2018, 11:58:06 snapshot accessed on 08/11/2024.

with 77 local grassroots organizations to sponsor events and get the word out. These organizations included gun-specific groups such as the Coalition to Stop Gun Violence and Maine Gun Safety Coalition but also included more general organizations that are invested in public health. Two organizations they explicitly direct activists towards on their website are the Town Hall Project and Indivisible (Women's March, 2018). Both organizations have websites where people can find local town hall meetings and provide a platform for people to communicate about local activism. WMYE also provided a link to register to vote on their web page to encourage activism through formal political channels (Women's March, 2018). This connection between the movement and grassroots organization established a pathway for people to become more active in local politics by participating in the walkout. MWYE is fundamentally a youth group whose goal is to encourage youth activism. They asked only students and staff to engage in the walkout for safety reasons. MWYE leaders specifically asked for school staff and adults to be supportive, but not take active roles in organizing and leading discussions about gun policy to promote mentorship and coordination between students (Women's March, 2018).

The effort organized by MWYE and carried by students was followed by a shift in gun policy discussion by local leaders. Figure 7 shows an approximate 200 percent increase in the number of firearm-related city council meetings that was not predicted by a trend. The walkout likely contributed to this increase. In 2018, mayors made a clear stand for an increase in gun control at the United States Conference of Mayors. This organization was founded to assemble local leaders to develop and promote urban policy, strengthen federal-city relationships, equip mayors with leadership tools, and provide a forum for idea exchange (**?**). In June of 2018, 1,408 mayors represented their cities at the event. The Council President, Steve Benjamin, stated that guns were a "top priority" of mayors, and the local governments were on the front lines of the issue. At the conference, they advocated for increasing local power to fight firearm violence, citing Parkland as the impetus for push (Blannelberry, 2018).

2.3.2. The Protest

On March 14, 2018, approximately 1.4 million people demonstrated in over 4,400 demonstrations (Crowd Counting Consortium). Some demonstrations garnered attention from prominent community leaders. Notably, New York Governor Andrew Cuomo marched with students in Zuccotti Park in Lower Manhattan. Other New York students demonstrated in prominent public spaces such as Columbus Circle, Battery Park, Brooklyn Borough Hall, and Lincoln Center (Barnard et al., 2018). Students in Boston marched to the state capitol and partnered with organizations such as the Massachusetts Coalition to Prevent Gun Violence to discuss ways to prevent school shootings and more generally, stop gun violence in the streets (Buell, 2018). Other examples of students engaging with community leaders were in Oakland (Jones, 2018), Philadelphia (Bontje, 2018), and Seattle (Barnard et al., 2018). The demonstrations during this walkout were not solely limited to the parking lots and football fields of schools. Rather, some of these students took the time to engage with community leaders to advocate for increased gun control.

Students were not fully free to engage in, or not engage in, the protests. Schools reacted differently to their students leaving class in the middle of the day to protest. Some schools went to great lengths to minimize protest turnout by threatening students with unexcused absences, detentions, or suspensions. Some administrations even contacted lawyers to determine how the school could legally punish protesters (Barnard et al., 2018). Other schools actively supported their students participating and teachers/administrative employees participated with them. In extreme examples where over two-thirds of the student population participated, teachers were reminded that students should not be punished for choosing not to participate (Sasko, 2018). Depending on the school, students potentially had external pressures to participate or not participate in the protests.

3. Data

3.1. Firearm Mortality

I use restricted access data from the National Vital Statistic System containing detailed mortality data to create firearm fatality outcomes. The raw data are at the individual fatality level. Each observation has a code for the cause of death. Firearm deaths are categorized as one of the following; suicides, homicides, accidental discharges, and legal intervention[4]. I sum the total number of annual deaths by county of incidence to create a county-year outcome of firearm mortality. Deaths from legal intervention and accidental discharge make up a small proportion of firearm deaths or just over 1% of total firearm deaths each in 2019. For this reason, I use total gun deaths, homicides, and suicides as my primary outcomes to measure the prevalence of firearm deaths.

The restricted access data contains information about the deceased person's age, race, sex, and educational level. To test if ordinances heterogeneously affect different groups, I create outcomes that separate deaths by these categories. Race is separated into Black Non-Hispanic, White Non-Hispanic, Hispanic, and other. The educational level is separated into no high school, high school, some college, and college. I can combine any combination of these four categories with the three types of firearm deaths for a variety of possible outcomes. The total number of gun deaths is the baseline outcome, but I use these additional outcomes to test the heterogeneous effects of ordinances on different groups.

3.2. Gun Ordinance

The longitudinal dataset on US municipal gun ordinances is a novel contribution of this paper. The ATF periodically produces a PDF for each state that compiles all state laws and municipal ordinances to inform citizens of the firearm laws they are governed by. Through a

[4]ICD-10 Codes for firearm deaths are coded as follows: W32-W34 - Accidental discharge of a firearm; Codes X72-X74 - Intentional self-harm by firearm; X93-X95 - Assault by firearm; Y35.0 - Legal intervention involving firearm discharge.

Freedom on Information Act request, I collect each state-year-level document for each state for 2008, 2011, 2016, and 2019. Each separate ordinance is titled with a short description. For example, see the beginning of Spokane County, Washington's section in Figure 2. I code this as three separate ordinances because Section 6.05.050, Section 6.05.070, and Section 6.05.080 describe three distinct unlawful activities that would all be described as "prohibited acts". Using text scraping methods, I create a dataset where each row begins with the municipality's name, then all following columns are the ordinance titles. This process is convenient because each state-level document reports ordinances similarly, so I use the same method for each document.

Firearm mortality data are aggregated at the county level. To create a single dataset, I attribute a primary county to each city and aggregate the number of municipal ordinances within each county. I use 2019 as the follow-up period, and 2016 as a measure for baseline ordinances. The primary outcome is the number of ordinances, but I also create the number of municipalities with any ordinances, the number of city ordinances, and the number of county ordinances. Figure 3 maps the total number of ordinances at the county level. Approximately 78% of counties have zero ordinances, which tend to be rural counties with low populations and small death counts. The average number of ordinances per county is 2.7 with the maximum being 532 in Los Angeles County. I manually verified every municipality with ordinances in the data and sampled a subset to ensure the data scraper accurately captured all ordinances for those municipalities.

3.3. Protest Turnout

The Crowd Counting Consortium collects information on crowds in the United States, including marches, protests, strikes, demonstrations, and riots. Observations primarily come from an automated web crawler that continuously searches the internet for news that searches for the previously mentioned events. Once events of interest have been identified, human coders review gathered information and record data about the events. The data recorders at the CCC use a macro event variable to link affiliated movements movements[5]. To construct outcomes, I keep all events associated with the National School Walkout macro event. Eight events with that macro event label happened on the following day. These observations are dropped from the analysis. Each observation has an estimated number of protesters. I use the average of all provided estimates if an event has multiple turnout estimates.

From the CCC estimates, there are 4,467 distinct protests recorded in 792 counties with an estimated total of 1,389,166 protesters. While these were mostly student-led protests, some adults did participate. Therefore, the crowd estimate is not fully composed of students. However, since parents were encouraged not to participate directly with the students, it is reasonable to assume that the estimated turnout captures almost entirely student participation. According to the National Center for Education Statistics, there were 50.6 million K-12 students in 2018 (National Center for Education Statistics, 2018). If we assume that 90% of protesters were students, then 2.4% of students protested on this day. Given that turnout was mostly driven by high school students, the percentage of high school students who participated is likely even more than that.

3.4. Rainfall

Weather data are from the National Oceanic and Atmospheric Administration (NOAA). I pull data from all active US weather stations which measure daily rainfall. Each weather station is attached to a latitude and longitude, which I can use to assign the station to a county. Most counties have multiple stations within them. To calculate a measure of county rainfall, I average daily rainfall over all stations in a county. Some counties do not have any stations, which are generally very low-populated and rural counties. This data restriction leaves 2,585 counties in the reduced form and 2SLS analysis. The average rainfall on March 14, 2018, was 0.05 inches with a standard deviation of 0.20 inches. See Figure 5 for a rainfall map on March 14, 2018. The West Coast and New England received the most rain, and

[5] The Macroevent for the National School Walkout is "20180314-nationalschoolwalkout".

parts of the Mid West received some rain. For a point of reference, I also include a map of the historic average March rainfall between 1900 and 1999 at the county level. Figure 6 shows that the West Coast and Gulf Coast areas tend to get more rain, while the Great Plains and areas east of the Rocky Mountains tend to not experience much rain.

3.5. County Characteristics

I include a set of 2016 county-level characteristics in all regressions. These data are provided by the MIT Election Lab. They construct a dataset with data from the 2012-2016 ACS 5-year estimates. These data include variables on race, education, income, and political leanings composition. The dataset is titled "U.S. General Elections 2018 - Analysis Dataset" and can be downloaded at the MIT Election Lab's website.[6]

4. Empirical Strategy

To estimate the relationship between firearm ordinances and firearm deaths at the county level, I use the following equation:

$$\text{Gun Deaths}_i = \beta_0 + \beta_1^{OLS}\text{Ordinances}_i + \mathbf{X}_i\beta + \lambda_s + \epsilon_i \tag{1}$$

The vector of covariates comes from the MIT Election lab described in Section 3.5, $\mathbf{X}_i$, and includes variables that are the percent of the population by race, percent that voted for Hillary Clinton in the general 2016 election, median household income, percent foreign-born, percent without a college degree, and percent unemployed. I also include the lagged value of firearm deaths; the number of firearm deaths in a county is very auto-correlated. The firearm literature views gun violence as a difficult metric to change, and counties have varying levels of firearm violence, even after adjusting for population.

[6]I last accessed the data on 9/11/2024 using the following link: https://github.com/MEDSL/2018-elections-unoffical/blob/master/election-context-2018.md

The primary challenge in estimating the effect of municipal ordinances on firearm deaths is the self-selection into passing the ordinances. Since firearm ordinances are passed to reduce gun crime or incidents, ordinances are anticipated to decrease firearm violence. However, cities that experience more gun violence are reasonably more likely to pass ordinances in the first place. If there a community does not experience firearm violence, they may not think there is a reason for an ordinance. This reverse causality between ordinances and firearm deaths makes any estimation using OLS likely biased. However, I begin with an OLS estimation to get a baseline estimate and measure descriptive relationships.

4.1. IV Strategy

I use rainfall on the day of the National School Walkout to exploit exogenous variation in the number of ordinances in a municipality. In Section 2.3.1, I describe how the movement was meant to engage people in their local governments to address firearms. However, I cannot use turnout data from the protest as exogenous, as protest turnout is likely driven by local factors which also affect the likelihood of passing ordinances. Then, protest turnout may only have reflected preexisitng beliefs about firearms and did not drive change in beliefs. Then, I follow the single-day protesting literature and use the day of protest rainfall as an exogenous source of variation in protest turnout. If protesting affected ordinances, then there is a causal chain linking rainfall and ordinances. In this analysis, the first stage is the effect of the day of protest rainfall on ordinances. While protest turnout does not directly enter into the first stage equation, I assume rainfall's effect on ordinances is solely driven by an exogenous variation in protest turnout.

The first condition that the rain instrument must satisfy is the relevance condition. In this context, rainfall on March 14, 2018, decreases the number of ordinances in 2019. As previously mentioned, rainfall affects ordinances through variations in the National School Walkout. I follow the established literature that documents people are less likely to attend outdoor events if it rains (Madestam et al., 2013; Hungerman and Moorthy, 2023; Madestam and Yanagizawa-Drott, 2012). If a protest only happens on a single day, then rainfall decreases participation compared to a day where it didn't rain. I interpret a significant first stage as rainfall on March 14, 2018, affecting the number of ordinances in a municipality through rain's effect on turnout in the National School Walkout.

The second condition that rain must satisfy is the exclusion restriction, meaning that rainfall does not belong as a control in the second stage model. In other words, single-day rainfall can only be related to firearm deaths through the number of gun ordinances. Rainfall on a given day should not have any effect on local legislation. A primary concern is that rainfall is not completely random; Seattle and Albuquerque have much different likelihoods of rain. A single day of rain can be correlated with historical rainfall, which can affect or be correlated with many characteristics that determine local legislation. To satisfy the exclusion condition, I control for county-level historic March rainfall. I include a set of county controls and state-fixed effects to better balance rainfall and establish it as good as random variation. I provide evidence for these assumptions in the following Section.

4.2. Identifying Equation

The first stage equation is as follows:

$$\text{Ordinances}_i = \gamma_0 + \gamma_1 \text{Rain}_i + \gamma_2 \text{Historic Rain}_i + \mathbf{X}_i \Gamma + \tau_s + \upsilon_i. \tag{2}$$

The outcome, Ordinances_i, is the number of ordinances in county i. Rain_i is the studentized value of rain in inches. Then, γ_1 is the coefficient of interest and is interpreted as the effect of a 1 standard deviation increase in rainfall on the number of gun ordinances. Historic Rain_i is counties i's average March rainfall between 1900 and 1999 in inches. The county controls from the MIT Election Lab or included in the vector, $\mathbf{X}_i$.The state-level fixed effect, τ_s, controls for any state-level common factors between counties such as state policy. For example, the number of laws passed at the state level may influence a municipality's likelihood of passing additional ordinances. The prior measure of ordinances in 2016 highly predicts the number of ordinances in 2019. To address autocorrelation, I report results for 1) a model with the change in ordinances as an outcome and 2) a model that controls for 2016 levels.

The second stage equation is as follows:

$$\text{Gun Deaths}_i = \beta_0 + \beta_1^{2SLS}\widehat{\text{Ordinances}}_i + \mathbf{X}_i\beta + \theta_s + \epsilon_i \tag{3}$$

The point estimate of interest is β_1^{2SLS} and under my 2SLS assumptions, I interpret it as the effect of an additional firearm ordinance on the number of gun deaths. $\widehat{\text{Ordinances}}_i$ is the predicted number of ordinances in county i from the first stage Equation 2.

5. Results

5.1. OLS

I begin the results section by reporting OLS results from estimating the relationship between ordinance and firearm deaths. Table 6 reports the point estimate of the number of ordinances on total deaths, homicides, and suicides. I estimate that an additional ordinance is associated withana increase of 0.05 firearm deaths per year. The standard error is 0.05, so I can rule out a relationship outside of 0.05 to 0.10. The number of suicides is the only outcome that has the expected negative sign and is statistically significant. All OLS estimates are tightly bound to zero, meaning that I can rule out large descriptive relationships once I control for county controls and state-fixed effects.

I estimate the relationship of ordinances on gun deaths for multiple subgroups described in Section 3.1. I separately estimate the OLS relationship using the number of deaths per subgroup in Figure 8. Of the 12 regressions in Figure 8, seven of the regressions have negative point estimates and 5 are positive. Only 3 of the regressions are individually significant at the 95% level. This type of analysis is subject to the multiple comparisons problem when I run the same regression but switch the outcome. I expect to get a type 1 error 5% of the time, so I am bound to find statistically significant relationships where there is none. To better understand the significance of the point estimates in Figure 8, I adjust the p-value for multiple hypothesis testing. The p-value to reject at the 95% confidence level after I use after implementing a Sidak correction is 0.004 [7]. The point estimates are mostly centered around zero when looking at the total number of gun deaths. Two point estimates that stand out for being large in magnitude are the number of White deaths and Black deaths. An additional gun ordinance is associated with a reduction in White deaths, but an increase in Black deaths. This may be driven by the fact that suicide and homicide have very different racial compositions. According to the CRC Wonder's restricted access data, 84% of suicides were committed by White people, and 59 % of homicide victims were Black. This is consistent with a positive point estimate in Table 6 for homicides, and a negative point estimate in suicides. Given that suicides and homicides are thought to have very different societal factors (Bender et al., 2015), ordinances may deferentially correlate with the two outcomes.

I also have gun ordinance data from 2008, 2011, and 2016. I run three more OLS regressions to gauge how the point estimate has changed over time. In Figure 9, I plot the point estimates for all the years. The point estimates in 2008 and 2011 are negative, similar to the 2019 estimate. The standard errors for the 2008, 2011, and 2016 regressions are approximately 7.5 times larger than the 2019 point estimate. From the data I obtained from the ATF, the standard deviation of the number of gun ordinances per county was only 2.8 as over 97% of counties had no ordinance in 2008. The maximum number of ordinances was just 58 ordinances in Law Angeles County. By 2019, the standard deviation of the number of gun ordinances per county increased to 20.2 as only 77% of counties had no ordinance, and the maximum number of ordinances increased to 532. The much smaller variance in the number of gun ordinance variables makes the point estimates less precise. As shown

[7] The Sidak correction calculates the new significance value when simultaneously running multiple regressions. The formula is $\hat{\alpha} = 1 - (1-\alpha)^{\frac{1}{m}}$ where α is the initial level and m is the number of regressions.

in Figure 1, the number of ordinances is roughly similar in the 2011 and 2016 regressions, which explains the large standard error in all three regressions. Given how large the standard errors are, the 2008, 2011, and 2016 regressions are not as informative because I cannot rule out large descriptive relationships.

5.2. First Stage

In the rest of this section, I report the first-stage and second-stage estimates from Equation 2 and Equation 3, respectively. Table 3 shows the reduced form point estimates for the effect of rainfall on ordinances. Columns 1 through 3 use the 2019 levels as the outcome and condition on the 2016 levels. Columns 4 through 6 use the change in ordinances between 2019 and 2016 as the outcome. Columns 3 and 6 include the full set of county controls and state-fixed effects, and represent the results from Equation 2. Standard errors are clustered at the state level to correct for any correlation between counties within a state. The point estimate of interest is the effect of one standard deviation of rainfall on the number of ordinances in a county. On March 14, 2018, the standard deviation of rainfall was approximately 0.2 inches. Depending on whether I control for 2016 levels or the difference in the outcome, a 1 standard deviation increase in rainfall decreases the number of ordinances by 0.90 to 1.55 ordinances.

In Section 2.2, I describe the policy environment around preemption; municipalities in states with punitive preemption may hesitate to pass ordinances in fear of the financial consequences of legal battles and court cases. Under these circumstances, we may expect that the first stage is weaker in states with punitive preemption. To test this, I reestimate equation 2, splitting the sample by punitive preemption status in 2018. Table 4 reports the first stages. In panel A, the sample is limited to counties in states with either no preemption or moderate preemption. There are only 9 states with no preemption, and some states have very few counties. For a large enough sample size for hypothesis testing, I group moderate preemption with no preemption. There is a similar pattern where the point estimate decreases with the addition of county controls and state-fixed effects. However, the point estimates in columns 3 and 6 are not as large in magnitude as in Table 3. Additionally, the point estimates are not as significant either. In Panel B, I limit the analysis to counties in states with punitive preemption. In this panel, the pattern of starting with a strong positive relationship in columns 1 and 4, and then ending with a strong negative relationship in columns 3 and 6 does not hold. In this analysis, there appears to be no discernible relationship between firearm ordinances and rainfall. This result suggests that municipalities are more hesitant to enact laws under punitive preemption. This may be due to the concern of being financially responsible for a lawsuit (Mascia and Brownlee, 2018b).

5.2.1. Covariate Balance of Rainfall

The point estimates change sign when covariates and state-level fixed effects are included in the model. The fact that covariates and fixed effects meaningfully change the point estimates implies that rain, unconditionally, is not as good as random. When I run a regression with rain as the outcome and county covariates as the controls with absorbing state fixed effects, I can reject the null that covariates are jointly unpredictive of rainfall. The F statistic of the null that all point estimates are zero is 2.3, which is larger than the critical value. To obtain a point of reference for this statistic, I randomize county rainfall within each state 200 times and obtain F statistics from each one. I plot the distribution of F statistics in Figure 10, where the dashed blue line is the F statistic from the true rain distribution, and the solid black line is the critical value to reject the null. The F statistic associated with the true rain distribution is the largest, and the only one above 1.8, so the distribution of rainfall on the day of the protest is different than other spatial realizations. The covariates that drive the high F statistic are the variables associated with race (White, Black, and Hispanic), and the percentage without a college degree. If I remove them from the model, the F statistic on the true rain variable looks like the F statistic from the placebo regressions. I show in Figure 11 the distribution of F statistics without the select controls.

While the F statistic of realized rain is approximately the 75th percentile of the distribution, it is below the critical value so we cannot reject the null that the joint county characteristics, conditional on state fixed effect, predict rainfall.

The omission of these county characteristics may be harmless if the first stage remains similar to the results in Table 3. I estimate the first stage models, but omit the race and percent less college and report the results in Table 5. The point estimates are very similar to the estimates with the full set of controls in Table 3. The point estimate in column 3 changes from -0.90 to -0.91, and the point estimate in column 6 changes from -1.55 to -1.58. These changes are very small in magnitude, and the point estimates maintain their statistical significance. The point estimates still decrease as controls and state-fixed effects are included, meaning those factors are important to the model. However, the F statistic analysis shows that I can jointly reject the null hypothesis that county and state characteristics predict rain.

5.2.2. Placebo Regressions

To ensure that the first stage captures a true relationship between rainfall and the number of ordinances in a county, I implement a set of placebo regressions. The protest was a single-day event and nationally coordinated in advance, so it could not have been locally adjusted to cater to the weather. If the rainfall operates through deterring protest turnout, then only the day of protest rainfall should negatively predict ordinances. I estimate Equation 2 365 times, using daily rainfall every day in 2018. Almost all of the estimations should yield null results. Figure 12 is a cumulative distribution function of the betas. I use the distribution to construct a two-tailed 95% confidence interval using the 2.5 percentile and 97.5 percentile. The day of protest rain measure, denoted with a black dashed line, is outside this constructed confidence interval. A few point estimates are similar in magnitude to the day of the protest estimate, which is driven by identical weather patterns. I use the set of placebo regressions to provide evidence that the results in Table 3 are capturing a casual relationship. After establishing a significant first stage, balancing rainfall with controls for good as random variation, and providing evidence for the importance of the day of protest rainfall, I continue to the 2SLS results.

5.3. 2SLS

Table 6 presents the 2SLS estimates. Each point estimate is the effect of an additional ordinance, instrumented with March 14, 2018 rainfall, on the number of gun deaths. The outcome in column 1 is the total number of gun deaths, column 2 is the number of firearm homicides, and column 3 is the number of firearm suicides. These point estimates are anticipated to have a negative sign, as the purpose of the ordinance is to decrease firearm deaths. However, only homicides have the expected sign. The point estimate in column 1 is interpreted that on average, an additional ordinance increases the number of firearm deaths by 0.07. The 95 % confidence interval contains -0.20. Using the largest negative value I cannot reject using the 95% CI, a county would need 5 ordinances to prevent 1 death. Less than 10% of counties have this many ordinances, so I interpret this as a small effect. To compare this to other papers, Loftin et al. (1991) find that an ordinance in D.C. decreased the number of deaths by 8 per year, so a 0.20 decrease is very small compared to this measure. The homicide and suicide estimates are also tightly bound to zero, meaning I can rule out any large effects of ordinances on any type of firearm fatality in the full sample. These results suggest that under my 2SLS assumptions, increasing the number of ordinances in a county does not decrease the number of gun deaths.

I estimate the effects of the ordinances in the 2SLS framework by state preemption. Similar to Table 4, I split the sample into punitive and non-punitive areas. Table 7 reports the point estimates for the regressions by sample. In Panel A, the point estimates are all larger than in the full sample regressions in Table 6. The interpretation of column 1 is that an additional ordinance increases the number of gun deaths by 0.21. This point estimate is marginally significant with a standard error of 0.11. This does allow me to rule out any negative effects of ordinances. The lower bound of the 95% confidence interval for homicides is -0.25, meaning that a city would need on average 4 ordinances to prevent a single homicide. The lower bound of the 95% confidence interval for suicides is -0.09, meaning that a city would need on average 10 ordinances to prevent a single suicide. While the estimates for counties in non-punitive states are a little less precise, I can rule out large negative effects of ordinances on all types of firearm fatalities. Panel B only includes counties from states that have punitive preemption. The point estimates are all much larger in magnitude. According to column 1, an additional ordinance increase leads to an increase of 4 firearm deaths. None of the point estimates are statistically significant as they all have standard errors larger than point estimates. The imprecision in Panel B is partly due to the low variation in ordinances in counties under punitive preemption. According to my gun ordinance data, only 162 counties had any ordinance in 2019, with the mean number of ordinances being 0.62.

Some states have preemption measures but allow cities to create ordinances that are identical in scope and punishment to the state law. For example, Wisconsin cities can pass laws that are the "same as or similar to, and no more stringent than, a state statute (Giffords Law Center, 2024)." Then, some firearm ordinances may not be effective under even moderate preemption because the passed ordinances may be enforced due to threats of lawsuits as they do not advance existing state or federal law. Therefore, preemption may hinder the ability of a passed ordinance to have any real effect.

California has always taken a stronger stance on gun control measures than other states. Many of the grassroots campaigns to enforce local gun control and advance violence prevention programs were started in California in the 1990s (Godwin and Schroedel, 2000). The state government has also contributed to fighting firearm violence at local levels. In 2007, California enacted the California Gang Reduction, Intervention, and Prevention Program. This program funded communities to locally combat gang violence. The program was expanded in 2017 with the establishment of the California Violence Intervention and Prevention Grant. Like its predecessor, this program funded communities to combat gun violence, but took a more holistic approach and viewed gun violence as a public health issue

(Board of State and Community Corrections, 2024). I test if California's history of devoting resources to locally preventing firearm violence translates to an increased effectiveness of ordinances. Given California's history of local dedication to preventing forearm violence through grassroots measures, it is the state where firearm ordinances may be the most strictly and faithfully enforced. Additionally, some of the biggest changes in the number of ordinances came in California, with Los Angeles County having the largest increase and the total number of ordinances. If there is to be any measurable effect happening, I expect it to be in California.

To test if ordinances are more effective in California, I do a separate analysis only using counties in that state. Table A reports the same 2SLS limited to the 56 counties in California. However, I use this exercise to focus on the magnitude of the point estimate compared to the full sample 2SLS. The point estimate in column 1 is interpreted as an ordinance, on average, decreases firearm violence by 0.45 deaths per year. This estimate is over double the lower bound of the full sample 2SLS sample. While a 0.45 death decrease is not very large, the confidence interval contains a decrease of 2.3 deaths per ordinance. Given a median of 5 ordinances per county, this translates to a median decrease of 11 deaths per year, which is a sizeable decrease. The lower bounds of the 95% confidence interval for homicides and suicides are -0.92 and -3.53, respectively. This suggests that most of the movement is coming from a decrease in suicides. Both of these point estimates are larger than their analogs in full 2SLS analysis. Due to the low sample size, I cannot reject the null hypothesis that the point estimates are zero. Rather, I use this exercise as suggestive evidence that ordinances may have an effect if the community is willing to invest in firearm violence prevention measures at the local level.

6. Conclusion

This paper presents new evidence on the impacts of firearm ordinances on firearm deaths.

Historically, student led movements have referred to college students, such as anti war protests during Vietnam or Civil Rights activism. The National School Walkout was the first time that highschool students led a large scale activist movement.

his paper opens the door for future work on firearm ordinances. A substation part of firearm ordinances is the preemption laws. For the states that do have some form of preemption, it is unclear how those ordinances move the needle on firearm policy. Many municipalities are restricted by law from enacting significant changes to firearm regulations, such as restrictions on firearm types, age limits for ownership, or red flag laws. Therefore, I cannot differentiate between ordinances not being effective because more firearm restrictions at the local level fundamentally do not prevent deaths or because firearm ordinances in most of the United States cannot truly increase firearm legislation past the state's legislation.

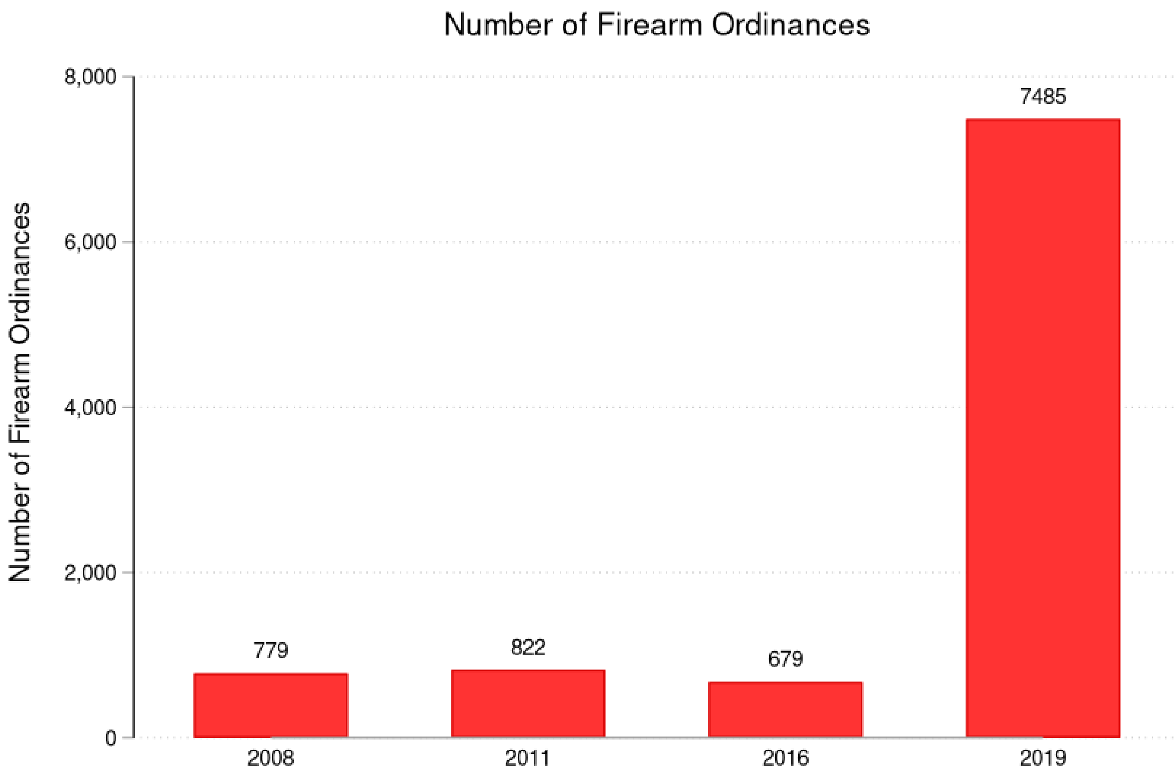
Regardless of which of these mechanisms contributes most to the observed ineffectiveness, firearm violence remains a pressing issue that policymakers on both sides of the aisle are eager to address. The failure of local ordinances to significantly reduce gun deaths suggests that other approaches might need to be considered. If future research supports this result, it could indicate that focusing on alternative prevention measures, beyond local-level legislation, might be more impactful. For example, programs like the California Violence Intervention and Prevention Grant, which allocates tens of millions of dollars toward youth prevention and community-based interventions, might represent a more promising avenue for reducing gun violence. Such programs aim to address underlying social factors contributing to gun violence, such as poverty, lack of education, and community disinvestment, which may ultimately have a more lasting impact than local regulatory measures alone.

Table 1: Common Ordinances Categories (2019)

Category	Number of Ordinances
Permits/Licenses	1,537
Sales/Transactions	1,295
Prohibited Actions	710
Minors/Age Restrictions	566
Definitions	530
Penalties	290
Zoning	229

Notes: Data comes from the ATF's "State Laws and Published Ordinances - Firearms", 33rd edition. Column 1 is a broad category that an ordinary can fit into. An ordinance is placed into a category if the ordinance's title contains that word or a synonymous word. Column 2 is the number of ordinances that fit into that category.

Fig. 1.



Source: Data are from the ATF's State Laws and Published Ordinances - Firearms editions that were published in 2008, 2011, 2016, and 2019. Each bar represents the total number of municipality-level ordinances are in that year's State Laws and Published Ordinances - Firearms edition.

Fig. 2. Firearm Ordinance Example

Spokane County Code of Ordinances

Current through Resolution 2019-0917, passed March 26, 2019. (Supplement 51, Update 1)

Title 6 – Police and Safety
Chapter 6.05 – Firearms and Weapons

Section 6.05.050. Persons prohibited from owning or possessing pistol.

No person who has been convicted in this state or elsewhere of a crime of violence, shall own a pistol or have one in his possession or under his control.

Section 6.05.070. Giving of false information prohibited.

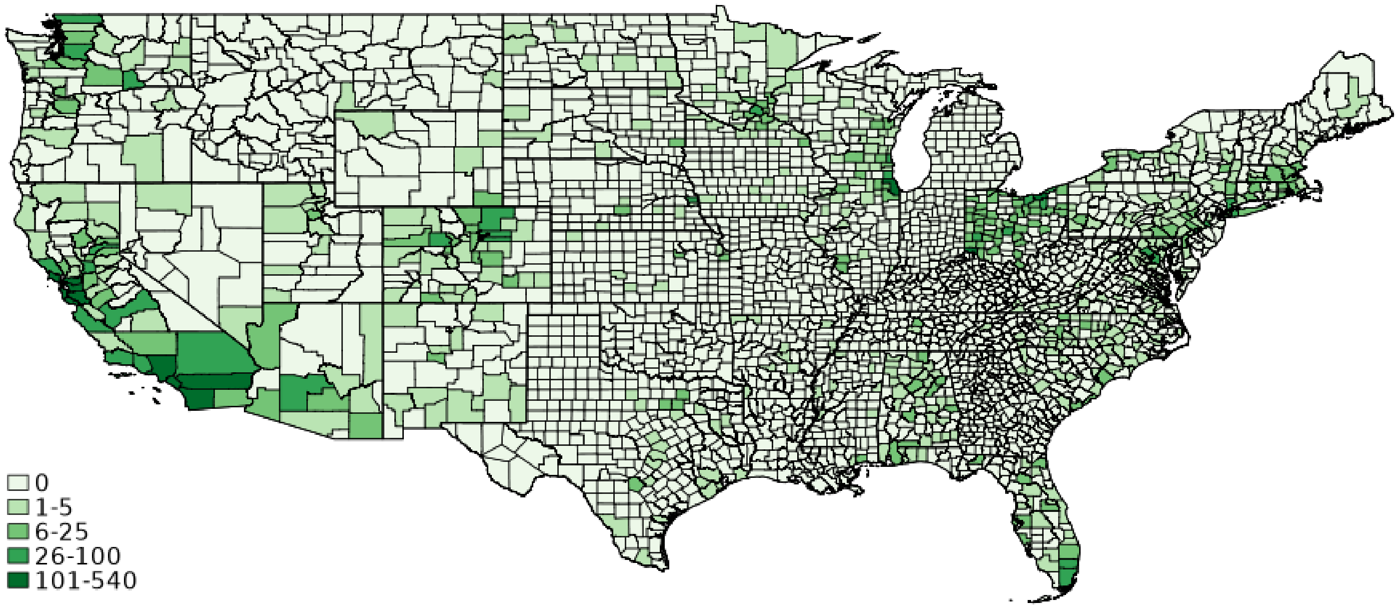
No person shall in purchasing or otherwise securing delivery of a pistol or in applying for a license to carry the same, give false information or offer false evidence of his identity.

Section 6.05.080. Alteration of identifying marks prohibited.

No person shall change, alter, remove, or obliterate the name of the maker, model manufacturer's number, or other mark of identification on any pistol. Possession of any pistol upon which any such mark shall have been changed, altered, removed, or obliterated, shall be prima facie evidence that the possessor has changed, altered, removed or obliterated the same; provided, this section shall not apply to antique pistols unsuitable for use as firearms and possessed as curiosities or ornaments.

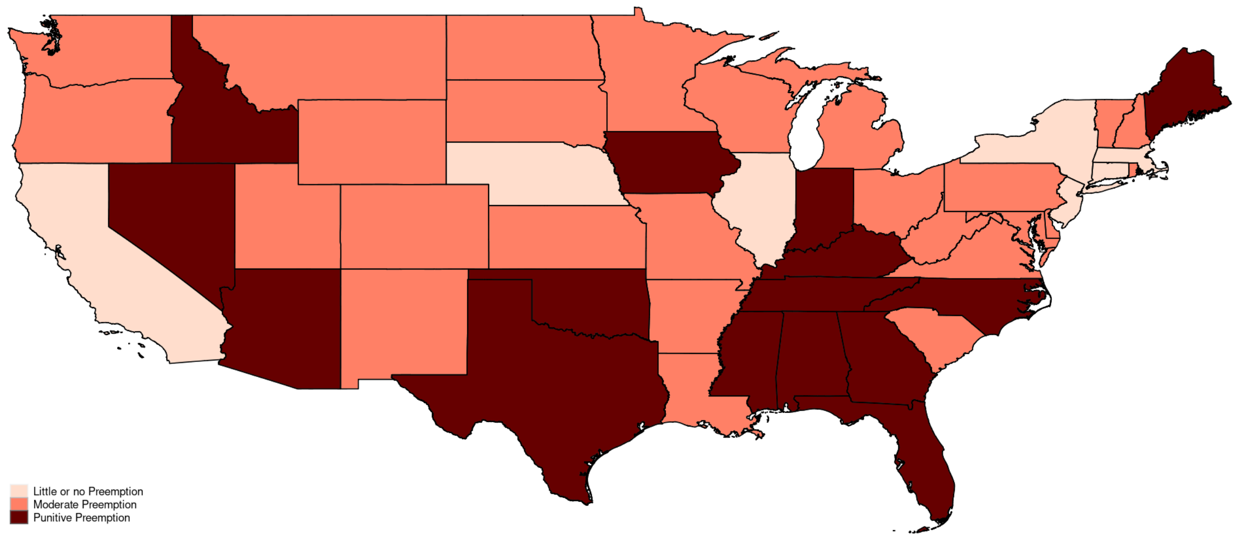
Notes: This excerpt is from the Washington state's document in the ATF's "State Laws and Published Ordinances - Firearms", 33rd edition. This document is used to create the ordinance measures described in Section 3.2. Each section is considered a distinct ordinance. The text after "Section" and the following section number are scraped as the ordinance title.

Fig. 3. Number of Ordinances per County, 2019



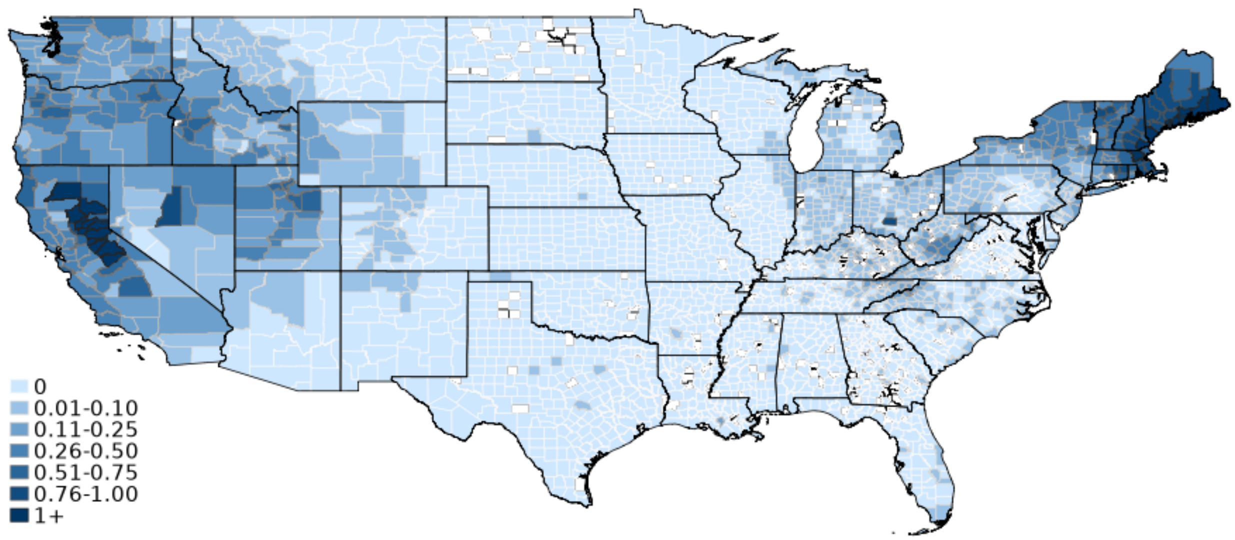
Notes: Data comes from the ATF's "State Laws and Published Ordinances - Firearms", 33rd edition. The color represents how many ordinances the cities within each county have. In cases where a city is in multiple counties, the city is assigned to a primary county.

Fig. 4. Firearm Preemption Map



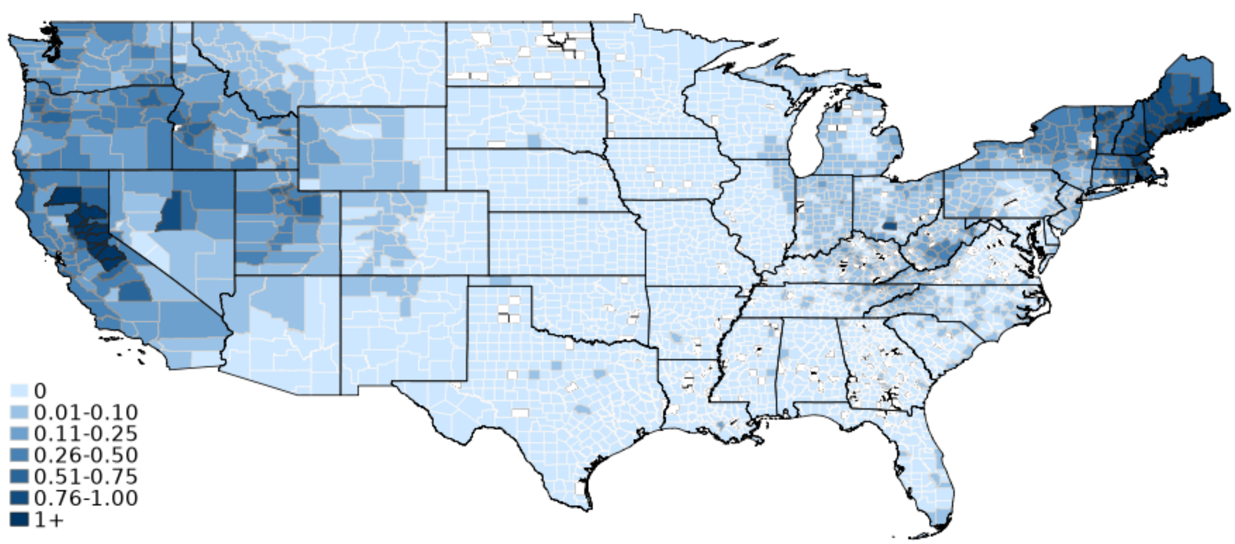
Notes: Data comes from Pomeranz et al. (2021). Little or no preemption means that a state does not preempt municipalities from passing ordinances, or the preemption is very narrow in scope. Moderate preemption means that a municipality does not have grounds to pass an ordinance, but cannot be sued for doing so. Punitive preemption means that a municipality does not have grounds to pass an ordinance, and can be sued for doing so.

Fig. 5. March 14, 2018 County Rainfall



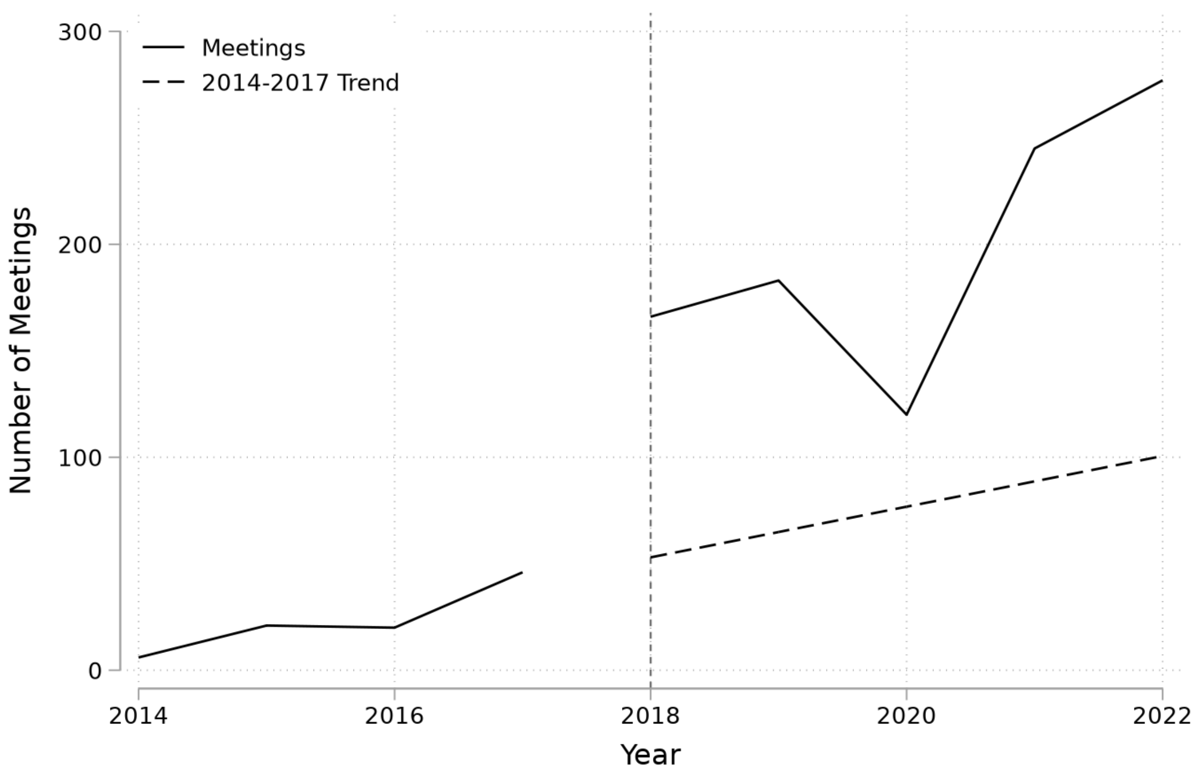
Notes: Data is from the National Oceanic and Atmospheric Administration. Each county value is the average daily rainfall of all weather stations within a county on March 14, 2018 in inches.

Fig. 6. Historic County Rainfall



Notes: Data is from the National Oceanic and Atmospheric Administration. Each county value is the average cumulative March rainfall in inches between 1900 and 1999.

Fig. 7. Number of Firearm-Related City Counsel Meetings



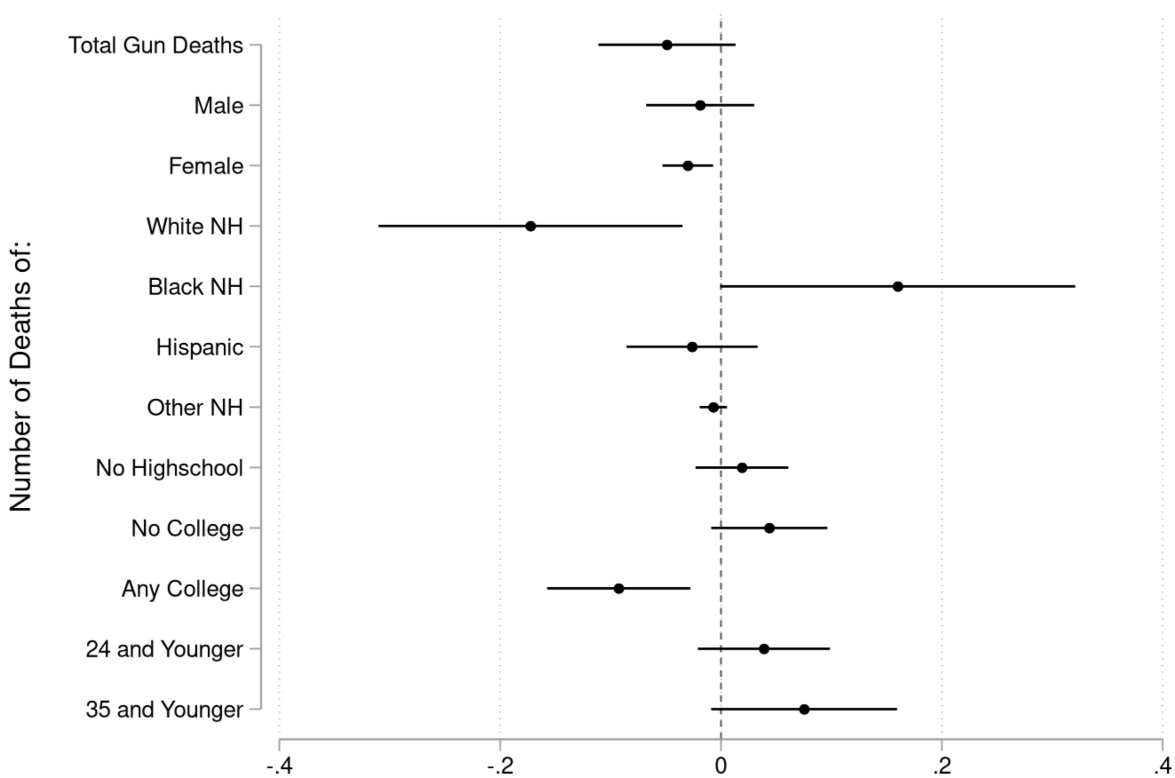
Notes: Data are from Localview. Each data point is a transcript of a city council meeting. The y-axis represents the number of meetings where gun control is discussed. The sample is 2,861 governments across 883 municipal and 129 county jurisdictions in all states except Hawaii.

Table 2: OLS: Firearm Deaths on Ordinances

	Total Deaths	Homicides	Suicides
	(1)	(2)	(3)
Number of Ordinances	0.05	0.14	-0.19**
	(0.05)	(0.08)	(0.09)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	2612	2612	2612

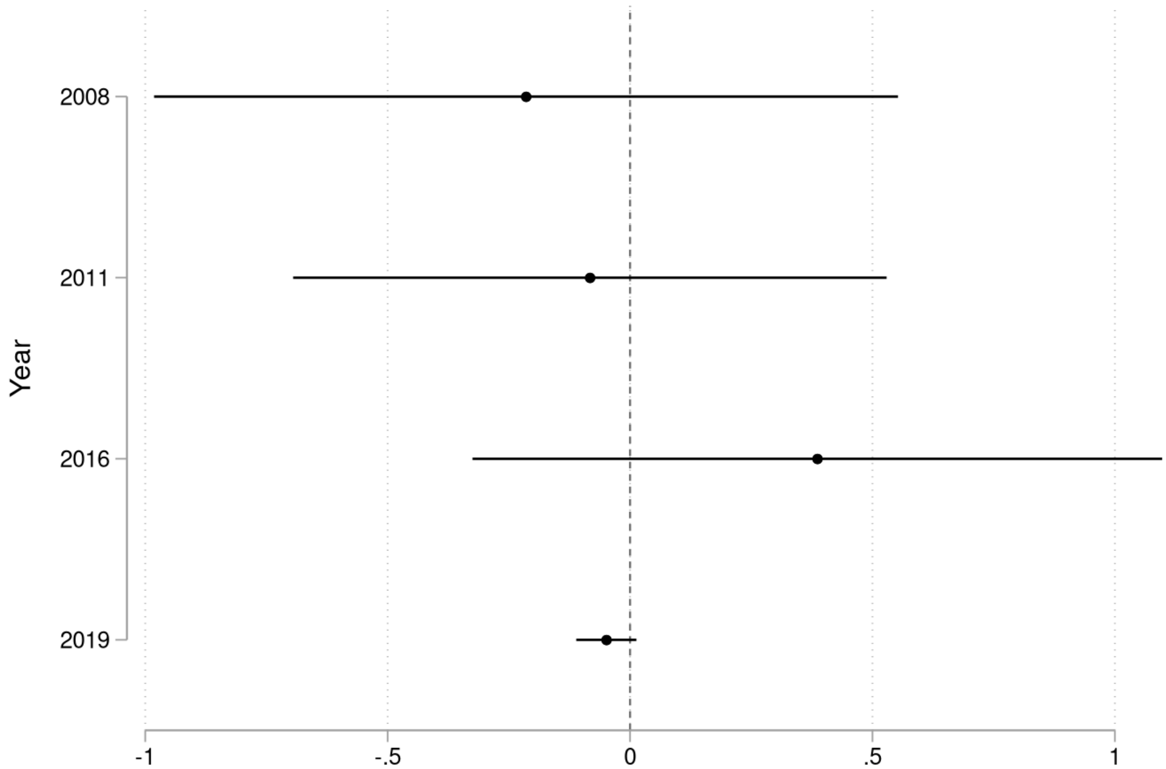
Notes: Each point estimate comes from a separate county-level regression. The outcome is the total number of gun deaths in 2019. The reported point estimate is the number of ordinances within a county. Each regression includes a set of county characteristics and state-fixed effects. Standard errors are clustered at the state level.

Fig. 8. OLS: Gun Deaths by Categories



Notes: Each point estimate comes from a separate county level regression. The outcome is the total number of gun deaths by demographic in 2019 according to the CRC. The plotted point estimate and 95% confidence interval come from the total number of municipal ordinances within a county in 2019. Each regression includes a set of county characteristics and state fixed effects. Standard errors are clustered at the state level.

Fig. 9. OLS: Gun Deaths by Year



Notes: Each point estimate comes from a separate county level regression. The outcome is the total number of firearm firearm deaths by year in 2019 according to the CRC. The plotted point estimate and 95% confidence interval come from the total number of municipal ordinances within a county in that year. Each regression includes a set of county characteristics and state fixed effects. Standard errors are clustered at the state level.

Table 3: Reduced Form - Number of Gun Ordinances on Rainfall

	Conditional on 2016			Difference in Levels		
	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall (SD)	1.74***	0.42	-0.90**	1.60***	0.39	-1.55***
	(0.42)	(0.30)	(0.45)	(0.38)	(0.33)	(0.48)
Total Ordinances 2016		3.44***	3.18***			
		(0.11)	(0.11)			
County Controls	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Observations	2494	2473	2473	2494	2473	2473

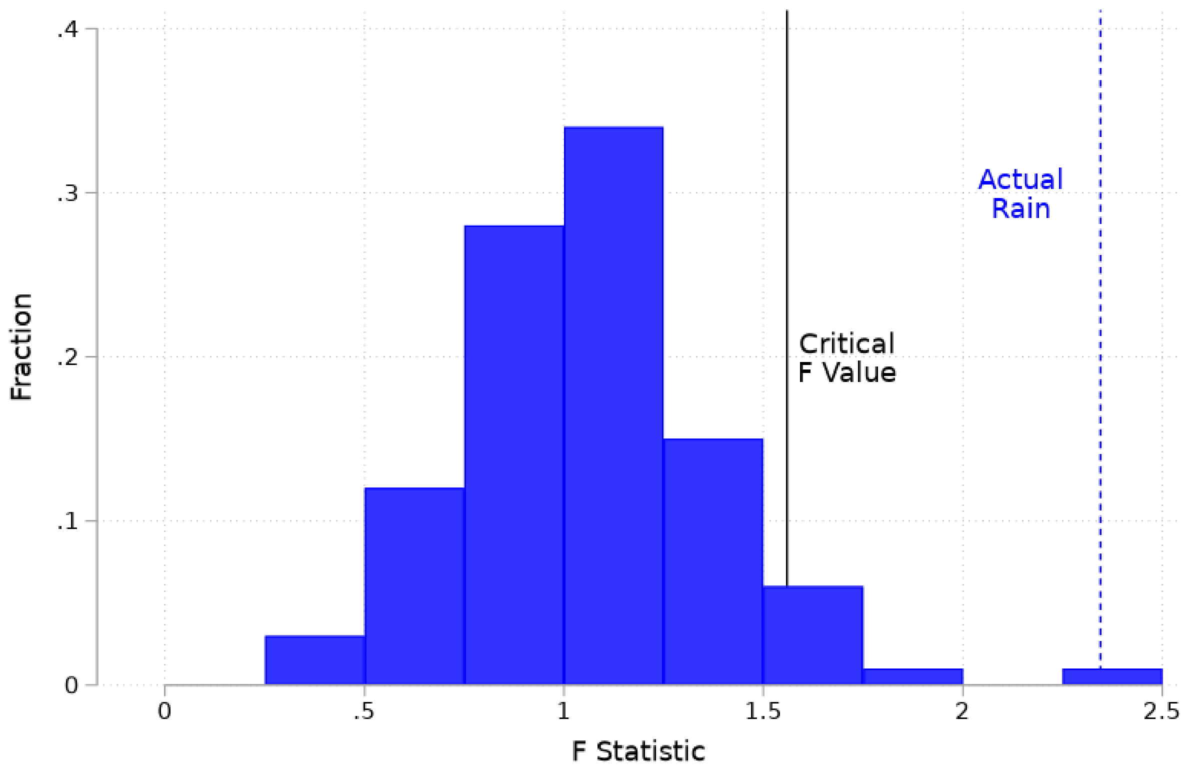
Notes: Each point estimate comes from a separate county-level regression. The outcome is the total number of firearm ordinances in 2019. Columns 1 through 3 are conditional on the number of 2016 ordinances in the regressions and columns 4 through 6 use the change in ordinances as the outcome. The point estimates and 95% confidence interval comes from the standardized amount of rainfall on March 14, 2018, in inches. Columns 3 and 6 include a set of county characteristics and state-fixed effects. Standard errors are clustered at the state level.

Table 4: Reduced Form -Number of Gun Ordinances on Rainfall, By Punitive Measures

	Conditional on 2016			Difference in Levels		
	(1)	(2)	(3)	(4)	(5)	(6)
Non-Punitive States						
Rainfall (SD)	2.07***	0.27	-0.73	1.91***	0.18	-1.19*
	(0.63)	(0.44)	(0.61)	(0.57)	(0.46)	(0.63)
Total Ordinances 2016		2.78***	2.55***			
		(0.15)	(0.15)			
County Controls	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Observations	1453	1432	1432	1453	1432	1432
Punitive States						
Rainfall (SD)	-0.16	-0.10	0.07	-0.15	-0.10	0.08
	(0.12)	(0.11)	(0.22)	(0.12)	(0.11)	(0.22)
Total Ordinances 2016		1.50***	1.54***			
		(0.17)	(0.17)			
County Controls	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Observations	1041	1041	1041	1041	1041	1041

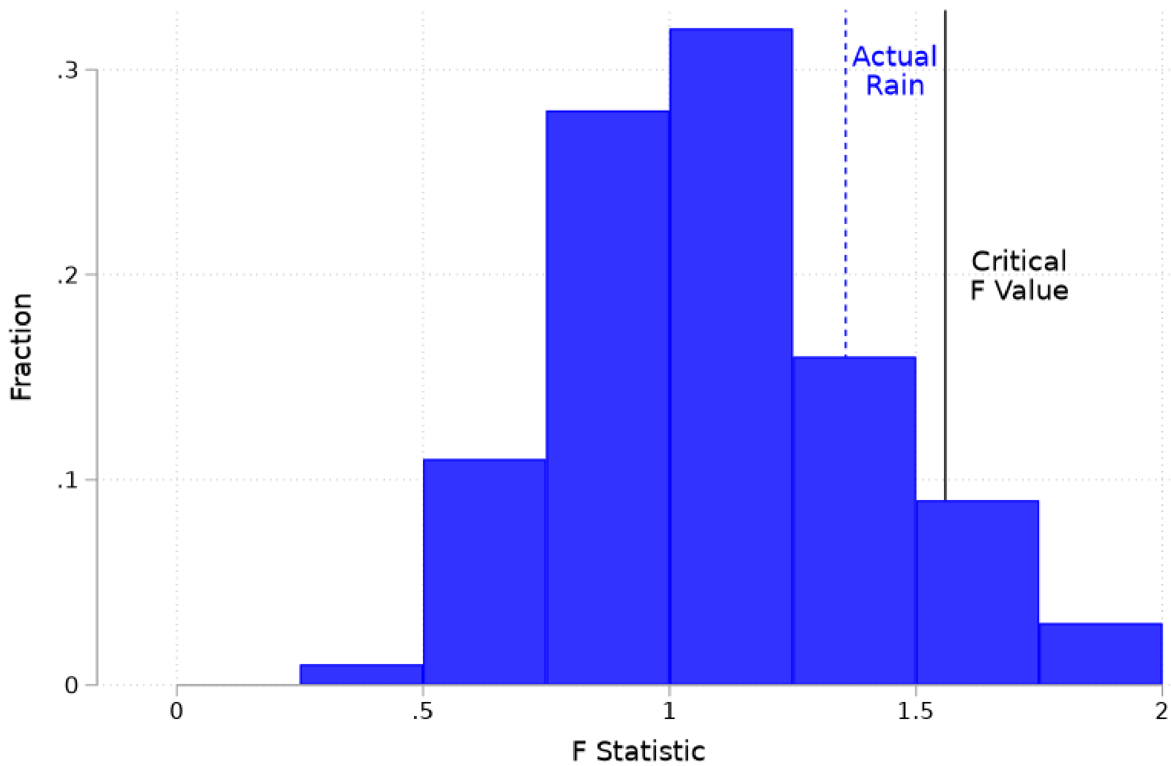
Notes: Each point estimate comes from a separate county-level regression. The outcome is the total number of firearm ordinances in 2019. Columns 1 through 3 are conditional on the number of 2016 ordinances in the regressions and columns 4 through 6 use the change in ordinances as the outcome. The point estimates and 95% confidence interval are the standardized amount of rainfall on March 14, 2018, in inches. Columns 3 and 6 include a set of county characteristics and state-fixed effects. Standard errors are clustered at the state level. Panel A contains all counties that are in states with no preemption, or non punitive preemption. Panel B contains all counties that are in states with punitive preemption.

Fig. 10. Rainfall Balance on County Characteristics



Notes: Each data point is the F statistic from a regression of March 14, 2018 rainfall on county covariates described in Section 3.5. The blue dashed line represents the F statistic associated with the true realization of rainfall. All other regressions randomize within state rainfall. The solid black line represents the critical value to reject the null hypothesis that the covariates are jointly insignificant.

Fig. 11. Rainfall Balance on County Characteristics, Balanced Controls



Notes: Each data point is the F statistic from a regression of March 14, 2018 rainfall on county covariates described in Section 3.5, except for the race variables and percent with less than a college education. The blue dashed line represents the F statistic associated with the true realization of rainfall. All other regressions randomize within state rainfall. The solid black line represents the critical value to reject the null hypothesis that the covariates are jointly insignificant.

Table 5: Reduced Form - Number of Gun Ordinances on Rainfall - Balanced Controls

	Conditional on 2016			Difference in Levels		
	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall (SD)	1.65***	0.60**	-0.91**	1.52***	0.57**	-1.58***
	(0.36)	(0.25)	(0.39)	(0.33)	(0.27)	(0.42)
Total Ordinances 2016		3.44***	3.20***			
		(0.11)	(0.10)			
County Controls	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Observations	2494	2473	2473	2494	2473	2473

Notes: Each point estimate comes from a separate county-level regression. The outcome is the total number of firearm ordinances in 2019. Columns 1 through 3 are conditional on the number of 2016 ordinances in the regressions and columns 4 through 6 use the change in ordinances as the outcome. The point estimates and 95% confidence interval comes from the standardized amount of rainfall on March 14, 2018, in inches. Columns 3 and 6 include a set of county characteristics and state-fixed effects. The controls do not include race characteristics, or the portion of adults without a college education. Standard errors are clustered at the state level.

Table 6: 2SLS - Firearm Deaths on Number of Ordinances

	Total Deaths	Homicides	Suicides
	(1)	(2)	(3)
Number of Ordinances	0.07	-0.04	0.10
	(0.12)	(0.13)	(0.16)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	2585	2585	2585

Notes: Each point estimate comes from a separate county-level regression. The outcome is the predicted total number of gun deaths in 2019 that come from Equation 2 The reported point estimate is the number of ordinances within a county. Each regression includes a set of county characteristics and state-fixed effects. Standard errors are clustered at the state level.

Table 7: 2SLS: Firearm Deaths on Ordinances, By Preemption Status

	Total Deaths	Homicides	Suicides
	(1)	(2)	(3)
Non-Punitive			
Total Ordinances	0.21*	0.02	0.18
	(0.11)	(0.14)	(0.14)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	1304	1304	1304
Punitive			
Total Ordinances	3.96	11.28	-6.60
	(8.13)	(14.58)	(9.93)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	982	982	982

Notes: Each point estimate comes from a separate county-level regression. The outcome is the predicted total number of gun deaths in 2019 that come from Equation 2 The reported point estimate is the number of ordinances within a county. Each regression includes a set of county characteristics and state-fixed effects. Standard errors are clustered at the state level. Panel A contains all counties that are in states with no preemption, or non punitive preemption. Panel B contains all counties that are in states with punitive preemption.

Table 8: 2SLS: Firearm Deaths on Ordinances, California Counties

	Total Deaths	Homicides	Suicides
	(1)	(2)	(3)
Number of Ordinances	-0.45	0.43	-0.79
	(0.95)	(0.69)	(1.40)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	56	56	56

Notes: Each point estimate comes from a separate county-level regression. The outcome is the predicted total number of gun deaths in 2019 that come from Equation 2 The reported point estimate is the number of ordinances within a county. Each regression includes a set of county characteristics and state-fixed effects. Standard errors are clustered at the state level. The sample are all counties in California.

Table 9: Population Weighted OLS: Firearm Deaths on Ordinances

	Total Deaths (1)	Homicides (2)	Suicides (3)
Number of Ordinances	-.002	.004	-.006**
	(.003)	(.002)	(.003)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	2612	2612	2612

Table 10: Population Weighted Reduced Form: Gun Death Rate on Rainfall

	Total Deaths (1)	Homicides (2)	Suicides (3)
Rainfall (SD)	.006	.004	-.058
	(.096)	(.062)	(.083)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	2612	2612	2612

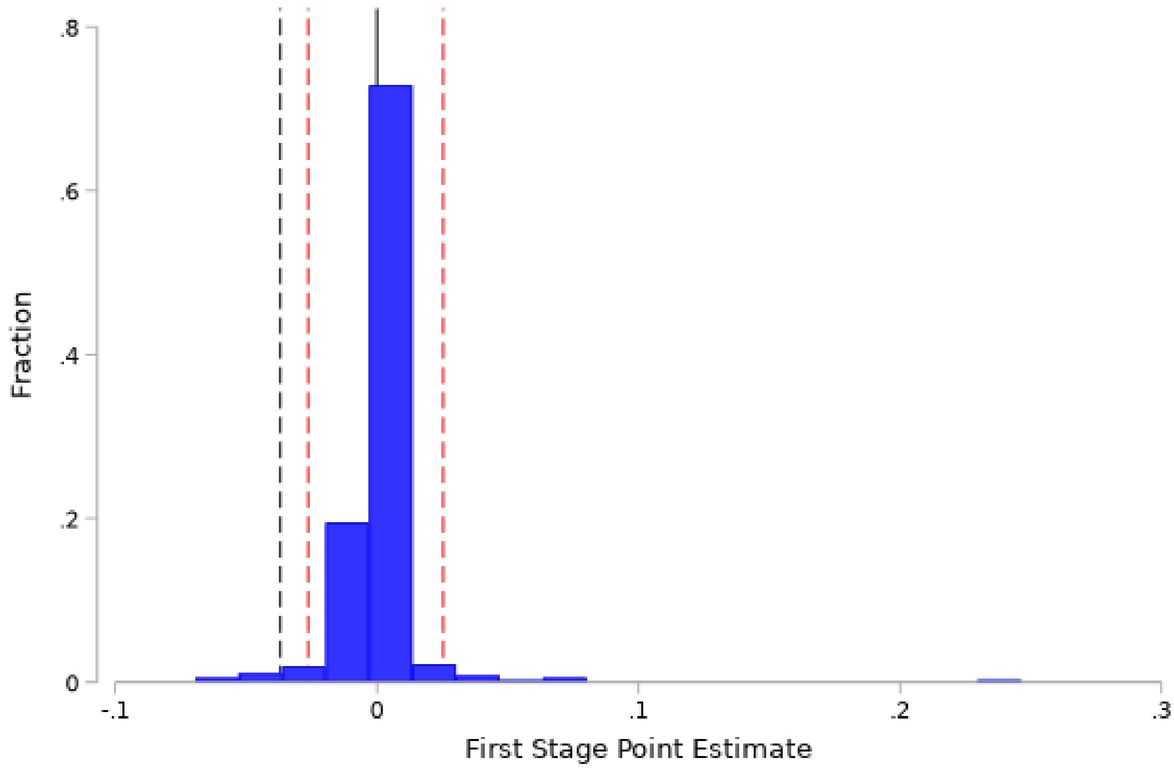
Table 11: Population Weighted 2SLS: Ordinances on Rainfall

	Total Deaths (1)	Homicides (2)	Suicides (3)
Number of Ordinances	-.001	-.000	.005
	(.007)	(.005)	(.007)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	2612	2612	2612

Table 12: Population Weighted 2SLS: Ordinances on Rainfall - California

	Total Deaths (1)	Homicides (2)	Suicides (3)
Number of Ordinances	-.009	-.001	-.009
	(.008)	(.004)	(.007)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	2612	2612	2612

Fig. 12. First Stage: Histogram of Placebo Regressions



Notes: Figure 12 graphs the distribution of the 365 point estimates for the first stage. Every regression uses Equation 2, but uses rainfall from every day in 2018 once. Each point estimate is separately estimated. The dashed black line is from the day of the protest. The red dashed lines represent the 2.5 percentile and 97.5 percentile of beta estimates. The solid black line is zero.

Table 13: Population Weighted First Stage: Ordinances on Rainfall

	Conditional on 2016			Difference in Levels		
	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall (SD)	12.58***	0.33	-0.54	11.37***	0.06	-10.60***
	(2.00)	(1.23)	(1.85)	(1.82)	(1.83)	(2.43)
Total Ordinances 2016		7.37***	6.62***			
		(0.12)	(0.13)			
County Controls	No	Yes	Yes	No	Yes	Yes
State FE	No	No	Yes	No	No	Yes
Observations	2478	2473	2473	2478	2473	2473

Appendix A. Appendix

Table A.1: Firearm Deaths on Prevention Spending, California Counties

	Total Deaths	Homicides	Suicides
	(1)	(2)	(3)
OLS			
Spending ($100,000)	-2.04*	-.11	-1.41
	(1.08)	(.99)	(.56)
2SLS			
Spending ($100,000)	-5.98	-3.00	-3.46
	(4.47)	(6.22)	(2.26)
County Controls	Yes	Yes	Yes
State FE	Yes	Yes	Yes
Observations	56	56	56

Notes: Each point estimate comes from a separate county-level regression. The outcome is the predicted total number of gun deaths in 2019 that come from Equation 2 The reported point estimate is the number of ordinances within a county. Each regression includes a set of county characteristics and state-fixed effects. Standard errors are clustered at the state level. The sample are all counties in California.

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