Gun Violence and Business Activity*

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Abstract

We study the relationship between gun violence and economic activity using data that contains the precise location and timing of 1 million shots fired and over 2 billion visits to 275 thousand businesses in high crime, urban neighborhoods. Our first finding is that common gun-violence proxies contain both classical and non-classical measurement error. This measurement skews both our understanding of who is impacted by gun-violence as well as its overall prevalence in lower-SES, urban areas. For example, our data reveal that 96% of all businesses in these neighborhoods are exposed to at least one shooting incident within a half-mile of their doorstep each month. Motivated by this fact, we next explore the causal effect of gun-violence on consumer foot traffic and revenue. We find little evidence that gun violence causes declines in business activity. Both a regression discontinuity design that aims to recover the short run impact of unexpected gun violence events, and an event study design that aims to capture the long run effects of compounding gun violence over time, yield null results. Our estimates are precise enough that they should reduce the uncertainty of a rational economist by 99% over the short-run impact of gun violence and by over 60% for the long-run impact of gun violence. Taken together, our results suggest that policies which address gun violence alone are unlikely to improve economic conditions in high-crime neighborhoods.

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1 Introduction

There is an epidemic of gun violence in the United States. Over the last 10 years, there have been over 160,000 gun related homicides, much of them occurring in large urban areas (Gramlich, 2026; Kegler *et al.*, 2022). At the extreme, cities like St. Louis, MO, and Birmingham, AL, have per capita rates of gun death that exceed those of other common lethal diseases like lung cancer.[1] The high cost of gun violence is also born inequitably. For example, gun violence is now the leading cause of death among young Black men (CDC, 2024) as well as among all children ages 1-17 (Johns Hopkins Center for Gun Violence Solutions, 2024).

Nationally, there is a strong association between gun victimization and poverty. This suggests that economic conditions could play an important role. Yet, despite this connection, a deeper exploration of the link between gun violence and it's impact on economic activity has proved elusive. The first challenge is that *actual* rates of gun violence are notoriously difficult to measure, with most individual gun shots going unreported (Carr & Doleac, 2016a). For that reason, researchers have often been forced to rely on imperfect proxies like gun violence injuries or killings (Parker, 2022; Bhatt *et al.*, 2024; Chiong *et al.*, 2025; Vachuska & Movahed, 2025). However, victimization data likely represents the tip of the iceberg. Even trained police officers, arguably the group with the most precise shooting skills, hit their target some 17-40% of the time (Donner & Popovich, 2019; Baker, 2007). The second challenge stems from the fact that simple cross sectional comparisons have the potential to be confounded. Does gun violence harm economic activity and cause underdevelopment? Or is it poverty itself and the associated lack of economic opportunity that creates conditions where gun violence thrives (Ludwig & Schnepel, 2025)? The answer to these questions are key to understanding the connection between the ongoing epidemic of violence and sustained economic prosperity.

We study the relationship between gun violence and economic activity using novel data that contains the precise location and timing of 1 million gun shots. We pair the gunshot data with data on economic activity that covers over 2 billion consumer store visits. Our data on shooting incidents and gunshots come from acoustic gunshot detection technology: a system of fixed array microphones that have been installed in high-crime neighborhoods across the United States in order to aid law enforcement agencies (Ang *et al.*, 2025). This data allows us to view, at an *extremely* fine level of temporal and spatial resolution, both the timing and extent of gun fire events for high-crime neighborhoods contained in 18 major US cities.[2] To measure economic activity, we unite the acoustically detected gunshot data with counts of consumer foot traffic measured via cell phone tower pings and, for a subset of stores, revenue. The combined data allows us to make an unprecedented exploration of the connection between gun violence and business activity.

We begin by descriptively characterizing the landscape of gun violence in high-poverty urban areas. Our

[1] Recent estimates suggest that the per-capita lung cancer morality rate in St. Louis is 64-69 per 100 thousand residents per year. For gun deaths, that number is 76.4. See https://www.stlouis-mo.gov/government/departments/mayor/initiatives/resilience/equity/opportunity/health-safety/mortality-rate.cfm and https://www.axios.com/2023/10/16/america-gun-deaths-crime-south for more detail.

[2] For the complete list of cities, see table A3

first descriptive finding is that commonly used measures, such as gun shot victimization and homicides, are poor proxies. For every victim of gun violence, there are at least 17 shooting incidents and over 83 individual shots fired. As a result of this mismeasurement, the correlation between rates of gun violence and victimization is only 0.049.

Notably, the measurement error is non-classical and skews our understanding of *who* is impacted by gun violence. For example, neighborhoods in the top quintile of 911 calls related to shootings or shots fired correctly classify neighborhoods in the actual upper quintile of gun violence as measured by the full, multi-year sample of gunshot data, less than 17% of the time. This is slightly *worse* than random. Other common proxies, like the shooting incidents cataloged in the Gun Violence Archive, are able to perform better; yet, they still only correctly identifying high gun violence neighborhoods 63% of the time. On the other hand, just one year of acoustically detected gunshot data correctly classifies high-violence neighborhoods 87% of the time. As a result, common proxies actually *understate* the degree to which gun violence is concentrated in high-poverty, Hispanic neighborhoods.

Descriptively, our gunshot data reveal that most businesses in high-poverty urban areas are regularly exposed to gun violence. The data contain information on consumer activity for 275,980 businesses, which are visited by an average of 61-157 consumers in a typical month. A typical gun violence incident in our data has 87 businesses located within a 0.5 miles radius of the incident. Moreover, fifty-eight percent of all businesses in areas with acoustic gunshot detection technology experience a shooting on their individual census block, effectively on their doorstep, in any given year. Zooming out to the broader, community-wide level, we find that 99% of all businesses are exposed to at least one shooting incident over the study period. Thus, depending on the definition of exposure, we find that either a majority of businesses or *virtually all* businesses in our sample are exposed to gun violence.

Motivated by this fact, we next explore the causal effect of gun-violence on short-run consumer behavior. To do so, we leverage the timing of gunshots in a regression discontinuity design. Intuitively, our design compares foot traffic patterns in affected blocks from just before a gun shot event to just after. If the precise timing of a shooting incident is orthogonal to other observed and unobserved determinants of consumer and business behavior, then this comparison will isolate the causal effect of gun-violence on economic activity.

We find that gun violence does not impact economic activity in the short-run. We find that a gun-shot incident causes a short-run increase in the likelihood of 911 calls for shots fired, the likelihood of a gunshot injury, and the likelihood of a homicide. Yet, the point estimates on foot traffic allow us to rule out short-run declines of more than 0.04%. For expected revenue, our estimates are precise enough to rule out changes of more than 37 cents per day. We find similar patterns when exploring more violent incidents involving multiple shots; incidents that result in victimization or homicide; and among incidents that occur in more or less violent neighborhoods. We also do not find evidence of meaningful heterogeneity by business type or by distance from the gunshot.

Why are consumers undeterred in the immediate aftermath of a gunshot? One possible explanation for this

null result could be due to the fact gun-shot detection technology is often installed in neighborhoods with above average rates of gun-violence at baseline. If residents of these neighborhoods are already numb from repeated exposure, then the elasticity of economic activity to a marginal shot will be small. This leaves open the possibility that escalating rates of gun violence over longer time periods could produce more dramatic declines.

To address this possibility, next we ask whether a sustained rise in gun-violence causes economic decline. In order to answer this question, we take advantage of the fact that, while the typical neighborhood with acoustic gunshot detection technology has an above average crime rate, that is not *uniformly* the case—in fact, there are neighborhoods in our data that experience sudden, escalating gun-violence from a low-base over our sample period. Thus, to isolate the impact of a sustained increase in gun-fire, we identify neighborhoods in our data that meet two criteria. First, we require the neighborhood to experience a sudden rise in gunfire, increasing by at least one standard deviation month-to-month. This criteria ensures the neighborhoods we view as "treated" experience a large, and sudden, rise in gunshots. Second, we require that the rise must be sustained, such that the elevated rate of gun-violence is maintained over the subsequent 12 months. This criteria rules out neighborhoods that experience a temporary flash of transitory violence. To build a counterfactual, we compare the trend in these neighborhoods with a sustained rise in gun-violence to the trend in neighborhoods that experience little change in gun violence over the same time period.

We do not find statistically precise evidence that a sustained rise in gun violence impacts economic activity. Our preferred specification suggests that a 1-SD rise in gun-violence reduces foot traffic by no more than 2.3% and expected revenue by no more than 1.9%. Importantly, we note that the trajectory of economic activity is comparable among treated and control neighborhoods prior to the rise in gun-violence. This suggests that our results do not reflect long-standing economic trends in neighborhoods about to experience a rise in gun-fire. To further probe the potential for reverse causality to generate these patterns, we also identify neighborhoods that experience sustained declines in foot traffic (defined identically to our preferred measure of gun violence). We find no evidence that sudden, sustained changes in foot traffic lead to more violence. These results are also robust to numerous placebo checks, alternative specifications, and falsification tests.

How precise are the null results? To answer this question, we elicited prior distributions over our effect sizes from 26 economists actively working in applied micro. Under the normative assumption that economists should rationally update their beliefs in light of new data, we show that a typical economist should reduce the uncertainty in their prior beliefs by 99% over the short-run impact of gun violence, and by 59% over the long-run impact. In fact, for most economists (58%) in our elicitation, the posterior mean of their updated distribution of beliefs lies *outside* the 90-10 interval of their prior distribution for both the short and long-run effects. Thus, the results are precise enough to substantially shape beliefs.

Taken together, the results from our causal analysis suggest that gun violence does not impact economic activity. This finding is important, since it suggest that policies which address gun violence alone are unlikely to improve economic conditions for individuals who live in high-gun violence neighborhoods. Instead, other forms of community investment, perhaps paired with interventions designed to address the

behavioral aspects of crime, could be key.

We advance the existing literature in multiple ways. When aiming to understand the impact gun violence has on the country, much of existing research focused on gun victimization. One strand of this literature examines the opportunities and policies focused on averting violent victimization, finding that on the enforcement side, policies like increased number of police officers reduce violence, while on the public's side, job training programs show substantial promise in reducing gun involvement (Mello, 2019; Bhatt *et al.*, 2024). Meanwhile, a second strand of this literature focuses on the costs of gun violence—predominantly examining mass shootings specifically—finding that the exposure to these events decreases a range of health outcomes (Currie *et al.*, 2023; Soni & Tekin, 2023). We advance this literature by examining the impacts of a more complete measure of gunfire.

Second, we advance the literature on determinants of economic activity in low income neighborhoods. This literature has predominantly focused on the effectiveness of place-based economic incentives, finding broadly mixed results in effectiveness (Neumark & Savych, 2022; Shybalkina, 2022). As for the role of public safety and the criminal justice system specifically in economic activity, limited evidence to date suggests that mass shootings have potential adverse impacts in local employment rates (Cuellar & Jung, 2025), and that gunfire reduced residential property values in one small- to mid-sized city (Locke *et al.*, 2024). We show that shootings incidents as well as level shifts in how shootings-heavy a neighborhood is have no impact on economic activity across sectors in a range of cities across the country, highlighting that existing gun violence policies, while valuable on their own right, cannot be held as policies that are actively addressing or improving economic conditions.

2 Data

2.1 Data Sources

To build our analytic sample, we rely on a number of data sources, which we describe below. For full detail on our data sources and sample construction, please see Appendix A.

SafeGraph Data. Our primary measure of economic activity is foot traffic to businesses. To build this measure, we use data from SafeGraph. This data is collected via cellphone apps covering approximately 7.5% of the US population (Li *et al.*, 2024). In terms of representativeness, a recent validation study found that the number of devices residing in an area is highly correlated with the number of residents based on the American Community Survey, and that biases for specific demographic groups are minimal over our sampling frame, ranging from -0.03 and +0.01 for every demographic category (Li *et al.*, 2024).[3]

From 2019-22, the data also contains measures of revenue for a subset of businesses. It is unclear what is the exact sampling frame Safegraph uses to determine the subset of businesses for which it collects revenue data; however, empirically we observe that this data is more likely to be available for large brands and

[3]Specifically, the device-population residence correlation is 0.99 in urban settings at the county, and 0.68 at the census tract level. Demographic groups are split into 23 categories across age, gender, race and ethnicity, education, and income groups.

national chains, which in total cover 24% of all businesses in the sample (see appendix table A1). For these businesses, the data provides information on credit and debit card purchases specifically, but not on other methods of payment, such as cash payments. Importantly, the dataset enables us to distinguish between card-based purchases made in person at the establishment versus purchases made online, which we exclude throughout.

The revenue data allow us to build a measure of expected revenue based on foot traffic. To build this measure, we regress monthly observations of spending on visits within 5-digit NAICS categories in each city. The result is a predictive model at the city-by-naics level which we then use to rescale store visits at the 5-digit naics level within each city into a measure of *expected revenue*. We prefer this proxy to the default measure of revenue since it is non-missing for every business in our sample. Importantly, because the composition of stores varies block-by-block within city, treatment effects on this variable are not a mechanical rescaling of treatment effects on store-visits once aggregated to different levels of geography (see discussion in section 2.2 for more discussion of how we aggregate the business level data into neighborhood measures of economic activity). However, in Tables 5 and A5, we show that our key findings hold, albeit with more noise, if we use the default measure of revenue instead.

Acoustic Gunshot Detection Data. Our primary measure of gunshot exposure is data generated from acoustic gunshot detection technology. This technology involves a series of permanently mounted sensors that have been installed by local law-enforcement agencies in high-crime, urban neighborhoods across the country (Ang *et al.*, 2025). These sensors use sound waves to triangulate the precise location of a gunshot via a process known as multilateration—in effect, the technology relies on delays in the speed of sound arriving to different monitors from the original source location to pin down the precise location of a shot. The technology works with a high degree of accuracy in controlled environments.(Maher, 2006; Loeffler & Flaxman, 2018; Mazerolle *et al.*, 2000; Watkins *et al.*, 2002; Goode, 2012; Irvin-Erickson *et al.*, 2017). Moreover, this technology is currently deployed in approximately 100 cities nationwide (Electronic Frontier Foundation, n.d.).

We acknowledge that the use of this technology by law enforcement has been controversial. While evaluations find relatively low rates of false negatives, with the technology able to detect over 80% of gunshots and to triangulate 91% of detected shots to within 40 feet of the actual location (Mazerolle *et al.*, 1999; Watkins *et al.*, 2002; Goode, 2012; Irvin-Erickson *et al.*, 2017), it is also prone to false positives, mis-classifying car backfires, helicopters, and fireworks as gunshots (Carr & Doleac, 2016b). Civil rights organizations have also raised concerns that the technology may serve as "a circular statistical justification for over-policing in communities of color" (Stanley, 2021). Lastly, critics have questioned whether the close relationships that exist between acoustic gunshot detection technology companies and law enforcement departments may compromise the integrity of the data and its admissibility as evidence in court cases.

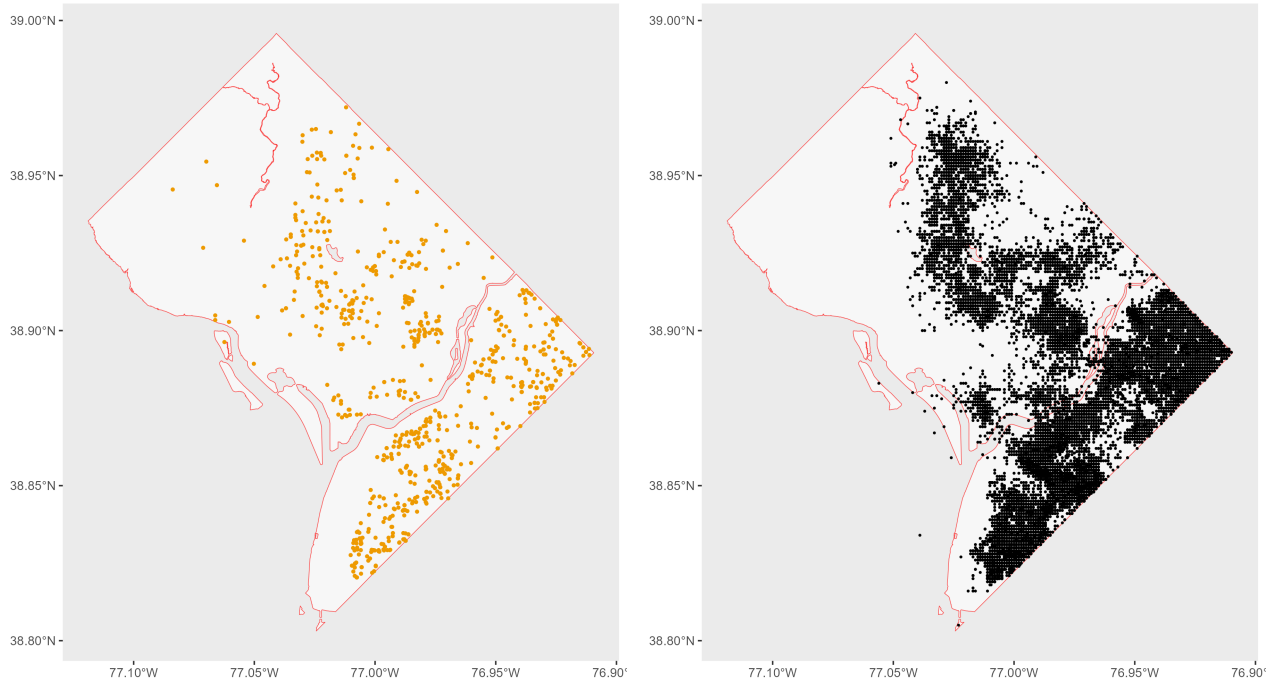
Notably, most of the controversy stems from how the technology is used by the police rather than pertaining to the underlying quality of the data itself. While the question of whether this data can be used effectively and fairly by law-enforcement as a crime fighting tool is important, it is also outside the scope of this paper. Instead, we simply note that there there is now a small body of academic research which relies on acoustic

gunshot detection data as an improved measure of violent crime—a testament to the data's potential quality and reliability when used responsibly (e.g. Ang *et al.*, 2025; Carr & Doleac, 2016b).

The acoustic gunshot detection data contains information on the exact time and location of every shooting incident that took place in the technology-covered areas in the study cities. These cities include Baltimore, MD; Boston, MA; Chicago, IL; Cincinnati, OH; Denver, CO; Fresno, CA; Hartford, CT; Newark, NJ; Oakland, CA; Pittsburgh, PA; Richmond, CA; San Diego, CA; and Washington, DC.

Figure 1 illustrates the data by displaying every gunshot detected in Washington, DC from 2018-2022, with a comparable map of homicides included as a benchmark. In addition, we note here that for eleven of the eighteen study cities, we also have information on the precise number of shots fired during each incident.[4]

Figure 1: The spatial distribution of gun homicides and shootings in Washington, DC



(a) Panel A: Gun Homicides **(b)** Panel B: Shootings

Notes: This figure illustrates the acoustic gunshot detection data. Panel (a) plots gun homicides in Washington, DC for the years 2018-2022 as recorded in the Gun Violence Archive. Panel (b) plots every acoustically detected gunshot incident over the same period.

We acknowledge that neighborhoods with shot-spotter coverage are selected, even within city. Generally,

[4]See Appendix table Table A3 for the list of cities with detected shots fired, and the city-specific temporal coverage of this data source.

law-enforcement will only cover specific parts of a given city due to the cost associated with the technology (Blackburn & Mares, 2019). Coverage areas in our sample range from a few neighborhoods to around half of a city.[5] The deployment neighborhoods are determined by law-enforcement, and tend to be found in more violent neighborhoods—a fact we will document empirically when we discuss summary statistics for our sample later on in this section.

Additional data sources. We supplement the Safegraph and acoustic gunshot detection data with a number of additional data sources. Our first supplementary data source is the Gun Violence Archive. The Gun Violence Archive collects the largest national collection of recorded instances of fatal and non-fatal violent gun victimization across the US, with exact information on the location and time of the incident, "from over 7,500 law enforcement, media, government and commercial sources" (Gun Violence Archive, n.d.). We use this to build measures of gun violence victimization. Our next supplementary data source comes from 911 Calls for Service. We use this data to build measures of civilian reported gunfire. Occasionally, 911 calls for service datasets will also include observation rows for shots fired based on acoustic gunshot detection monitors. Where present, we exclude these detection monitor-generated observations from both measures. Our third supplementary data source is the American Community Survey 2018-2022 and Decennial Census 2020. We use these data sources to measure the demographics of cities and neighborhoods in our sample. In all cases, more detail on the definitions of the exact variables built from these supplementary sources can be found in Appendix A.

2.2 Sample Construction

Baseline analysis file. To build our analysis file, we begin by aggregating the Safegraph data for our 18 analysis cities. Specifically, we reshape the data to the business-date level and aggregate by summing the visit counts over census-block dates. We then perform a nearly identical procedure with the acoustic gunshot detection data to generate a file of gunshot counts at the census-block-date level. The primary difference between the Safegraph aggregation procedure and the gunshot aggregation procedure is that the gunshot data arrives to us at the incident level with precise time-stamps. For that reason, we define a day in the gunshot file to run from 7am to 7am, which will result in early morning gunshots (e.g. 2am) being attached to the night-time period during which they occurred. We unite the data sets by merging at the census block by date level. Supplementary data (e.g. from the ACS and census) are merged on similarly. For additional details on the aggregation and merging procedure, see Appendix A

Short-run analysis sample. To build the short-run analysis sample we use for the regression discontinuity design, we begin by restricting the data to census blocks that are covered by acoustic gunshot detection technology. Next we aggregate the data into a panel of gunshot incidents that describes store visits in a thirty day window before and after each gunshot event. Specifically, we define a block as treated on date t if at least one gunshot incident occurs on that date. From there, we find every other block whose centroid lands within a 0.5 mile ring and aggregate visit counts across all blocks within that ring to the daily level,

[5] In our sample, coverage area is lowest in San Diego, covering 1% of the city, while on the other end of the spectrum, Chicago, Hartford, Newark and Oakland have at least 50% of the city covered.

retaining only the 30 days before and after the shot.[6] We repeat this procedure for each gunshot event in our data. The final result is an analytic sample where the unit of observation is an incident-by-date panel that describes business visits in a 30 day window around each event. See Appendix A for more detail on how this file is built.

Long-run analysis sample. To build the long-run analysis sample we use for the event study, we begin by taking the analysis file and collapse it to the block-month level by summing across gunshots and store visits. We then compute the monthly exposure of each block to gunshots in blocks whose centroid lands within a 0.5 mile ring around the focal block, and restrict the sample to blocks where at least 80% of the blocks in the 0.5 mile ring are covered by sensors. To define treatment events, we look for blocks where there is a sudden and sustained jump in gunfire exposure from one month to the next. Specifically, treatment events are those where a block experiences a greater than 1 standard deviation increase (1,948 events in our data) in incidents within the 0.5 mile ring from one month to the next, and the average of gunfire exposure during the 12 months after the event onset is 1 standard deviation larger than the average of gunfire exposure in the 12 months leading up to the event. We define control blocks as those with minimal change in gunfire exposure, specifically blocks where the standard deviation across the 24 month treatment window is less than a quarter of the global standard deviation. Armed with these treatment and control blocks, we stack the data for each of the treatment onset dates, where each stack contains all treated blocks who share that event onset date and all control blocks that are flat in exposure over that period. Within each stack, we then restrict the sample so that treatment and control blocks have non-zero visits in all analysis periods.

2.3 Summary Statistics

Table 1 gives descriptive statistics for our data. Column (1) provides summary statistics for every block-month in all 18 of our analysis cities. Column (2) restricts the data to block-months covered by the acoustic gunshot detection technology. Column (3) reports summary statistics at the block-day level for the sample of blocks contained in the bandwidth of our regression discontinuity design. Column (4) reports summary statistics at the block-month level for the sample of blocks used to estimate the event study model. Note that we refer to the sample in column (4) as "stacked" since we organize the data by treatment timing cohorts and control blocks can be repeated across stacks. The unit of observation used to build this table is a block observed at a given unit of time; therefore, averages and standard deviations (reported in parentheses) represent a typical census block as observed during a randomly chosen moment in time (i.e. day or month) over the course of our sample frame. Panel A describes neighborhood demographics. Panel B describes gun violence. Note that we do not observe sensor recorded gunshots for blocks that are not covered by the technology; therefore, we cannot report summary statistics for these variables in column (1). Panel C describes our key outcome variables from the Safegraph data.

The city blocks in our data broadly resemble large U.S. cities in their racial and socioeconomic composition, although Black residents are relatively more prevalent and Hispanic residents relatively less prevalent

[6] In sensitivity analysis, we also explore different definitions of exposure by varying the distance we use for aggregation (e.g. a 0.25 mile or a 1 mile ring).

Table 1: Summary statistics

	(2) Our cities	(3) Covered blocks	(4) Short-run sample	(5) Long-run sample
Panel A. Demographics				
Population	81	73	129	97
	(137)	(97)	(208)	(153)
Percent white	36.8	14.4	9.0	22.7
	(30.9)	(20.9)	(15.3)	(23.7)
Percent Black	27.1	46.7	60.0	45.9
	(33.6)	(37.9)	(35.4)	(35.8)
Percent Hispanic	23.6	30.1	23.2	19.7
	(25.7)	(32.0)	(29.4)	(24.7)
Poverty rate (%)	13.6	21.0	26.5	19.1
	(16.3)	(18.2)	(19.0)	(19.6)
Median household income ($)	71,738	50,154	40,911	60,245
	(43,265)	(28,854)	(21,986)	(37,266)
Panel B. Gunfire				
Gunfire incidents (AGD)		0.125	0.024	0.142
		(0.428)	(0.184)	(0.481)
# Gunshots (AGD)		0.396	0.053	0.227
		(2.029)	(0.673)	(1.102)
Shooting incidents (GVA)	0.004	0.009	0.001	0.012
	(0.068)	(0.097)	(0.032)	(0.115)
Victims killed (GVA)	0.0012	0.0023	0.0003	0.0037
	(0.0373)	(0.0509)	(0.0174)	(0.0639)
Victims injured (GVA)	0.0041	0.0088	0.0010	0.0120
	(0.0827)	(0.1214)	(0.0402)	(0.1425)
Panel C. Outcomes				
Businesses open	1.13	0.77	1.13	2.58
	(3.46)	(1.84)	(2.30)	(2.88)
Visits	227.1	153.0	9.3	608.0
	(1,684.3)	(1,392.2)	(80.3)	(3,516.2)
E[Revenue] ($)	6,374	4,202	144	5,569
	(48,738)	(23,423)	(322)	(33,261)
Unit of observation	Block-month	Block-month	Block-day	Block-month (stacked)
Observations	8,745,360	2,562,360	19,119,894	1,683,325
Blocks	145,756	42,706	39,745	4,410

Notes: This table gives descriptive statistics for our data. Column (1) provides summary statistics for every block-month in all 18 of our analysis cities. Column (2) restricts the data to block-months covered by the acoustic gunshot detection technology. Column (3) reports summary statistics at the block-day level for the sample of blocks contained in the bandwidth of our regression discontinuity design. Column (4) reports summary statistics at the block-month level for the sample of blocks used to estimate the event study model. Note that we refer to the sample in column (4) as "stacked" since we organize the data by treatment timing cohorts and control blocks can be repeated across stacks. The unit of observation used to build this table is a block observed at a given unit of time; therefore, averages and standard deviations (reported in parentheses) represents a typical census block as observed during a randomly chosen moment in time (i.e. day or month) over the course of our sample frame. Panel A describes neighborhood demographics. Panel B describes gun violence. Note that we do not observe sensor recorded gunshots for blocks that are not covered by the technology; therefore, we cannot report summary statistics for these variables in column (1). Panel C describes our key outcome variables from the Safegraph data. See Appendix A for more detail on the data sources, construction, and specific variable definitions.

in our sample. Column (1) reveals that the typical block in our sample is 36.8% non-Hispanic White, 27.0% non-Hispanic Black, and 24.0% Hispanic, with a median household income of approximately $72,000. For comparison, among residents of the ten largest U.S. cities, approximately 30.8% are non-Hispanic White, 18.2% are non-Hispanic Black, and 36.0% are Hispanic, while the median of these cities' median household incomes is approximately $79,000 (U.S. Census Bureau, 2024). Thus, while our sample is not a demographic cross-section of large U.S. cities, it captures economically and racially diverse urban neighborhoods with demographic characteristics broadly comparable to those found in major U.S. cities.

Neighborhoods covered by acoustic gunshot detection technology tend to be struggling economically and have more violent crime. Column (2) reveals that median household income in a covered block is $50,000, roughly 30% lower than a typical neighborhood. Moreover, the number of gun violence victims-per-capita (as reported by the gun violence archive) is over twice as large in areas with acoustic gunshot detection technology when compared to a typical city block. While we cannot benchmark it to non-covered areas, these high-rates of violence are also reflected in the sensor detected gunshots, with a typical block in a covered area, itself a very small geographic unit with only about 73 people on average, experiencing a gunshot incident on their doorstep roughly once per year. These neighborhoods also have less business activity, with fewer businesses being open, less consumer foot traffic, and lower expected revenue than a typical block in our 18 city sample.

Finally, we note that our short- and long-run analaysis samples have significantly more crime than the typical covered block. This is by construction: in both cases, we build the sample with the intention of examining the causal effect of gun-fire, a process that results in disproportionate representation of neighborhoods that are "treated" or at-risk of treatment by gun-fire. Interestingly, this yields a short run sample that is of even lower-income than a typical covered neighborhood. This is because the sample is heavily weighted towards blocks that experience a gunshot on their doorstep. On the other hand, the long-run sample actually has slightly higher income than a typical covered neighborhood. This is due to the fact that the control group is comprised of neighborhoods that do not experience large changes in gun violence over this period, and hence skewed towards areas with less acoustically detected gunshots per-capita than a typical covered block—this can by seen by dividing the average number of gunshots by the average population for the covered neighborhoods and long-run sample contained in table 1.

3 The Landscape of Gun Violence

In this section, we descriptively explore the connection between gun violence and economic activity. As noted in the introduction, a key advantage of this study is our ability to measure gun violence. Prior research has been forced to rely on imperfect proxies (e.g. Parker, 2022; Bhatt *et al.*, 2024; Chiong *et al.*, 2025; Vachuska & Movahed, 2025). However, it is ultimately an empirical question whether this measurement issue is small and easily ignored or large and impactful. We find that relying on common proxies leads to a large amount measurement error, and that this mismeasurement can skew our understanding of who is impacted by gun violence. To highlight the power of the acoustic gunshot detection data to improve our

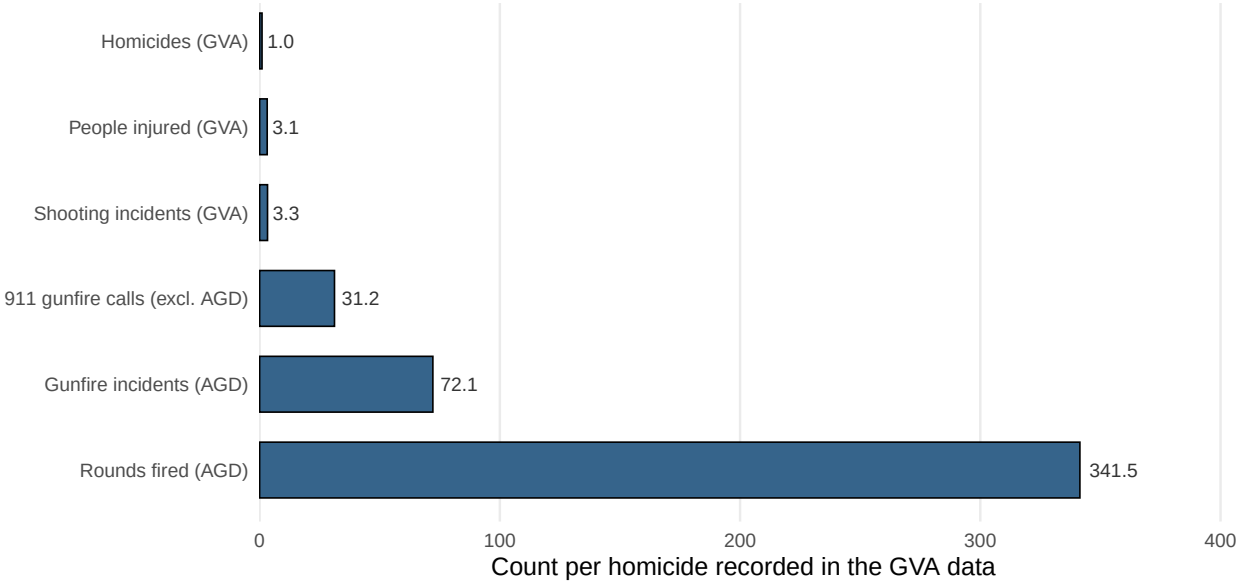
understanding of gun violence, we end this section with a new descriptive fact that motivates our causal analysis: nearly all businesses in neighborhoods with acoustic gunshot detection technology are impacted by gun violence.

3.1 Common gun violence proxies are poor and can skew our understanding of who is impacted by gun-violence

We begin with a simple accounting exercise. Suppose we did not have access to the acoustic gunshot detection data and were instead forced to rely on measures of gun violence commonly available to researchers: firearm homicides (e.g., Cook & Ludwig, 2006), gunshot injuries (e.g., Dalve *et al.*, 2021), shooting incidents compiled from media, police, and other public reports (e.g., Donohue *et al.*, 2024), or 911 calls reporting gunfire (e.g., Braga *et al.*, 2001). How much gun violence would we miss?

We find that common proxies like homicides and gun-violence injuries are just the tip of the iceberg. Figure 2 plots the ratio of various measures of gun-violence to gun homicides as recorded in the Gun Violence Archive data. Thus, the figure gives a visual representation of the extent to which common gun violence proxies understate its prevalence.

Figure 2: Common gun violence proxies are just the tip of the iceberg



Notes: This figure gives a visual representation of the extent to which common gun violence proxies understate it's prevalence. Specifically, each bar denotes the ratio of a gun-violence measure (X-axis) to gun homicides as recorded in the Gun Violence Archive (GVA) data. Injuries and incidents are also measured from the Gun Violence Archive. Emergency medical system (i.e. 911) calls for gun-fire are taken from publicly available 911 call logs. Gunfire incidents and rounds fired are measured with acoustic gunshot detection (AGD) technology.

We find that for every homicide, there are over 72 shooting incidents and 342 shots fired. Proxies other than homicide also perform poorly. For example, injuries and incidents from the gun violence archive understate the number of actual incidents by a factor of 20 and gunshots by a factor of 100. 911-calls for gunfire

incidents perform better then casualties or homicides, but continue to exhibit substantial mismeasurement, capturing less than half of all gunfire incidents and understating the number of shots fired by a factor of 10.

As a result, the standard proxies are rife with measurement error. Figure 3 scatters the logarithm of acoustically detected gunshot counts (Y-axis) against the logarithm of a common proxy (X-axis).[7] The top left panel gives the correlation between homicides and actual gunshot incidents. The top right panel gives the correlation between reported gun violence injuries and actual gunshot incidents. The bottom left panel gives the correlation between gun violence archive incidents and actual gunshot incidents. The bottom right panel gives the correlation between 911 calls and actual gunshot incidents. The dashed line denotes the 45 degree line, centered at the mean of the data. The red line denotes the line of best fit. The slope and R^2 are reported in the bottom right hand corner.

If the proxies accurately measured gun violence, and hence had little measurement error, we would expect all of the dots in figure 3 to lie on the 45 degree line. As is clear from Figure 3, that is not the case. To begin, we note that in all cases the slope coefficient is statistically different from 1, and hence we can reject that the proxies accurately capture the mean relationship of gun-violence. Moreover, there is also wide dispersion around the best fit line, with the proxies explaining, at most, 48% of the variation in actual gun violence and almost none of the points actually landing on the 45 degree line. Put differently, Figure 3 shows that the proxy variables commonly have large and systematic measurement errors.

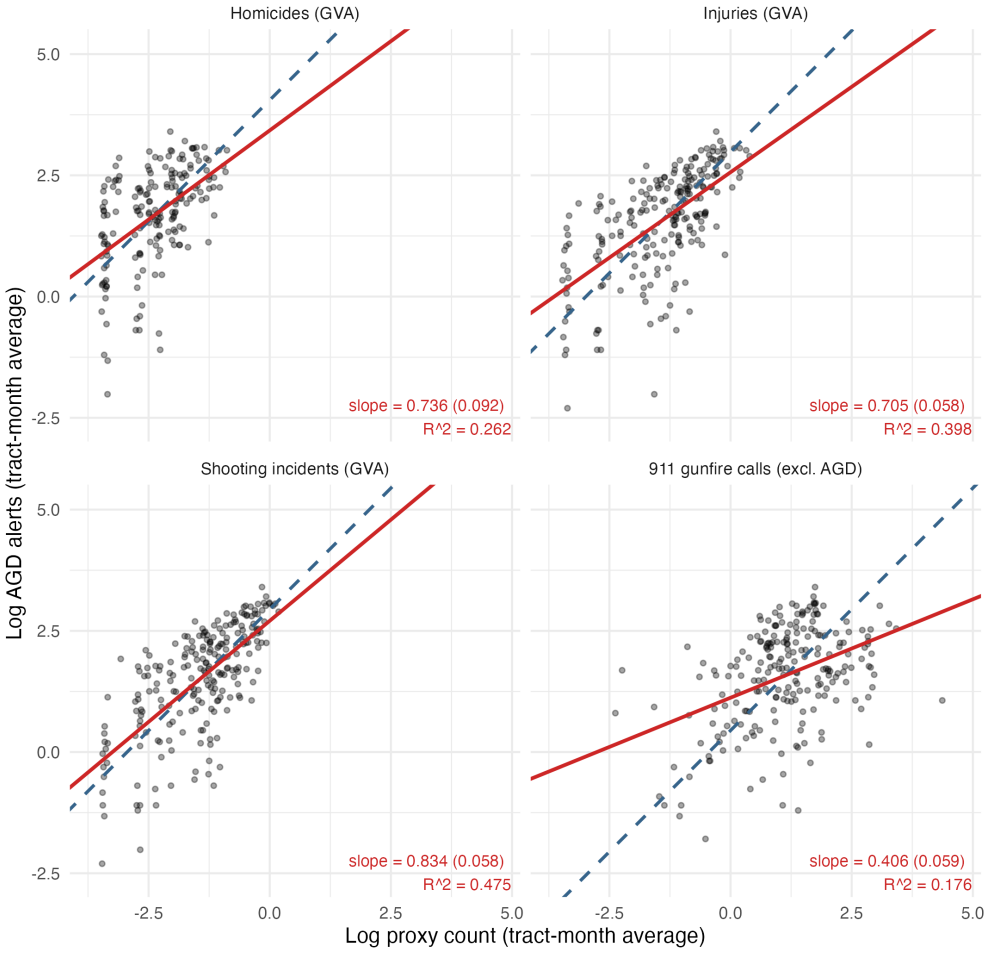
Perhaps more consequentially, we also find that these measurement errors are nonclassical. This is important because non-classical measurement error can cause unpredictable biasses which will distort our understanding of the people and places that bear the burden of gun violence. We illustrate this point by revisiting a classic question that has been examined in public health, criminological, and economic research: how is exposure to gun violence distributed across racial and socioeconomic groups (Morrison *et al.*, 2017; Papachristos *et al.*, 2015; Evans *et al.*, 2022; Kravitz-Wirtz *et al.*, 2022)? Our approach is to identify the highest gun violence neighborhoods using acoustically detected gunshots and also the common proxy variables. We will then use these indicators to examine how type-2 errors (i.e. failing to correctly identify a highest gun-violence neighborhood) skews our perception of who lives in the most violent places.

Figure 4 plots the frequency of type-2 errors for each proxy variable. More precisely, we define a neighborhood as high-crime if it falls in the top quintile of average gun-violence as measured using the number of acoustically detected gunshots. We then construct similar top-quintile indicators using the proxy variables (X-axis). The height of the bars denotes the rate of agreement between the proxy indicator and actual gun-violence.

The figure makes it clear that common proxies do a poor job at classifying the neighborhoods with the highest rates of gun violence. For example, we find that 911 calls for gunshots correctly classify the highest violence neighborhoods only 17% of the time. Injury reports from the gun violence archive do not do much

[7] For this visualization, we drop the 4-23% of observations with zero proxy counts and take the logarithm in order to create a clear visualization; however, our results are not sensitive to this decision. We find similar results if we plot the data in levels as well. See Appendix Figure A3.

Figure 3: Standard proxies are measured with substantial amounts of error

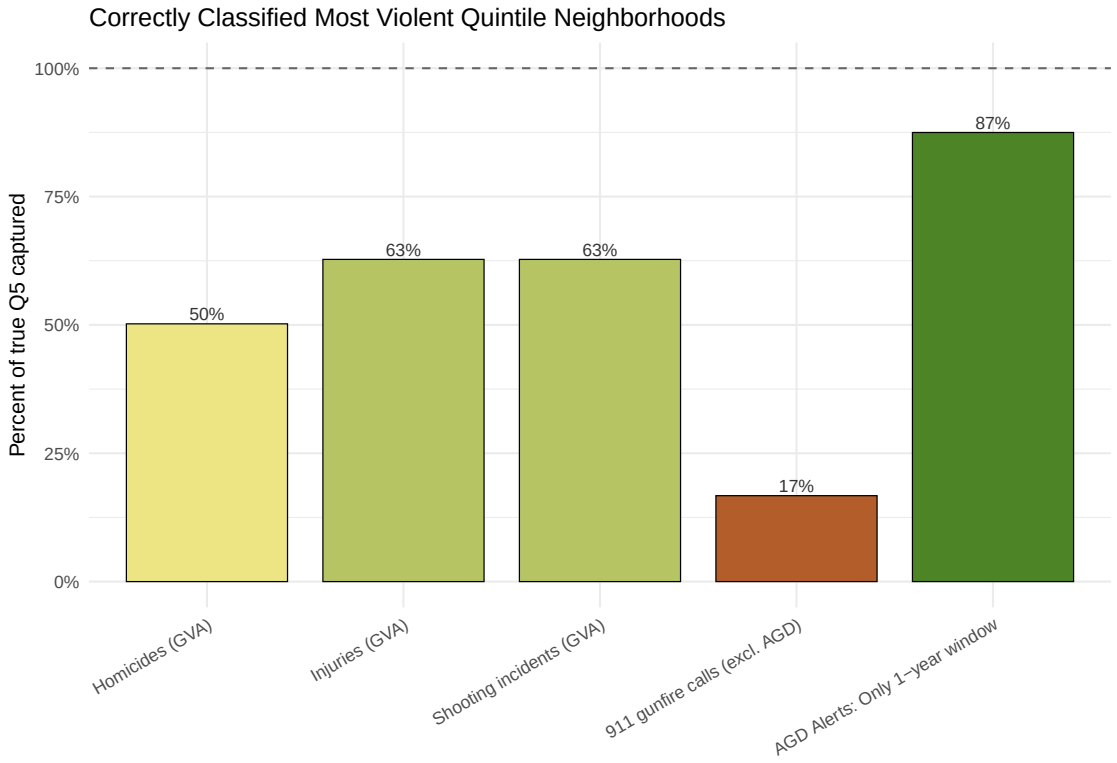


Notes: This exhibit displays plots that scatter the logarithm of acoustically detected gunshot counts (Y-axis) against the logarithm of a common proxy (X-axis). The top left panel gives the correlation between homicides and actual gunshot incidents. The top right panel gives the correlation between reported gun violence injuries and actual gunshot incidents. The bottom left panel gives the correlation between gun violence archive incidents and actual gunshot incidents. The bottom right panel gives the correlation between 911 calls and actual gunshot incidents. The dashed line denotes the 45 degree line that goes through the mean of the data. The red line denotes the line of best fit. The slope and R^2 of the best-fit line are reported in the bottom right hand corner.

better, correctly classifying high gun violence neighborhoods only 63% of the time. One concern with these results could be measurement error in the acoustic gun violence data itself. If our sample period is small enough that it fails to precisely capture underlying rates of violence, then some of the "disagreement" we document here could actually just be due to classical measurement error in our acoustic gunshot benchmark. However, that is not the case. We find that even restricting to a smaller, 1-year sample of acoustic gunshot detection data represents a substantial improvement, correctly classifying the highest violence neighborhoods 87% of the time. This suggests that small-sample measurement error in the acoustic gunshot detection data is unlikely to be the culprit.

Next, we show that these classification errors are more likely in low-income, Hispanic neighborhoods. Table

Figure 4: Correctly classified most violent quintile neighborhoods



Notes: This figure plots the frequency of type-2 errors for various gun violence proxies. Specifically, we define a neighborhood as high-crime if it falls in the top quintile of average gun-violence as measured using the number of acoustically detected gunfire incidents over our entire sample period. We then construct similar top-quintile indicators using the proxy variables (X-axis). We also construct a similar indicator restricted to 1 year of acoustically detected gunshot data–this benchmark let's us explore the role of measurement error in small samples. The height of the bars denotes the rate of agreement between the proxy and actual gun-violence with respect to classifying the highest gun-violence neighborhoods.

2 displays the mean demographic characteristics of the highest gun violence neighborhoods, as well as the mean demographic characteristics of neighborhoods classified as high gun violence by the proxy variables. Neighborhoods classified as high-violence using 911 calls for service have median incomes that are 50% higher and have Hispanic population shares that are too small by a factor of 1.5. Similar patterns emerge when we examine other proxies. Interestingly, neighborhoods identified by proxy variables actually somewhat overstate the degree to which gun violence is concentrated in African American neighborhoods, with estimates of the Black population share that range anywhere from 8-17% larger than they should be.

Taken together, the results in this section paint a clear picture. Common proxies for gun violence are insufficient for characterizing gun-violence. Acoustic gunshot data suggest that proxy variables contain consequential, and non-classical, measurement errors that can skew our understanding of which communities are most impacted. Having established this fact, we next turn to the Safegraph data to explore what we can learn descriptively about the connection between gun violence and economic activity.

Table 2: Consqeuences of Non-classical Measurement Error in Gun Violence Proxies

	Homicides (GVA)	Injuries (GVA)	Shooting incidents (GVA)	911 gunfire calls (excl. AGD)	AGD alerts
White (non-Hispanic) share	0.081	0.065	0.065	0.128	0.058
	(0.113)	(0.093)	(0.090)	(0.152)	(0.071)
Black (non-Hispanic) share	0.709	0.757	0.780	0.691	0.679
	(0.274)	(0.251)	(0.230)	(0.272)	(0.299)
Hispanic share	0.149	0.103	0.104	0.114	0.171
	(0.227)	(0.171)	(0.187)	(0.164)	(0.265)
Median household income	49,640	48,105	45,998	66,627	38,915
	(21,239)	(22,893)	(21,154)	(32,741)	(14,086)
Poverty rate	0.303	0.312	0.326	0.245	0.352
	(0.114)	(0.140)	(0.135)	(0.098)	(0.130)
Labor force participation rate	0.601	0.596	0.583	0.663	0.558
	(0.089)	(0.115)	(0.095)	(0.097)	(0.080)
Tracts	47	47	47	47	47
p-value vs. AGD alerts Q5	0.010	0.005	0.003	$<$0.001	—

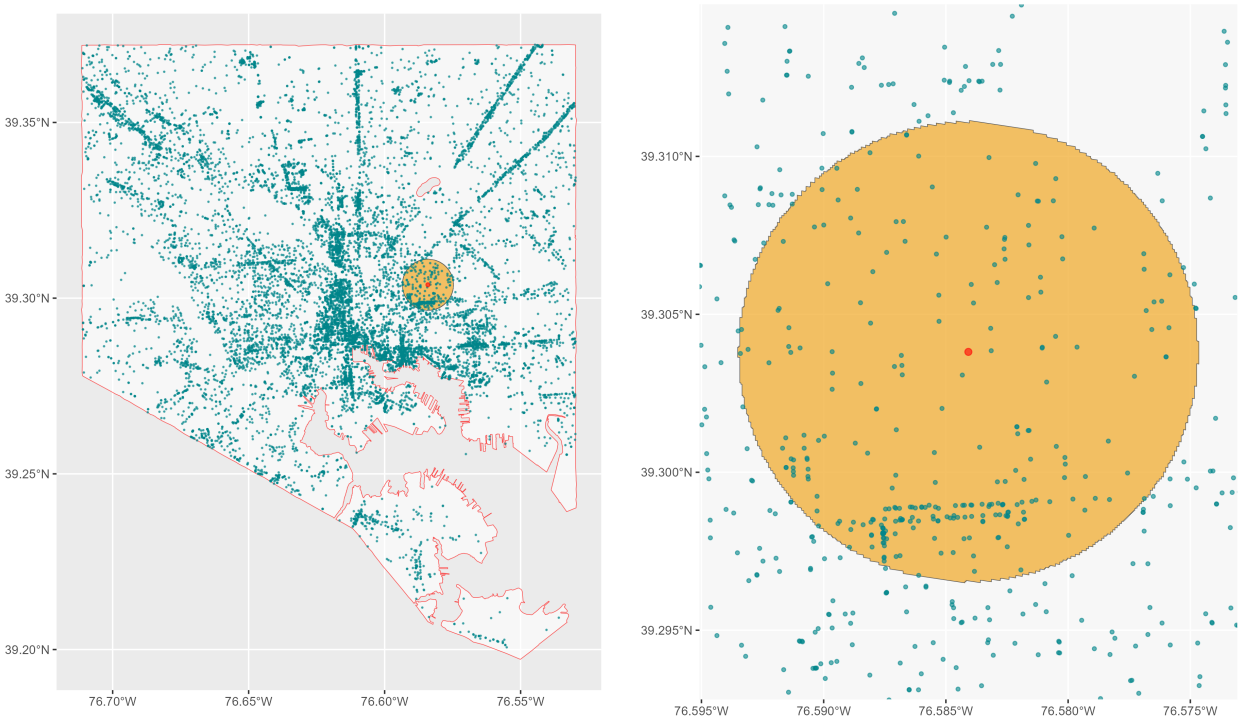
Notes. Each column holds the census tracts in the top quintile of the named measure. Sample: 239 tracts in 7 cities, 2019-01–2021-06; all gunfire measures are summed over sensor-active block-months only, so every column is selected on the same geography. Cells report means with standard deviations in parentheses. The final rows report p-values from joint Wald tests of the null that all 6 demographic means equal those of the reference column's tracts, clustered by tract. "AGD" is acoustic gunshot detection data.

3.2 A New Stylized Fact: Most Businesses are Impacted by Gun Violence

How commonly are business exposed to gun violence? To answer this question, we use two complementary approaches. The first approach involves measuring the number of business within a given geographic proximity of a gunfire incident. Figure 5 illustrates this idea graphically. Panel A denotes every business in Baltimore (blue dots) with a single gunshot incident denoted via the red-dot. Panel B displays the portion of Baltimore impacted by the gunfire incident at a much finer spatial resolution. The yellow circle denotes the set of businesses we characterize as exposed to the gunshot. Our second approach is similar, except that we begin with a business, and then ask how many gunshots fall within a ring of a given radii. In both cases, we vary the definition of exposure to include circles with radii of 0.25, 0.5, and 1 mile.

Table 3 displays the results. Columns correspond to different ways of defining exposure geographically. Panel A describes businesses near each gunfire incident, with rows corresponding to the average and median number of businesses, as well as the overall share of incidents without a single business nearby. Panel B describes the share of business exposed to gunfire, with rows corresponding to different time horizons.

Figure 5: Businesses within a 0.5 mile radius of a gunshot



Panel A: Businesses in Baltimore

Panel B: Businesses near the gunshot

Notes: These figures exhibit our strategy of measuring businesses exposed to gun violence. Panel A denotes every business in Baltimore (blue dots) with a single gunshot incident denoted via the red-dot. Panel B displays the portion of Baltimore impacted by the gunfire incident at a much finer spatial resolution. The yellow circle denotes the set of businesses we characterize as exposed to the gunshot. We vary the definition of exposure to include circles with radii of 0.25, 0.5, and 1 mile.

Table 3: Businesses and gunfire: proximity and exposure

	Same block	Within 0.25 mi	Within 0.5 mi	Within 1 mi
Panel A. Businesses near each gunfire incident				
Mean number of businesses	1.2	22.4	86.6	323.5
Median number of businesses	0	18	75	293
Share of incidents with none nearby	0.567	0.025	0.019	0.019
Panel B. Share of businesses exposed to gunfire				
In the same month	0.132	0.772	0.959	0.997
In the same year	0.534	0.981	0.998	1.000
Across our sample	0.699	0.988	0.999	1.000

Notes: This table describes the extent to which business in high-crime, urban areas are exposed to gun violence. Columns correspond to different ways of defining exposure geographically. We consider circles with radii of 0.25, 0.5, and 1 mile, as well as the block-level boundaries from the census. Panel A describes businesses near each gunfire incident, with rows corresponding to the average and median number of businesses, as well as the overall share of incidents without a single business nearby. Panel B describes the share of business exposed to gunfire, with rows corresponding to different time horizons.

Table 3 makes it clear that the businesses in our data are exposed to gun fire. For example, there are 22.4 businesses with a quarter mile of a typical gunshot incident. Zooming out, we observe that there are 86.7 businesses within a half mile and 323.5 within a full mile.

This geographic proximity results in frequent and repeated contact. Just under 55% of the businesses in our data will experience a gunshot incident on the same block—essentially on their doorstep—each year. If we zoom out geographically, we find that 96% of all businesses will be exposed to at least 1 gunfire incident within a half mile of it's doorstep each month, and virtually 100% of businesses will experience at least one gun fire incident each year.

Taken together, the facts established in table 3 make it clear that most business in lower-SES, urban neighborhoods are impacted by gun violence. Yet, the economic costs of this exposure are unclear. On the one hand, it could be that these repeated gun fire events drive down consumer demand, resulting in less foot-traffic and lower levels of revenue. On the other hand, gun violence is not random. If impoverished urban areas create the conditions where gun violence can flourish, it could be the case that the businesses who chose to locate their would have always had lower revenue, even in the absence of gun-fire. This motivates our research designs, which we describe in detail in the next two sections.

4 The Short-run Impact of Gun Violence

Motivated by the extent to which businesses are exposed to gun violence, we next turn to measuring it's casual effect. In this section, we specifically ask whether a gun-fire episode results in short-run declines in foot traffic or expected revenue for business owners. To do so, we exploit the fine grained resolution our data affords on the timing of gunshots in a regression discontinuity design. We find that gunshots have little impact on foot traffic or expected revenue in the short-run.

4.1 Research Design

To measure the impact of a gun fire incident on business activity, we exploit the timing of gunshots in a regression discontinuity design. Intuitively, our design asks whether a neighborhood sees a sudden change in foot traffic or expected revenue in the immediate aftermath of a gunfire episode. This comparison will yield causal estimates if the conditional expectation function mapping the running variable (time) into the outcome (e.g. store visits) would have otherwise been smooth across the date of the gunfire incident (Lee & Lemieux, 2010).

More formally, we estimate the following model on our short-run sample:

$$Y_{nt} = \beta \mathbb{1}(t - t_n^* > 0) + F(t - t_n^*) + \delta_{d(t)} + \epsilon_{nt}, \tag{1}$$

where Y_{nt} is an outcome (e.g. store visits) for neighborhood n observed at time t; t_n^* is the date neighborhood

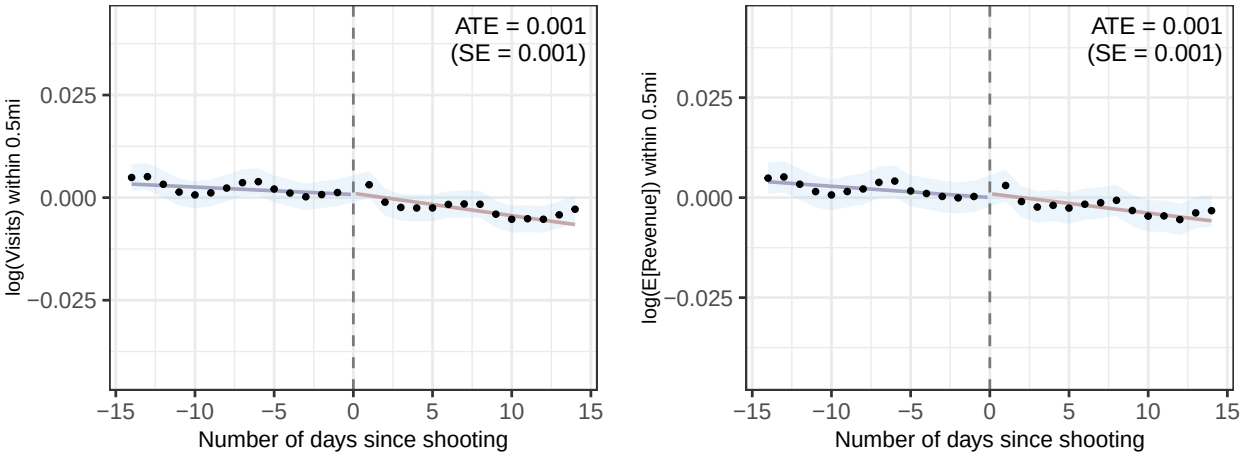
n experienced a gunfire incident; $d(t) \in \{\text{Monday, Wednesday, Tuesday} \dots\}$ is an index that maps dates into days of of the week; and $\delta_{d(t)}$ is a day-of-week fixed effect that ensures our results are not driven by an increased likelihood of gunshots occurring on Fridays. We use a triangular kernel weighted by the number of alerts in the block. Our baseline model specifies $F()$ as a linear polynomial that is piecewise around 0; however, our results are not sensitive to this decision. Our preferred model also uses a bandwidth of two weeks around the date of the incident (i.e. 14 days before and 14 days after). We prefer to use a fixed bandwidth in our baseline model, rather than rely on the data driven bandwidth, to ensure our results are estimated on a consistent sample across different outcomes. However, our results are not sensitive to the particular bandwidth used or the particular order of the polynomial. We find similar results using higher order polynomials, alternative bandwidths, and the data driven bandwidth of Imbens & Kalyanaraman (2012). See Appendix table A5 for these results using these alternative specifications. For inference, we adjust our standard errors for spatial correlation within 2 miles of the gunshot event using the method of Conley (1999).

Typically, there are two types of evidence researchers offer in support of the identifying assumptions in an RD design. The first is evidence of balance. If the RD assumptions are met, baseline or otherwise exogenous characteristics should not exhibit a discontinuity through the cutoff. In our case, where the running variable is time, any difference in baseline characteristics will be mechanically zero across the cutoff since, by definition, baseline characteristics do not vary over time. Moreover, other variables that would be natural to check for balance in this setting (e.g. demographics) are not measured at a fine enough temporal frequency by the census and hence will also be mechanically zero across the cutoff. The second is evidence that the running variable is continuous through the cutoff. However, that is also mechanically true in our case, since we have an identical number of observations at each date relative to the cutoff. Hence, we also do not report density tests.

4.2 Findings

We find little evidence that gun shot incidents impact foot traffic or expected revenue. Figure 6 displays the lack of discontinuity visually. In Panel (a) the Y-axis variable is log visits. In panel (b) the Y-axis variable is log expected revenue. In both cases, we residualize on day of week fixed effects prior to plotting. Points represent daily means. The blue shaded area represents a 95% confidence interval around those means. In accordance with our baseline specification, we also plot a piecewise linear polynomial around the cutoff. In both panels, the Y-axis is scaled to one quarter of a within-block standard deviation of the outcome in treated blocks in the pre-period (0.17) on either side of 0.

Figure 6: The impact of shootings and victimizations on visits to businesses



(a) Log visits

(b) Log expected revenue

Notes: Figure 6 displays the lack of discontinuity visually. In Panel (a) the Y-axis variable is log visits. In panel (b) the Y-axis variable is log expected revenue. In both cases, we residualize on day of week fixed effects prior to plotting. Points represent daily means. The blue shaded area represents a 95% confidence interval around those means. In accordance with our baseline specification, we also plot a piecewise linear polynomial around the cutoff. In both panels, the Y-axis is scaled to a quarter of a within-block standard deviation of the outcome in treated blocks in the pre-period (0.17) on either side of 0. For inferences, we report Conley (1999) standard errors adjusted for spatial correlation within a 2 mile radius of the gunshot event.

Table 4 summarizes the key findings for log visits and expected revenue. Panel A gives estimates for the full sample, summarizing the variation on display in figure 6. Panel B gives estimates for sub-samples defined by neighborhood characteristics. Specifically, for each variable considered, we split the RD sample according to whether it is above or below the median and estimate our preferred model separately on each sub-sample. We also report the p-value from a formal test of the null hypothesis that the two sub-sample estimates are equal. Panel C gives estimates that split the sample by the intensity of the gunshot incident. Specifically, we restrict the data to incidents that have at least 2 gunshots, at least 5 gunshots, and at least 10 gunshots.

For the full sample, we find little evidence of meaningful changes in foot traffic or expected revenue in the aftermath of a gunshot event. The full sample estimates suggest changes in foot traffic of 0.001 log points, with a 95% confidence interval that can rule out declines of larger than 0.1%. Scaling by the sample mean which, at the daily level, is $144 we observe that this estimate rules out declines in daily revenue that are larger than 15 cents.

We also find little evidence that the treatment effects vary meaningfully with the demographic composition of the block. Turning to panel B, we observe that the sub-group estimates are statistically indistinguishable from zero, and also statistically indistinguishable from one another, for the vast majority of sub-groups. In fact, of the 24 outcome-by-sub-group combinations explored, 4 of them are significant at conventional levels which, at the 5% level, is roughly 2.8 more than would be expected by chance alone. However, in all cases, the magnitudes remain economically insignificant. Taking the minimum over the collection of lower bounds implied by the 95% confidence intervals, we find that our estimates rule out declines of larger than 0.0024

Table 4: The short-run impact of gunshots: Average and heterogeneous effects

	Log Visits			Log E[Revenue]		
	Above cut	Below cut	Diff. p-value	Above cut	Below cut	Diff. p-value
Panel A: Full-sample estimates						
Full sample	0.0006			0.0014*		
	(0.0005)			(0.0006)		
Panel B: By neighborhood characteristics						
Block-group median income	-0.0005	0.0013	0.915	0.0009	0.0020**	0.941
	(0.0009)	(0.0008)		(0.0008)	(0.0008)	
Block-group poverty rate	0.0010	0.0003	0.840	0.0018**	0.0009	0.956
	(0.0006)	(0.0008)		(0.0007)	(0.0008)	
Block-group white share	-0.0004	0.0015	0.260	0.0007	0.0019**	0.248
	(0.0009)	(0.0008)		(0.0009)	(0.0007)	
Block-group Black share	0.0002	0.0006	0.111	0.0009	0.0014	0.126
	(0.0008)	(0.0008)		(0.0007)	(0.0009)	
Block-group Hispanic share	0.0005	-0.0000	0.033*	0.0014*	0.0004	0.015*
	(0.0007)	(0.0009)		(0.0007)	(0.0008)	
Baseline ShotSpotter alerts	0.0008	0.0007	0.607	0.0021**	0.0006	0.585
	(0.0007)	(0.0008)		(0.0007)	(0.0007)	
Panel C: By incident intensity						
Gunshots ≥ 2	-0.0001	0.0012	0.444	0.0008	0.0016*	0.846
	(0.0009)	(0.0007)		(0.0007)	(0.0006)	
Gunshots ≥ 5	-0.0003	0.0008	0.581	0.0006	0.0014*	0.504
	(0.0011)	(0.0006)		(0.0008)	(0.0006)	
Gunshots ≥ 10	-0.0013	0.0008	0.379	-0.0008	0.0016**	0.524
	(0.0021)	(0.0005)		(0.0015)	(0.0005)	

Notes: This table summarizes key findings from our preferred short-run RD model on log visits and expected revenue. Panel A gives estimates for the full sample, summarizing the variation on display in figure 6. Panel B gives estimates for sub-samples defined by neighborhood characteristics. Specifically, for each variable considered, we split the RD sample according to whether it is above or below the median of the indicated variable and estimate our preferred model separately on each sub-sample. We also report the p-value from a formal test of the null hypothesis that the two sub-sample estimates are equal. Panel C is similar to panel B, except that we split the sample by the intensity of the gunshot incident. Specifically, we restrict the data to incidents that have at least 2 gunshots, at least 5 gunshots, and at least 10 gunshots respectively. For inferences, we report Conley (1999) standard errors adjusted for spatial correlation within a 2 mile radius of the gunshot event. In all cases, corresponding regression discontinuity plots are contained in Appendix Figures A12 and A13.

log points for visits and 0.0038 log points for expected revenue. Scaling the revenue lower bound by the sample means suggests that we can rule out declines in daily revenue that are larger than 55 cents.

We also fail to find evidence that the treatment effects vary meaningfully with the intensity of the gunshot incident. Turning to panel C of table 4, we observe that 3 of the 12 coefficients are significant at conventional levels, which is 2.5 more than would be expected by chance alone, and none are significantly different from one another. More importantly, none of the coefficients are economically meaningful. The minimum lower bound across all 12 95% confidence intervals represents a decline of 0.0055 log points. Scaling the revenue lower bound by the sample means suggests that we can rule out declines in daily revenue that are larger than 79 cents.

Robustness. These results are robust to numerous alternative specifications and sensitivity tests. Appendix table A5 presents specifications with alternative bandwidths and polynomials. The results are effectively unchanged. Appendix table A5 gives results with alternative geographic definitions of exposure. The results are robust to defining exposure using radii as small as 0.1 mi. Appendix table A5 also presents all of our key results in levels, inclusive of zeros. Thus, the results are not an artifact of the log transformation used in our baseline specification. Taken together, this comprehensive set of robustness checks makes clear that the findings are a feature of the data and not a consequence of the particular modeling choices that inform the baseline specification.

Discussion. Does the fact that economic activity fails to shift in response to gun violence in the short run imply that there is no causal relationship? Not necessarily. As documented in table 1, neighborhoods covered by acoustic gunshot detection technology tend to have high rates of violence relative to an average city neighborhood. And as documented in section 3.2, prevalence of gunshot exposure in these neighborhoods is frequent and widespread. If the residents of these blocks are numb from repeated exposure, it could be the case that the impact of the marginal shot is effectively zero. Thus, it is an open question whether large, persistent escalations of gun violence cause negative impacts on economic activity. We turn to this point in the next section.

5 The Long-run Impact of Gun Violence

In this section, we explore the impact of long-run, sustained increases in gun violence. To do so, we first identify neighborhoods that experience a sudden, large and persistent increase in gunshots during our sample period. We then compare trends in these neighborhoods to trends in areas that had relatively stable rates of gun violence over the same period. We find little evidence that sudden persistent increases in gun violence cause declines in economic activity. This result is robust to numerous sensitivity checks.

5.1 Research Design

To implement our research design, we first need to establish criteria that identifies neighborhoods that experience a rapid escalation of violence. As discussed in section 2, we define treatment using two criteria. The first criteria is that the block experiences a greater than 1 standard deviation increase in gun violence

incidents within a half-mile of its centroid over the course of a single month. This criteria is meant to capture the idea that the rise in gun violence is sudden and large. The second criteria is that average gun violence within the half mile ring is 1 standard deviation larger for the 12 months after the sudden rise than it was during the 12 months before the sudden rise. This criteria is meant to capture the idea that the change is persistent. To build a comparison group for a given event, we define comparison blocks to be those with minimal change over the same period, which we implement by selecting blocks whose standard deviation of gun violence exposure within a half mile ring is less than a quarter of the sample-wide standard deviation during every 24 month window in the sample. However, as discussed later in this section, our results are not particularly sensitive to these decisions (see Table 5).

Equipped with this analysis sample, we implement our identification strategy by employing a stacked event study design. Specifically, we create one stack for each treatment start date. Each stack contains the treated blocks that began their sustained increase in gun violence in that month and the entire set of control blocks, covering the time period of 12 months before and 12 months after the treatment onset month. Our baseline specification follows Wing *et al.* (2024) and reweights the control blocks so that the overall ATT estimate is an average of the stack-specific ATT weighted by the number of treated blocks in each stack. We also restrict the analysis only to blocks that have at least one visit every month of the pre- and post-period, so that we can obtain meaningful information on the number of business visits and revenue. In Table 5, we show that our results are robust to estimation in levels inclusive of zeros.

Our identifying assumption is that the trend in economic activity for neighborhoods without a sustained increase in gunfire is an accurate counterfactual for the trend that treated neighborhoods would have otherwise experienced. Our preferred specification implements this identification strategy with the following event study model:

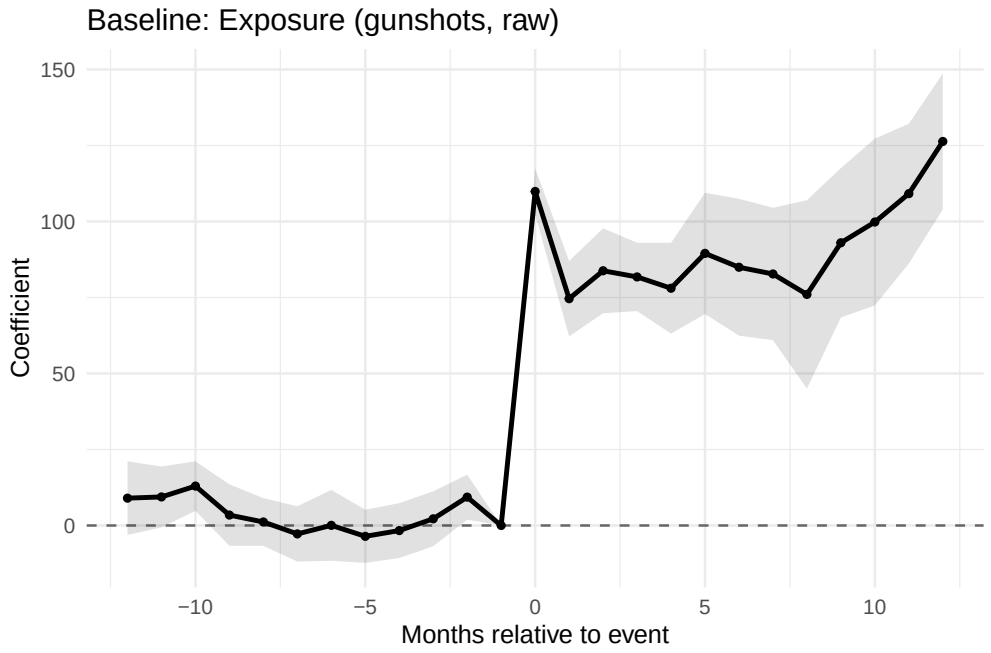
$$Y_{nst} = \alpha_{sn} + \delta_{st} + \sum_{\tau \in \mathbb{T}_s} \beta_\tau D_{ns\tau} + \epsilon_{nst} \tag{2}$$

Where Y_{nst} is an outcome for neighborhood n as observed in stack s at time t; α_{sn} is a stack-by-neighborhood fixed effect; δ_{st} is a time-by-stack fixed effect; $\mathbb{T}_s = \{\tau : \tau = t - t_s^*, \forall \tau \neq -1\}$ is the set of event times relative to the treatment date (t_s^*) in stack s and where the period before treatment ($\tau = -1$) is omitted; $D_{ns\tau} = \mathbb{1}(t - t_s^* = \tau) \times Treated_n$ is an indicator that takes a value of 1 for the treated unit in stack s at relative treatment time τ; and ϵ_{nst} is a residual. The parameters of interest are the β_τ, each of which is an equally weighted average of the stack specific difference-in-difference estimates at event time τ relative to the omitted period. For inference, we report Conley (1999) adjusted standard errors which account for spatial dependence within a 2-mile radius and, since the geographic coordinates of a neighborhood is invariant across stack, also effectively account for the fact that control observations can be reused across stacks.

In total, across the cities, we have 4,410 sample blocks.[8] Of these, we observe that 1,911 experience a sustained increase in gunfire between 2018 and 2022, forming our treatment blocks, while 2,499 experience minimal changes, forming our control blocks. Blocks that experience some change, but not a sudden, sustained change—such as blocks on slow upward or downward trajectories in violence—are excluded from the empirical design.

Figure 7 highlights the sustained rise in gun-violence isolated by our research design. The X-axis denotes event time. The Y-axis gives event study coefficients from our preferred model where the outcome variable is a 12 month moving average of gun violence incidents. Each point denotes one estimate from the event study model. The grey band denotes confidence intervals.

Figure 7: First-stage on Exposure



Notes: This figure highlights the sustained rise in gun-violence isolated by our research design. The X-axis denotes event time. The Y-axis gives event study coefficients from our preferred model where the outcome variable is gunshots within 0.5 miles. Each point denotes denotes one estimate from the event study model. The grey band denotes confidence intervals. For inference, we report Conley (1999) adjusted standard errors which account for spatial dependence within a 2-mile radius and, since the geographic coordinates of a neighborhood is invariant across stack, also effectively account for the fact that control observations can be reused across stacks.

Figure 7 reveals that our design isolates neighborhoods experiencing an increase of roughly 88 gunshots per month over a 12 months period. In the treatment group, the pre-period mean over this period is 79 and hence this represents a roughly 112% increase in monthly gun-fire. Thus, the treatment we explore in this section is not marginal, and we expect that if gun-violence is linked to economic activity, it should be moved by this extreme event.

[8] A block constitutes part of the sample if at least 80% of the blocks within a half mile fall within the acoustic gunshot detection coverage area.

Notably, the rise in gunshots appears to cause an increase gun violence injuries and homicides. Appendix Figure A17 displays the results from our preferred event study model with the gun violence archive outcomes as the left hand side variable. Visually, we observe flat pre-trends that suddenly spike with the onset of the event, with point estimates that suggest a sustained rise in violence is associated with roughly 1.5 additional gun violence injuries per month over the ensuing period. Thus, our definition of treatment appears to adequately capture a set of neighborhoods that experienced a sustained rise in gun violence over our sample frame.

5.2 Findings

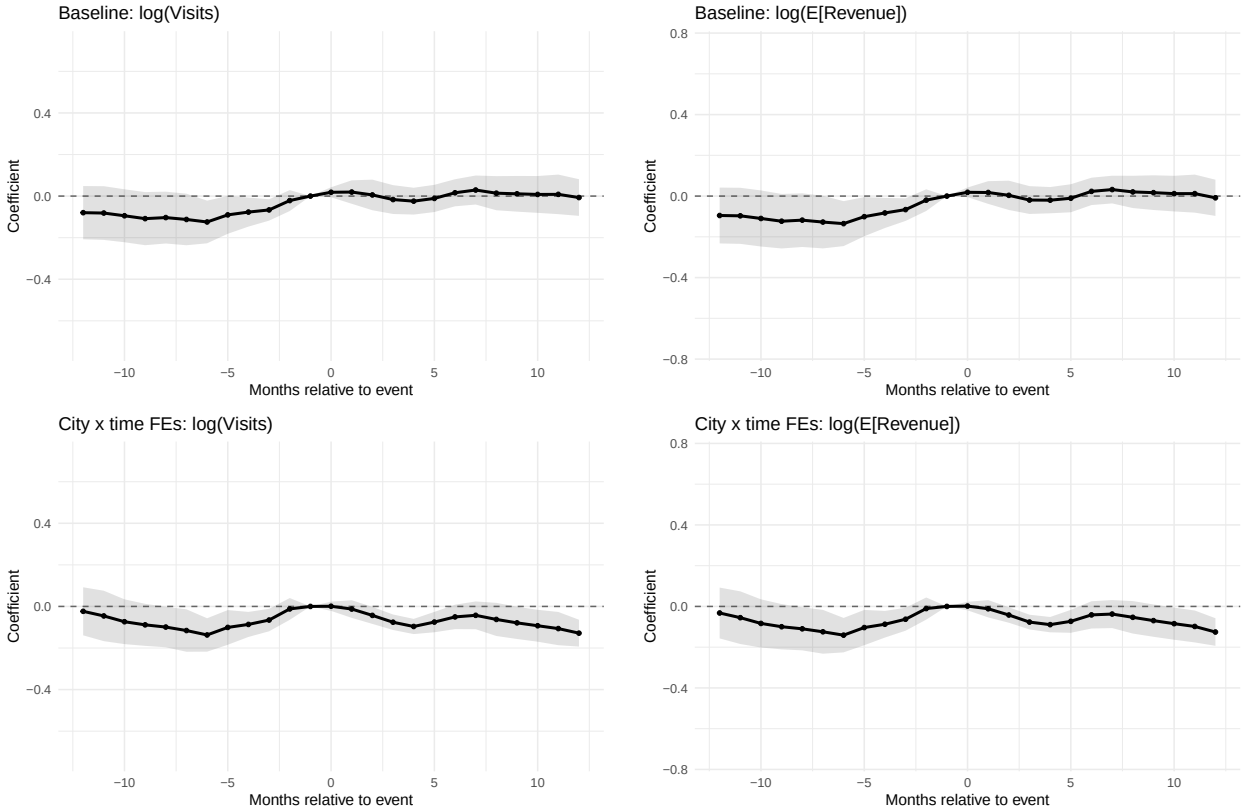
We find little evidence that gun violence reduces economic activity. Figure 8 displays the results. Within each panel, the X-axis denotes event time. Dots denote event study coefficients from model 2. Grey bands denote 95% confidence intervals constructed using Conley (1999) standard errors that allow for spatial dependence within a 2 mile radius. The Y-axis is scaled to 2 within-block standard deviation of the outcome in treated blocks in the pre-period. The top left panel gives results for store visits. The top right panel gives results for expected revenue. The bottom left and bottom right panels are similar, except that we interact the time fixed effects with city to ensure the counterfactual trend is built entirely within city.

Figure 8 reveals relatively flat pre-trends that remain near zero after the rise in gun violence. Focusing on the top-left panel, we observe that, if anything, the figure suggests a slight *increase* in foot traffic relative to the pre-period mean, a pattern that is also reflected in expected revenue. The point estimates for visits suggest an 8.5p.p. rise in foot traffic accompanied by a 9.7p.p. rise in expected revenue, with confidence intervals that are sufficiently precise to rule out declines of more than 2.2p.p. for foot traffic and 1.9p.p. for expected revenue. However, turning to the bottom two panels, we observe that this apparent increase in economic activity largely disappears once we restrict to within-city comparisons. Here, the point estimates for visits suggests a 0.4p.p. increase, and the point estimate for expected revenue suggests 1.4p.p. rise. However, in both cases we acknowledge that the lower bound of the confidence intervals for the within city comparisons cannot rule out larger declines.

Table 5 provides evidence that our results are robust to numerous sensitivity and robustness checks.[9] To begin, we ask whether our results could be an artifact of Covid-19 induced changes in foot traffic and violence. To address this, we present results for models that restrict to observations from prior to January 1, 2020. Next, we show robustness to varying our half-mile definition of exposure to gun-violence. Specifically, we redefine treatment to count a block as exposed to gunshots if they occur within a quarter mile, within a mile, or within a half-mile but using a gaussian exposure mapping. Next, we ask whether our results are sensitive to changing the definition of treatment. Specifically, we relax our criteria that the neighborhoods experience a 1-SD increase in violence to include less violent increase (0.5-SD), and we also restrict it to be more demanding and isolate more violent increases (2-SD). Next, we explore sensitivity to our defintion of the control group by varying are requirement that gun violence be stable. Specifically, we explore controls groups built by requiring the standard deviation of gun violence over the comparable period

[9]Corresponding event study figures are contained in Appendix Figures A14 and A15

Figure 8: Impact of sustained gun violence on business visits



Notes: This figure gives results from model 2. Within each panel, the X-axis denotes event time. Dots denote event study coefficients. Grey bands denote 95% confidence intervals constructed using Conley (1999) standard errors that allow for spatial dependence within a 2 mile radius. The Y-axis is scaled to 2 within-block standard deviation of the outcome in treated blocks in the pre-period. The top left panel gives results for store visits. The top right panel gives results for expected revenue. The bottom left and bottom right panels are similar, except that we interact the time fixed effects with city to ensure the counterfactual trend is built entirely within city.

Table 5: Robustness across specifications

	log(Visits)	log(E[Revenue])	log(Revenue)
Panel A: Log outcomes			
Baseline	0.085	0.097	0.059
	(0.055)	(0.059)	(0.090)
Events on/before 2020-01-01	0.105	0.102	0.160*
	(0.087)	(0.088)	(0.067)
City x time FEs	0.004	0.014	0.143**
	(0.046)	(0.052)	(0.055)
Exposure within 0.25mi	0.095	0.113	0.086
	(0.053)	(0.059)	(0.068)
Exposure within 1mi	0.065	0.080	-0.030
	(0.048)	(0.052)	(0.093)
Gaussian exposure (0.5mi)	0.101	0.115*	0.101
	(0.053)	(0.058)	(0.080)
Event threshold 0.5 SD	0.092	0.105	0.141*
	(0.052)	(0.058)	(0.070)
Event threshold 2 SD	0.043	0.057	-0.019
	(0.107)	(0.107)	(0.125)
Control tolerance 0.1 SD	0.061	0.074	0.085
	(0.070)	(0.068)	(0.074)
Control tolerance 0.5 SD	0.040	0.045	0.045
	(0.048)	(0.049)	(0.088)
Control tolerance 1 SD	0.029	0.032	0.006
	(0.043)	(0.042)	(0.075)
Panel B: Levels outcomes, zero-visit blocks retained			
	Open stores	Visits	Visits (wins. p99)
Baseline	0.065*	92.426*	88.590**
	(0.028)	(46.495)	(32.243)

to range from 0.1-SD, 0.5-SD, and 1-SD. Finally, we explore sensitivity to estimation in levels, which also allows us to include the blocks with zero open stores that are otherwise dropped when we explore outcomes in logs.

While the series of robustness checks here produce a range results, the point estimates continue to provide little evidence that economic activity declines. More precisely, we note that across all 36 of the specifications reported in Table 5, exactly two are negative. Thus the point estimate are largely consistent with the idea that a sustained rise in gun violence does not negatively impact economic activity.

However, we acknowledge that the picture is less cut and dry once we incorporate sampling uncertainty. Even for our preferred baseline specification, the lower bound of the 95% confidence interval is large enough to include economically meaningful declines. This raises an important questions regarding the interpretation of the results: What should economists and other researchers conclude from this evidence? Thus, in the next section, we ask if the evidence is precise enough to justify a significant updating of prior beliefs and a significant reduction in overall uncertainty over the effect size? Or is the evidence just too noisy to permit stronger conclusions?

6 How Precise are the Null Results?

Our results suggest that gun violence does not change, in the short- or long-run, business activity in the neighborhoods where they occur. However, it is not clear *a priori* whether the null results we observe are estimated with enough precision to meaningfully alter the prior beliefs of academics, policy makers, and the general public. This is an important distinction, as policymakers—and experts advising policymakers—often have to rely on their best understanding of causal relationships when evidence does not yet exist or when an area is fast-evolving. To address this limitation, we solicited the prior beliefs of experts in the field of applied micro-economics, a group of individuals who should arguably have the most informed opinions on the impact of gun violence on economic activity. Under the assumption that economists should process evidence rationally, and therefore update their beliefs in light of new evidence, we can use our preferred estimates to ask whether our findings are precise enough to significantly reduce the uncertainty in these experts beliefs.

6.1 Key-informant Elicitation

To obtain information on the opinion of experts in the field, we elicited their prior beliefs through a questionnaire. Our questionnaire contained two substantive questions. The first concerned the short-run impact of gun violence. The second concerned the long-run impact of gun violence. Specifically, our question regarding the short-run impact of gun violence read as follows:

> *Suppose an urban neighborhood in the US with relatively high rates of poverty and crime experiences a gunfire incident. The event consists of four gunshots, all fired over a short timespan.*
>
> *What would you expect the* ***average causal effect*** *of this incident to be on foot traffic to local*

businesses within a half-mile radius (4–8 city blocks) in the short term (1–2 days)?

Please answer as a ***signed(+/-) percent change*** *relative to the baseline mean. The baseline mean is 555 visits over the two weeks prior to the incident.*

Low % (10th percentile)
[response box]
Best Guess %
[response box]
High % (90th percentile)
[response box]

"Best guess" is your expected average effect. "Low" and "High" are values such that you would put about a 10% chance of a causal effect below Low and about a 10% chance above High.

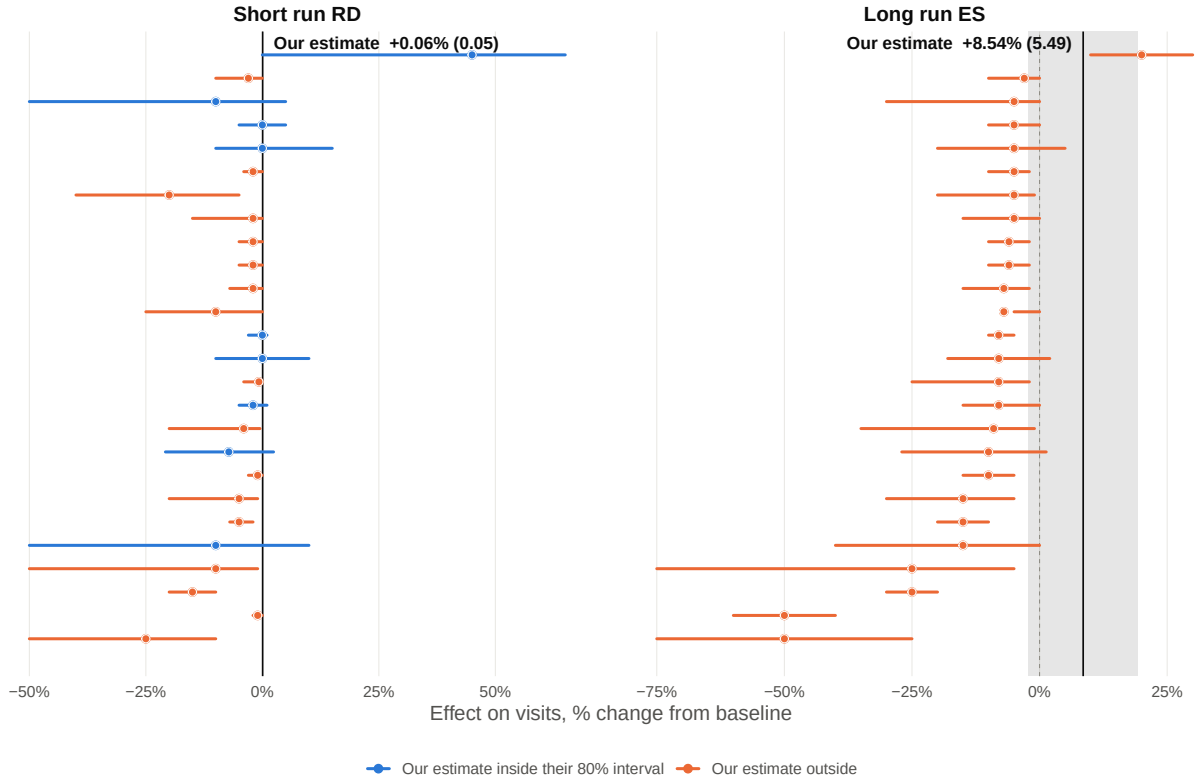
The question regarding the long-run impact of gun-violence was nearly identical, but altered the relevant summary stats and description to match the long-run setting. See Appendix B for complete questionnaire.

We elicited these expert opinions during July through September 2026 through Qualtrics, with respondents also permitted to reply with their responses via email. Potential participants were selected (and subsequently confirmed to be) US-based PhD-holding applied microeconomists, with the majority of respondents specializing in the area of crime economics. Potential participants were approached in two ways, first at a leading economics conference (the National Bureau of Economic Research's Summer Institute, including the Economics of Crime Meeting) and subsequently via email, with email recipients chosen based on the co-authors' network.

6.2 Results

Figure 9 displays each informants expected average effect as a point along with an interval that spans the each informants 10th and 90th decile estimates. The panel on the left displays responses to the question about short-run effects, corresponding to our regression discontinuity design, and the panel on the right displays responses to the question about long-run effects, corresponding to our event study design.

Figure 9: Expert Priors



Notes: This figure displays the results from our expert elicitation. The panel on the left displays results from our question about our short-run regression discontinuity design, and the panel on the right displays results from the long-run event study design. Each row corresponds to an expert ordered by their best guess for the long-run design, with the horizontal bar representing their 80% confidence interval. Our estimates are shown in the vertical black bar with the shaded area corresponding to our 95% confidence interval. The survey instrument is available in Appendix B

On average, the experts predicted that a short-run increase in gun violence would lead to an -3.6% change in business visits, with 81% them having a negative point estimate. While they almost all agreed in sign, there was significant variation in the spread of the distribution. The most certain experts had a 90th-10th percentile gap of 2 percentage points. The most uncertain experts had a 90-10 gap of 65 percentage points. In total only, 9 out of 26 experts reported a distribution of prior beliefs that contained our short-run point estimates.

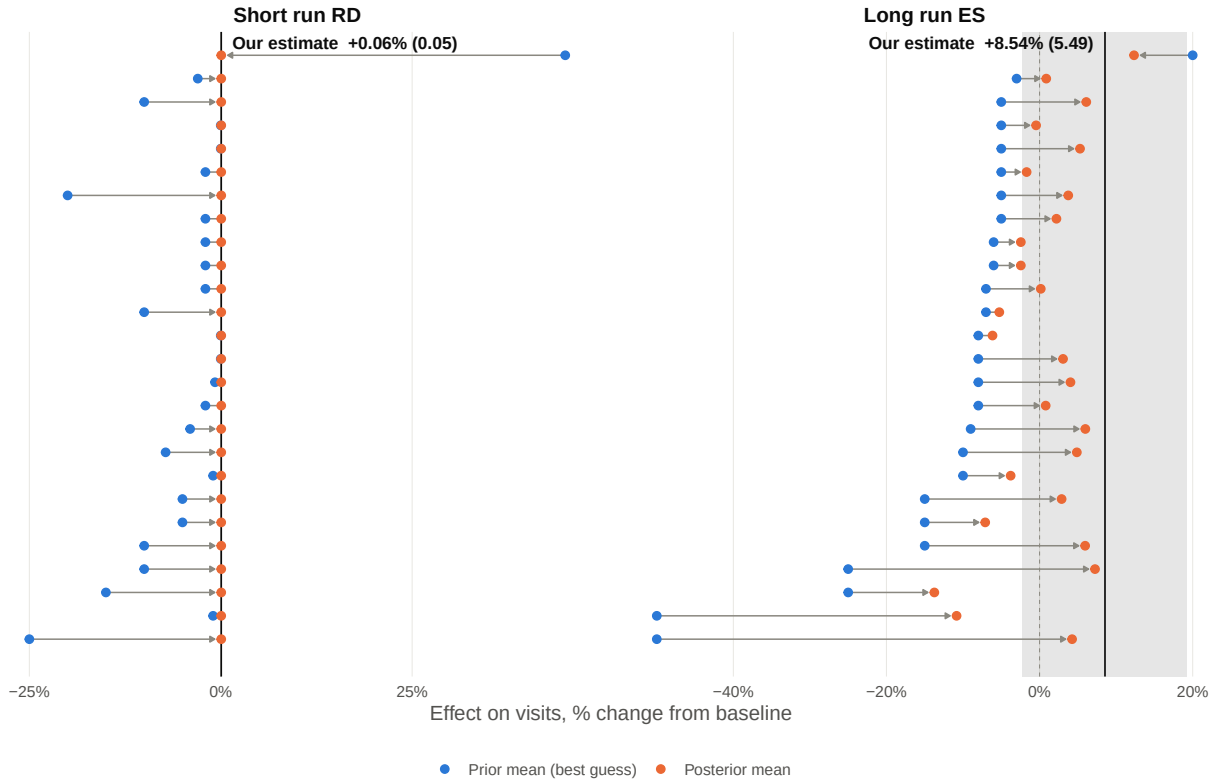
The experts in our elicitation were even more uncertain about the long-run impact of gun violence. At the mean, all but one expert reported a negative estimate, with average expected effect across all of our informants coming in at -11.3 percentage points. More importantly, the experts had a much larger spread of prior beliefs than they did for the short-run effect. For the long-run effect, the 90-10 differential ranges from 5 to 70 percentage points. For our long run study *not a single expert* included our estimated effect in their 90-10 interval.

Next, we analyze how the estimates from our study should shape beliefs over the impact of gun violence on

economic activity. As is well know from basic economic theory, rational actors should incorporate new information by updating their prior beliefs according to Bayes rule (Savage, 1954). Thus, if the prior distribution is known, then under the normative assumption that Economists should interpret new evidence rationally, we can use Bayes rule to calculate each economists implied posterior distribution.[10] If the variance of the posterior distribution is substantially smaller than the prior distribution, that implies that the evidence in our data is informative enough to substantially reduce the uncertainty of experts in the field.

Figure 10 shows the prior and posterior mean for each expert. Because our short-run estimates are so precise, and the prior distribution of beliefs over the short-run effect are more certain, the posterior mean of virtually every expert collapses to our estimate. For the long-run study, the posterior means all move substantially in the direction of the point estimate from our study. This is despite the fact that the estimates of the long-run effect are less precise and that the prior beliefs are generally more diffuse.

Figure 10: Expert Updating



Notes: This figure displays the results from our Bayesian updating exercise. The panel on the left displays results from our question about our short-run regression discontinuity design, and the panel on the right displays results from the long-run event study design. Each row corresponds to an expert ordered by their best guess for the long-run design. Our estimates are shown in the vertical black bar with the shaded area corresponding to our 95% confidence interval. The survey instrument is available in Appendix B

[10]To identify the prior beliefs of the informants who answered our questionnaire, we assume their beliefs are normally distributed and use the three distributional moments (i.e. the reported mean, 10th decile, and 90th decile of beliefs) to calibrate the distribution. In ongoing work, we are exploring sensitivity to departures from normality.

In Table 6, we report three summary statistics from this exercise: the reduction in variance from each expert's prior to their posterior, which we take as a natural way to measure the power of our data to shape beliefs; the "surprise" of our findings, which is the distance between our estimate and the expert's prior mean in prior standard deviation units; and the overlap in the prior and posterior probability distributions for each expert, which is another natural way to highlight the power of our estimates to move beliefs. Due to the precision of our short-run estimates, the median expert experienced a 99% reduction in variance from their prior to posterior, and less than 2% overlap in the two distributions—this is despite the fact that these results are less surprising, with the median experts prior "best-guess" being only 0.77 standard deviations away form our estimated effect. On the other hand, our long-run estimates were even more surprising to experts, with the median expert's best guess being almost 3 standard deviations from our realized point estimate. The long-run effect also results in a substantial reduction in uncertainty, with the median expert experiencing a nearly 60% reduction in the variance of their beliefs. Taken together, the results in table 6 indicate that our estimates are precise enough to substantially shape beliefs on the connection between gun violence and economic activity.

Table 6: Expert priors updated on the study estimates

	Short run	Long run
Reduction in variance (median)	99.99%	58.88%
Surprise in SD (median)	0.77	2.89
Prior/posterior overlap (median)	0.018	0.392
N	26	26

Notes: Each expert's elicited 10th percentile, best guess and 90th percentile are treated as a normal prior; the study estimate and its standard error are the likelihood. Reduction in variance is the median across experts of $1 - \sigma^2_{\text{post}}/\sigma^2_{\text{prior}}$. Surprise is the median distance between the study estimate and the expert's prior mean, in prior standard deviations. Prior/posterior overlap is the median overlapping coefficient, the area under $\min(\text{prior}, \text{posterior})$, where 1 is an unmoved belief and 0 a fully displaced one.

7 Conclusion

In this study, we examine the relationship between gun violence and business activity using data unprecedented in its scale and detail. We show a series of stylized facts, which in turn lead us to examine two interrelated causal questions, ultimately concluding by contextualizing our results with respect to how much these findings should update beliefs.

First, we show that actual rates of gun violence are vastly understated by commonly used metrics, such as gun homicides or gunfire related 911 calls. For example, gun homicides understate the prevalence of gun fire incidents 72-fold. Importantly, this missed understanding of the scale of gun violence impacts communities substantially differently, leading to a distorted understanding of which communities are impacted most by gun violence. In fact, the best other common proxies only identify the most violent neighborhoods correctly approximately half the time, with Hispanic neighborhoods especially underrepresented by pre-existing proxies. Misclassifications, in turn, might influence which neighborhoods and communities are viewed as most in need of support and funding.

We next observe that most businesses are regularly exposed to gunfire. The average gunshot has nearly two dozen businesses around it, and in the reverse, over half of all businesses experience a gun violence incident in their immediate vicinity (on the same block) every year. Overall, the much higher than previously understood exposure, both in the rate and the direct business exposure of gun violence leaves open the causal question of how this exposure impacts business activity.

Using a regression discontinuity design, we find that in the short-run, an incident of gunfire leaves visits to businesses unchanged, with our estimation being able to rule out declines (or increases) as small as any change larger than 0.1%. In terms of revenue, this translates to ruling out larger total daily revenue changes than 15 cents. On the long-run, we observe similarly muted changes. Using a stacked difference in differences, we find that neighborhoods that experience a sudden sustained increase in gunfire do not experience changes in business visits.

Should these null effects update our shared preexisting prior about how gun violence might influence business activity? To test this, we gathered prior beliefs from experts (US-based PhD holders) in economics, and observe that both the short-run and long-run null estimates substantively update their pre-existing priors. In particular, short-term priors are updated to the extent that the prior and the posterior basely overlap.

Overall, we conclude that gun violence extensively reaches businesses in urban America, that victimizations and 911 calls vastly underestimate the amount of gunfire that happens in these areas, but single incidents of gunfire or compounding levels of gun violence do not change the amount of visits taking place to businesses—a fact novel to experts.

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Online Appendix to "Gun Violence and Business Activity"

A Additional details of data construction

This appendix supplements Section 2. Section A.1 records details of each data source that are not covered in the main text. Section A.2 describes the geographic and temporal conventions shared by every analysis file, and Sections A.3–A.6 describe, step by step, how each analysis sample is constructed.

A.1 Additional Details on Data Sources

SafeGraph foot traffic. SafeGraph's *Patterns* product is released monthly, with one record per business per month. Each business is identified by a *placekey*, a persistent identifier tied to the business's physical location, and each monthly record contains a vector of daily visit counts. We use the daily vector for the short-run analysis and the monthly total for the long-run analysis. We extract businesses in our study cities from the national files by SafeGraph's city and state fields, and assign each business to a 2020 census block using its coordinates. Businesses whose coordinates fall outside our study-city census geographies are not assigned a block and do not enter any analysis file.

We infer from visit patterns whether a business is open in a given month. We first balance the panel so that every business appears in every month from January 2018 to November 2022. A month in which a business does not appear in the Patterns file is recorded as zero visits. Let V_{it} denote visits to business i in month t, and let $\bar{V}^6_{it}$ and s^6_{it} denote the mean and standard deviation of visits over the six months ending in month t (months $t-5$ through t). A business is classified as *closed* in month t when four conditions hold: (i) $\bar{V}^6_{it} > 5$; (ii) $V_{it} < 0.1\,\bar{V}^6_{it}$; (iii) average visits from month t through the end of the sample are also below $0.1\,\bar{V}^6_{it}$; and (iv) $s^6_{it}/\bar{V}^6_{it} < 2$. Condition (iii) prevents temporary dips from being classified as closures, and conditions (i) and (iv) do the same for low-volume, erratic businesses. Once a business is classified as closed it remains closed in all later months. A business is *open* in month t if it is not closed and has positive visits in at least one of months $t-5$ through t. Businesses are therefore not counted as open in the months before their first recorded visit. At the start of the panel the six-month window is shortened to the months available since January 2018. The closure rule cannot apply in January 2018, and open-business counts for early 2018 should be read as lower bounds.

SafeGraph revenue. Revenue data come from SafeGraph's *Spend* product (distributed through Dewey), which reports monthly card transactions for a subset of placekeys from 2019 to 2022. We measure in-person revenue as total card spend minus online spend. The data exclude cash; in the restaurant industry, card payments account for roughly 65% of in-person payments over our sample period.[11] Revenue is observed for 23.6% of businesses in our sample. Coverage varies significantly by industry, as displayed in Table A1.

Expected revenue. To build a revenue measure that is defined for every business, we estimate, separately for each city, a dollars-per-visit rate for each 5-digit NAICS industry using the placekey-months with both visits and in-person spend observed. Specifically, for industry j in city c we fit $\text{Spend}_{ijt} = k_{jc} \times \text{Visits}_{ijt} + u_{ijt}$ by least squares through the origin, which gives $\hat{k}_{jc} = \sum_{i,t} \text{Spend}_{ijt}\text{Visits}_{ijt} / \sum_{i,t} \text{Visits}^2_{ijt}$. Fitting in levels retains placekey-months with zero spend or zero visits, and guarantees that a business with zero visits has zero expected revenue. Industries with fewer than 30 placekey-months in a city are assigned the rate for their 2-digit sector, and sectors with fewer than 30

[11] Authors' calculation from the Federal Reserve's Diary of Consumer Payment Choice, 2019–2022.

observations are assigned the city's pooled rate. To limit noise in thinly populated industries, we then apply empirical-Bayes shrinkage of each $\hat{k}_{jc}$ toward its 2-digit sector rate, with weight $\hat{\tau}^2/(\hat{\tau}^2 + \hat{\sigma}^2/\sum \text{Visits}^2)$, where $\hat{\sigma}^2$ is the residual variance of the industry-level fits and $\hat{\tau}^2$ is the estimated variance of true industry rates around their sector rate. Expected revenue for business i on day or month t is then $\hat{k}_{jc} \times \text{Visits}_{it}$, and we sum it across businesses to the block or ring level. For an individual business, expected revenue is therefore proportional to its visits. Aggregated across businesses, it weights each visit by the business's industry rate, so treatment effects on expected revenue differ from treatment effects on visits whenever the industry mix of the visits changes.

Acoustic gunshot detection. The data arrive from two kinds of sources. For 11 cities (Boston, Chicago, Denver, Fresno, Hartford, Jacksonville, Las Vegas, Louisville, Newark, Oakland, and Pittsburgh) we use alert-level records obtained directly from the vendor. These records include the number of rounds fired and the vendor's incident classification (single gunshot, multiple gunshots, or "probable gunfire or firecracker"). For the remaining seven cities, we use alert records published by the city, which report the time and location of each alert but not the number of rounds fired. For these seven cities, each alert is counted as one round. "Gunshots" in our tables and exposure measures are therefore rounds fired in the vendor-data cities and alerts in the public-data cities. Coverage dates for each city are reported in Table A3.

We clean the alert data in four steps. First, we drop alerts with missing coordinates or timestamps, and alerts outside 2018–2022. Second, we keep only alerts that geocode to a census block inside the study city. Third, to limit false positives from fireworks, we drop all alerts on Independence Day, New Year's Eve, Halloween, Cinco de Mayo, Memorial Day, and Labor Day, and on 27–30 May 2020, when protests following the murder of George Floyd produced unusual alert activity in several cities. Fourth, we define each day to run from 7am to 7am (Section 2), so we collapse all alerts and gunshots in the same census block on the same 7am–7am day to get a daily panel.

The vendor also provides shapefiles of each city's sensor coverage zones with the dates each zone was active. We treat a census block as covered by a zone if the block's centroid lies inside it, and define the block's coverage period as running from the earliest start date to the latest end date of any zone containing it.

Gun Violence Archive. We geocode each Gun Violence Archive incident in our study cities to a 2020 census block and collapse to the block-by-calendar-day level, recording the number of incidents and the number of victims killed and injured. Suspects killed or injured are recorded separately and excluded from our victim counts. Gun Violence Archive data are available citywide, regardless of sensor coverage.

911 calls for service. We assembled computer-aided dispatch records of 911 calls for service for 7 cities. Each city reports call types in its own taxonomy, so we classify calls as *shooting* or *shots fired* by matching on call-type descriptions within each city. Calls generated by acoustic sensors rather than by civilians are identified and dropped. Where the records include coordinates or street addresses rather than census geographies, we geocode them.

Census and ACS. Block-level population and racial composition come from the 2020 Decennial Census. Median household income and poverty rates come from the 2018–2022 American Community Survey at the block-group level and are assigned to every block in the block group.

A.2 Common Conventions

Geography The basic geographic unit in every file is the 2020 census block. Businesses, gunfire incidents, and Gun Violence Archive incidents are each assigned to blocks by point-in-polygon matching on their coordinates.

Distance rings All of our analysis files measure activity in a ring *around* a focal block. We compute the distance between the centroids of every pair of 2020 census blocks that lie within two miles of each other. A block's 0.5-mile ring contains every block whose centroid lies within 0.5 miles of the focal block's centroid, including the focal block itself. Rings of 0.1, 0.25, and 1 mile are defined analogously. "Visits within 0.5 miles" of a block are therefore visits to all businesses located in blocks in its ring, and a single business contributes to the ring of every block within 0.5 miles of it.

Sensor coverage. At the monthly level, a block-month is covered if the month falls within the block's coverage period. At the daily level, we keep block-days from 30 days before the start of a block's coverage period to 30 days after its end, and flag the days inside the period as covered. Within covered blocks and periods, missing gunfire counts are true zeros. Outside them, gunfire is unobserved and set to missing rather than zero.

A.3 Baseline Block-Month Panel

The baseline panel underlies the long-run and descriptive samples. It is a balanced panel of 145,756 census blocks observed monthly from January 2018 to December 2022 (8,745,360 block-months) within city limits. It is built as follows.

1. *Businesses.* We merge visits and revenue at the placekey-month level, apply the open/closed classification above, and sum visits, open businesses, and expected revenue to the block-month.
2. *Gunfire.* We sum gunfire incidents and rounds fired to the block-month.
3. *Gun Violence Archive.* We sum incidents, victims killed, and victims injured to the block-month.
4. *Coverage and demographics.* We attach the block-month coverage flag and the time-invariant census and ACS characteristics.

Exposure measures. For each block-month we compute gunfire *exposure* as the sum of gunshots in all blocks in the focal block's ring. We also compute the share of blocks in the ring that are covered in that month, so that exposure can be restricted to areas where gunfire is actually observed. Besides the 0.25-, 0.5-, and 1-mile rings, we construct a Gaussian-kernel version that weights each neighbouring block by $\exp\{-d^2/(2 \times 0.2^2)\}$, where d is centroid distance in miles. This weight falls to about 0.05 at half a mile.

A.4 Short-Run (Regression Discontinuity) Sample

The short-run sample is built at the block-day level from a spine around each gunfire incident.

1. *Block-day spine.* For each block with sensor coverage, we create one row per day from 30 days before the start of its coverage period to 30 days after its end.
2. *Gunfire.* We merge on the number of alerts and rounds fired in the block on each 7am–7am day.
3. *Business activity.* We expand each business's monthly record to daily visits, assign it to its block, and sum to block-days. For each focal block-day, we then sum visits over all blocks in its 0.1-, 0.25-, and 0.5-mile rings. Expected revenue is built the same way, from each business's daily visits multiplied by its industry rate $\hat{k}_{jc}$. Observed revenue is summed over the same rings on days for which spend data exist.

4. *Events.* Every block-day with at least one alert in the focal block is an event. For each event, we extract the 60-day window from 30 days before to 29 days after the event date. A block-day can appear in the windows of several events. We keep overlapping windows rather than dropping them, addressing dependence in inference directly using Conley (1999) standard errors. Since the distance kernel is 0 between the same blocks, this is more conservative than just clustering by block-day.

5. *Restrictions.* We drop events whose date falls on Independence Day or the day after, New Year's Eve, New Year's Day, Halloween, Memorial Day, or Labor Day. We keep only events for which all 60 days are observed, and drop the event day itself, so the post-period begins on the day after the incident. In specifications that use logs, days with zero visits (or revenue) in the ring are dropped, and events are kept only if every day within the bandwidth is still observed. For the intensity splits (Table 4, Panel C), we restrict to events with at least 2, 5, or 10 gunshots on the event day.

In estimation, observations are weighted by a triangular kernel in days from the event date multiplied by the number of alerts in the focal block on the event date. Standard errors are Conley (1999) with a 2-mile cutoff based on focal-block centroids.

A.5 Long-Run (Event Study) Sample

The long-run sample is built from the baseline block-month panel, balanced over January 2018 to November 2022. Let E_{bt} denote gunshots within 0.5 miles of block b in month t (its exposure), and let σ_E denote the standard deviation of E_{bt} across all block-months in the sample defined in step 1. In our sample, $\sigma_E = 64.1$ gunshots within 0.5 mi per month.

1. *Coverage restriction.* We keep block-months in which more than 80% of the blocks in the 0.5-mile ring are covered by sensors. Combined with step 5, a block therefore enters a stack only if this holds in every month of its 25-month window.

2. *Treatment events.* Block b has an event in month t if three conditions hold. First, the rise is abrupt: $E_{bt} - E_{b,t-1} > \sigma_E$. Second, it is sustained: mean exposure over months t to $t + 12$ exceeds mean exposure over months $t - 12$ to $t - 1$ by more than σ_E. Third, no other month in the preceding 12 months satisfies the first two conditions, so that a single episode is not counted repeatedly. This yields 1,948 events.

3. *Control blocks.* A block is a control if it is observed for at least 24 months and the standard deviation of its exposure over every trailing 24-month window remains below $0.25\,\sigma_E$. The same set of control blocks is used in every stack.

4. *Stacking.* We create one stack per event month. Each stack contains all blocks with an event in that month and all control blocks, observed from 12 months before to 12 months after the event month.

5. *Business restrictions.* Within each stack, we drop a block if it has no open business in any pre-event month, or if it has a missing or zero visit count in any of the 25 months.

6. *Weights.* Following Wing *et al.* (2024), control observations in each stack are reweighted so that each stack's contribution to the aggregate estimate is proportional to its share of all treated blocks.

The final sample has 4,410 blocks, 1,911 treated and 2,499 control, and 1,683,325 stacked block-months (column (4) of Table 1).

A.6 Descriptive Samples (Section 3)

For our neighborhood-level descriptives (Figures 3 and 4 and Table 2), we collapse our block-month panel to the Census tract-level. This block-month panel is restricted to just covered blocks (blocks covered more that 75% of the sample) before aggregation, so that only covered blocks in the tract are included for comparison with the gun violence metrics.

Both panels of Table 3 use the block-day gunfire incidents defined in Appendix A.1.

For Panel A, the unit of observation is a gunfire incident. For each incident, we count the businesses that are open in the month of the incident and located in the incident's block ("Same block") or in any block within 0.25, 0.5, or 1 mile of it. We then report the mean and median of these counts across incidents, and the share of incidents with no open business in the ring. All open businesses are counted, including those in blocks without sensor coverage.

For Panel B, the unit of observation differs by row. A business-month is *exposed* at a given radius if at least one gunfire incident occurs that month in a block within that radius of the business's block. Because gunfire is observed only inside the sensor coverage area, a business-month enters the denominator only if the business is open and its own block is covered by sensors in that month. The row "In the same month" is the share of these business-months that are exposed. "In the same year" is the share of business-years, among those with at least one such month, that have at least one exposed month. "Across our sample" is the share of businesses with at least one exposed month over the full sample. For the wider rings, part of the ring can lie outside the coverage area even when the business's own block is covered, so these shares are lower bounds on true exposure.

B Survey Instrument

Our survey was distributed through Qualtrics, and contained a single screen requiring a response from the respondent in every box. Each box was a text box, but the questions asking about our study effects required a signed integer response (and would ask the respondent to correct their response if the first character was not a + or - unless they entered 0). The questions were as follows:

- Please enter your name.
  First
  [response box]
  Last
  [response box]
- Q1: Suppose an urban neighborhood in the US with relatively high rates of poverty and crime experiences a gunfire incident. The event consists of four gunshots, all fired over a short timespan.

  What would you expect the **average causal effect** of this incident to be on foot traffic to local businesses within a half-mile radius (4–8 city blocks) in the short term (1–2 days)?

  Please answer as a **signed(+/-) percent change** relative to the baseline mean. The baseline mean is 555 visits over the two weeks prior to the incident.

  Low % (10th percentile)
  [response box]
  Best Guess %

[response box]
High % (90th percentile)
[response box]

"Best guess" is your expected average effect. "Low" and "High" are values such that you would put about a 10% chance of a causal effect below Low and about a 10% chance above High.

- Q2: Suppose an urban neighborhood in the US with relatively high rates of poverty and crime experiences a sustained rise in gunfire, from an average of 88 gunshots (21 distinct incidents) per month to an average of 169 gunshots (38 distinct incidents) per month over a 12-month period.

  What would you expect the **average causal effect** of this event to be on foot traffic to local businesses within a half-mile radius (4–8 city blocks) during the 12-month period?

  Please answer as a **signed(+/-) percent change** relative to the baseline mean. The baseline mean is 291 visits during a typical month before the increase in gun violence.

  Low % (10th percentile)
  [response box]
  Best Guess %
  [response box]
  High % (90th percentile)
  [response box]

  "Best guess" is your expected average effect. "Low" and "High" are values such that you would put about a 10% chance of a causal effect below Low and about a 10% chance above High.

C Additional figures and tables

Table A1: Top 10 Sectors by Number of Establishments

	Total sample			Sensor-covered sample		
Sector	Placekeys	Share of visits	Share with revenue	Placekeys	Share of visits	Share with revenue
Retail trade	54,909	0.206	0.344	12,706	0.220	0.322
Accommodation and food services	53,862	0.273	0.433	11,914	0.221	0.398
Other services	41,877	0.052	0.174	12,849	0.085	0.123
Health care and social assistance	39,481	0.071	0.120	7,571	0.086	0.095
Arts, entertainment, recreation	18,687	0.202	0.194	3,421	0.218	0.152
Finance and insurance	9,670	0.006	0.065	1,602	0.006	0.074
Real estate, rental and leasing	8,837	0.076	0.128	1,315	0.044	0.155
Educational services	7,909	0.052	0.109	2,205	0.061	0.087
Construction	7,446	0.005	0.048	1,478	0.005	0.035
Professional, scientific, technical	6,188	0.005	0.088	1,071	0.004	0.068
All sectors	275,980	1.000	0.236	62,309	1.000	0.210

Table A2: Summary statistics by analysis sample (aggregated to block-months)

	(2) Our cities	(3) Covered blocks	(4) Short-run sample	(5) Long-run sample
Panel A. Demographics				
Population	81	73	129	97
	(137)	(97)	(208)	(153)
Percent white	36.8	14.4	9.0	22.7
	(30.9)	(20.9)	(15.3)	(23.7)
Percent Black	27.1	46.7	60.0	45.9
	(33.6)	(37.9)	(35.4)	(35.8)
Percent Hispanic	23.6	30.1	23.2	19.7
	(25.7)	(32.0)	(29.4)	(24.7)
Poverty rate (%)	13.6	21.0	26.5	19.1
	(16.3)	(18.2)	(19.0)	(19.6)
Median household income ($)	71,738	50,154	40,911	60,245
	(43,265)	(28,854)	(21,986)	(37,266)
Panel B. Gunfire				
Gunfire incidents (Sensor)	0.127	0.125	0.715	0.142
	(0.435)	(0.428)	(1.713)	(0.481)
# Gunshots (Sensor)	0.393	0.396	1.572	0.227
	(2.021)	(2.029)	(4.604)	(1.102)
Shooting incidents (GVA)	0.004	0.009	0.029	0.012
	(0.068)	(0.097)	(0.182)	(0.115)
Victims killed (GVA)	0.0012	0.0023	0.0081	0.0037
	(0.0373)	(0.0509)	(0.0951)	(0.0639)
Victims injured (GVA)	0.0041	0.0088	0.0292	0.0120
	(0.0827)	(0.1214)	(0.2291)	(0.1425)
Panel C. Outcomes				
Businesses open	1.13	0.77	1.13	2.58
	(3.46)	(1.84)	(2.30)	(2.88)
Visits	227.1	153.0	273.0	608.0
	(1,684.3)	(1,392.2)	(1,919.1)	(3,516.2)
E[Revenue] ($)	6,374	4,202	4,222	5,569
	(48,738)	(23,423)	(9,070)	(33,261)
Revenue ($)	12,072	6,916	6,142	9,156
	(62,145)	(29,272)	(25,248)	(31,873)
Unit of observation	Block-month	Block-month	Block-month	Block-month (stacked)
Observations	8,745,360	2,562,360	648,132	1,683,325
Blocks	145,756	42,706	39,745	4,410
Cities	18	15	18	15

Notes: Means with standard deviations in parentheses. Column (2) is every block-month in the study-city panel and column (3) restricts it to sensor-covered blocks. Column (4) is the block-day RD event panel (aggregated to block-months for comparability), which conditions on being within 30 days of a gunfire alert. Column (5) is the stacked event-study panel, in which control blocks repeat across event groups.

Figure A1: Study cities

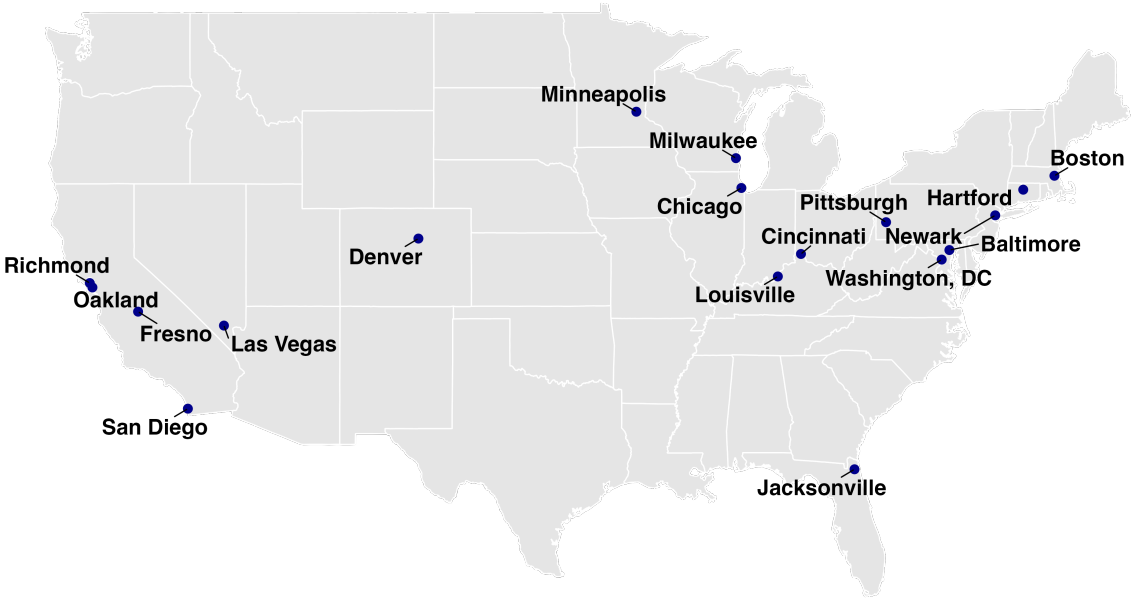
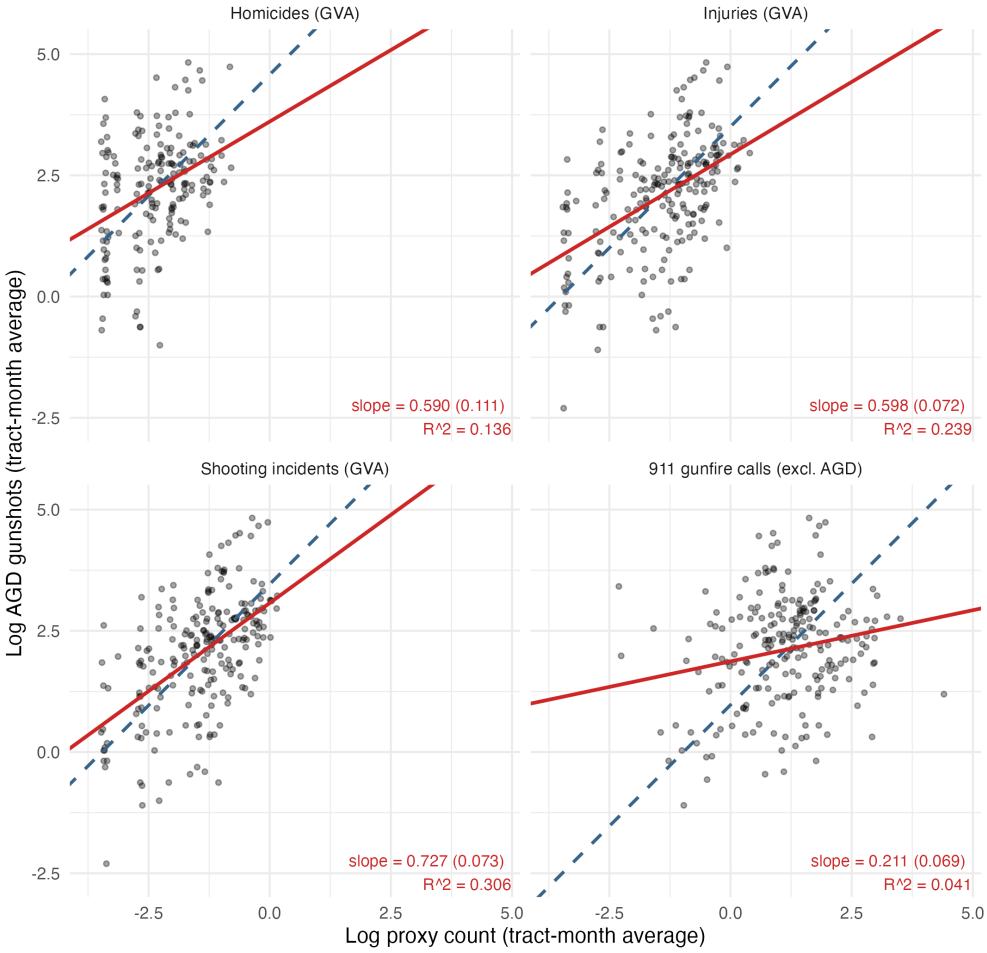


Table A3: Acoustic Gunshot Detection Data Coverage

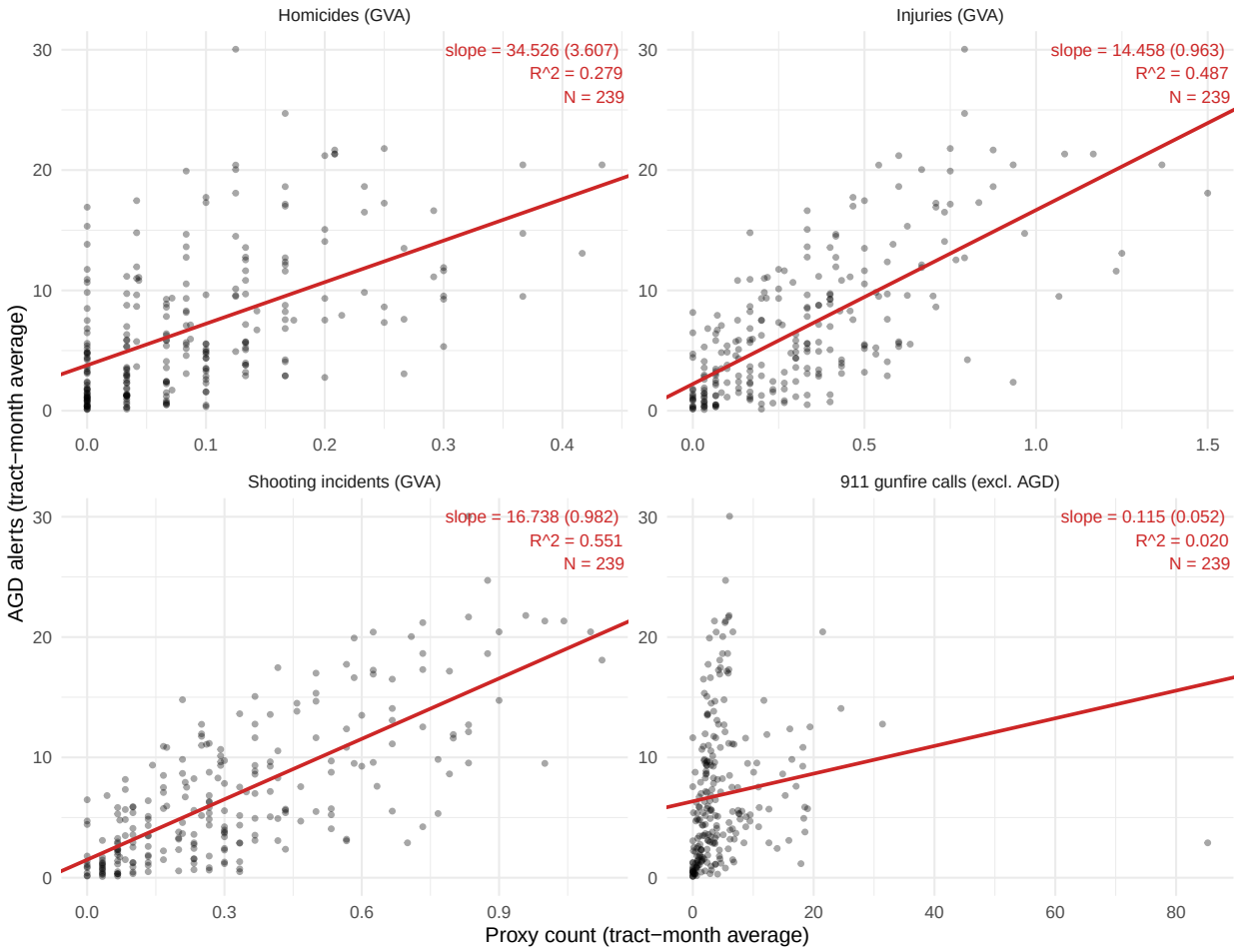
City	Start date	End date	Shooting incident data	Shot count data
Baltimore	May 2018	November 2022	Yes	—
Boston	January 2018	November 2022	Yes	Yes
Chicago	January 2018	November 2022	Yes	Yes
Cincinnati	January 2018	November 2022	Yes	—
Denver	January 2018	November 2022	Yes	Yes
Fresno	January 2018	November 2022	Yes	Yes
Hartford	January 2018	November 2022	Yes	Yes
Jacksonville	January 2018	November 2022	Yes	Yes
Las Vegas	November 2019	November 2022	Yes	Yes
Louisville	January 2018	November 2022	Yes	Yes
Milwaukee	January 2018	November 2022	Yes	—
Minneapolis	January 2018	November 2022	Yes	—
Newark	January 2018	November 2022	Yes	Yes
Oakland	January 2018	November 2022	Yes	Yes
Pittsburgh	January 2018	November 2022	Yes	Yes
Richmond (CA)	October 2019	December 2020	Yes	—
San Diego	January 2018	September 2021	Yes	—
Washington, DC	January 2018	November 2022	Yes	—

Figure A2: Standard proxies are measured with substantial amounts of error



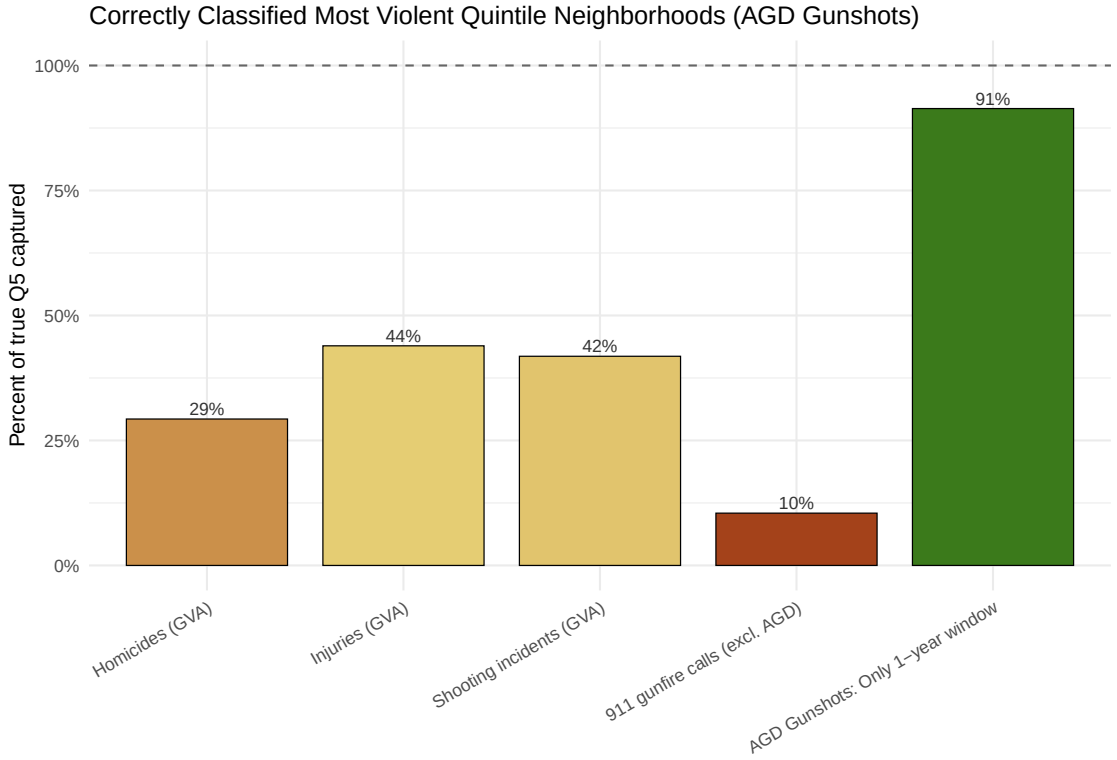
Notes: This exhibit displays plots that scatter the logarithm of acoustically detected gunshot counts (Y-axis) against the logarithm of a common proxy (X-axis). The dashed line denotes the 45 degree line that goes through the mean of the data. The red line denotes the line of best fit. The slope and R^2 of the best-fit line are reported in the bottom right hand corner.

Figure A3: Standard proxies are measured with substantial amounts of error



Notes: This exhibit displays plots that scatter the levels of acoustically detected gunshot incidents (Y-axis) against the levels of a common proxy (X-axis). The top left panel gives the correlation between homicides and actual gunshot incidents. The top right panel gives the correlation between reported gun violence injuries and actual gunshot incidents. The bottom left panel gives the correlation between gun violence archive incidents and actual gunshot incidents. The bottom right panel gives the correlation between 911 calls and actual gunshot incidents. The dashed line denotes the 45 degree line that goes through the mean of the data. The red line denotes the line of best fit. The slope and R^2 of the best-fit line are reported in the bottom right hand corner.

Figure A4: Correctly classified most violent quintile neighborhoods

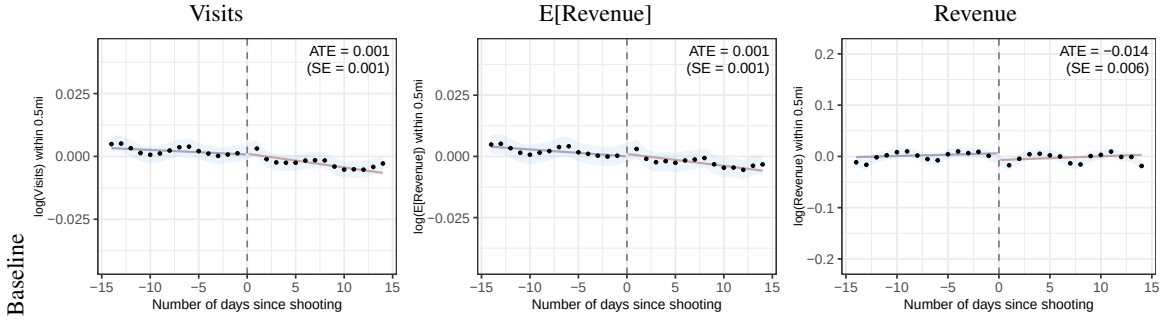


Notes: This figure plots the frequency of type-2 errors for various gun violence proxies. Specifically, we define a neighborhood as high-crime if it falls in the top quintile of average gun-violence as measured using the acoustically detected gunshots over our entire sample period. We then construct similar top-quintile indicators using the proxy variables (X-axis). We also construct a similar indicator restricted to 1 year of acoustically detected gunshot data–this benchmark let's us explore the role of measurement error in small samples. The height of the bars denotes the rate of agreement between the proxy and actual gun-violence with respect to classifying the highest gun-violence neighborhoods.

Table A4: "First stage" RD estimates

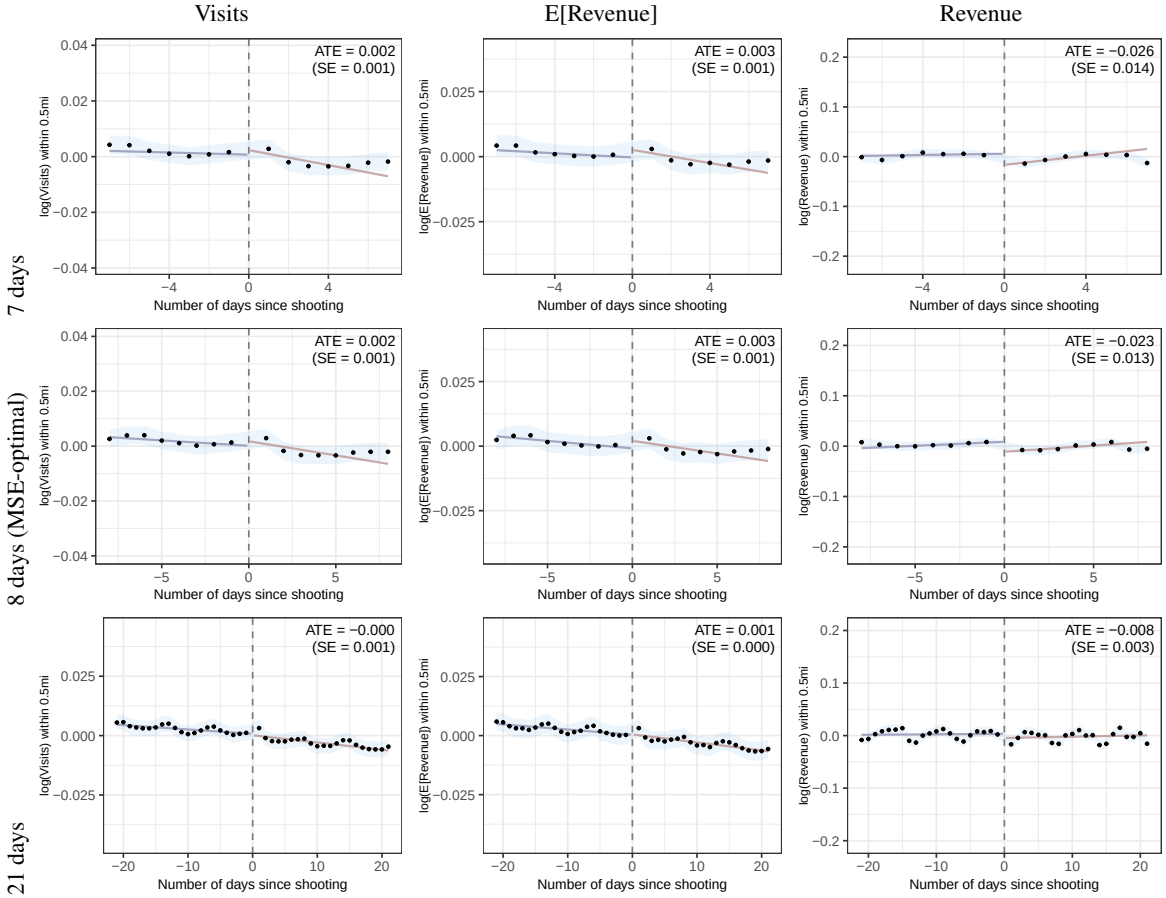
	GVA injuries	GVA homicides	911 shootings/shots fired
RD estimate	0.0096***	0.0021***	0.0488**
	(0.0023)	(0.0003)	(0.0154)
Pre-period mean	0.0564	0.0140	0.5327
Observations	9,067,123	9,067,123	2,240,666
Events	323,926	323,926	81,317
Cities	18	18	8

Figure A5: RD robustness: baseline specification



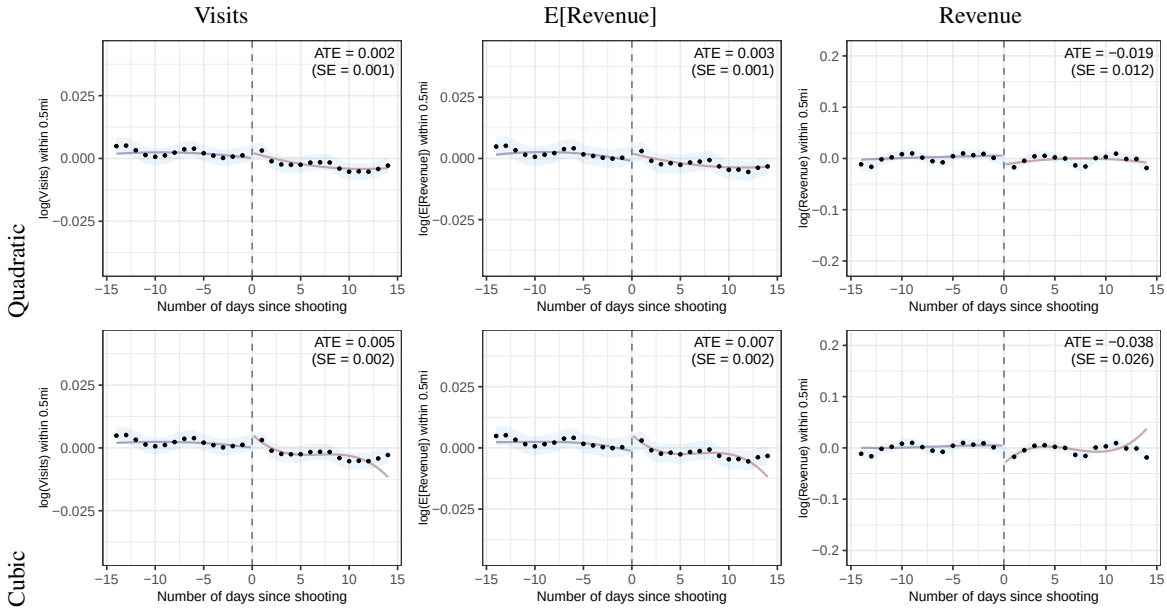
Notes: Each row varies one dimension away from the baseline specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot); all other settings are held at the baseline. Columns match Table A5. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Figure A6: RD robustness: bandwidth



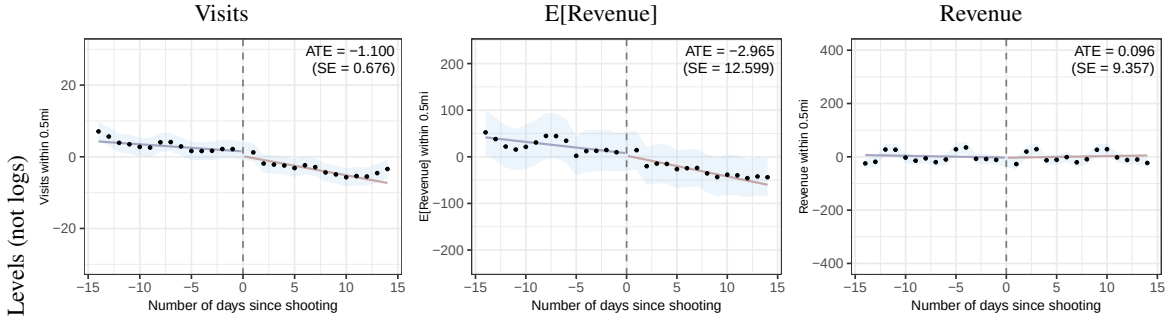
Notes: Each row varies one dimension away from the baseline specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot); all other settings are held at the baseline. Columns match Table A5. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Figure A7: RD robustness: polynomial order



Notes: Each row varies one dimension away from the baseline specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot); all other settings are held at the baseline. Columns match Table A5. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Figure A8: RD robustness: outcome in levels



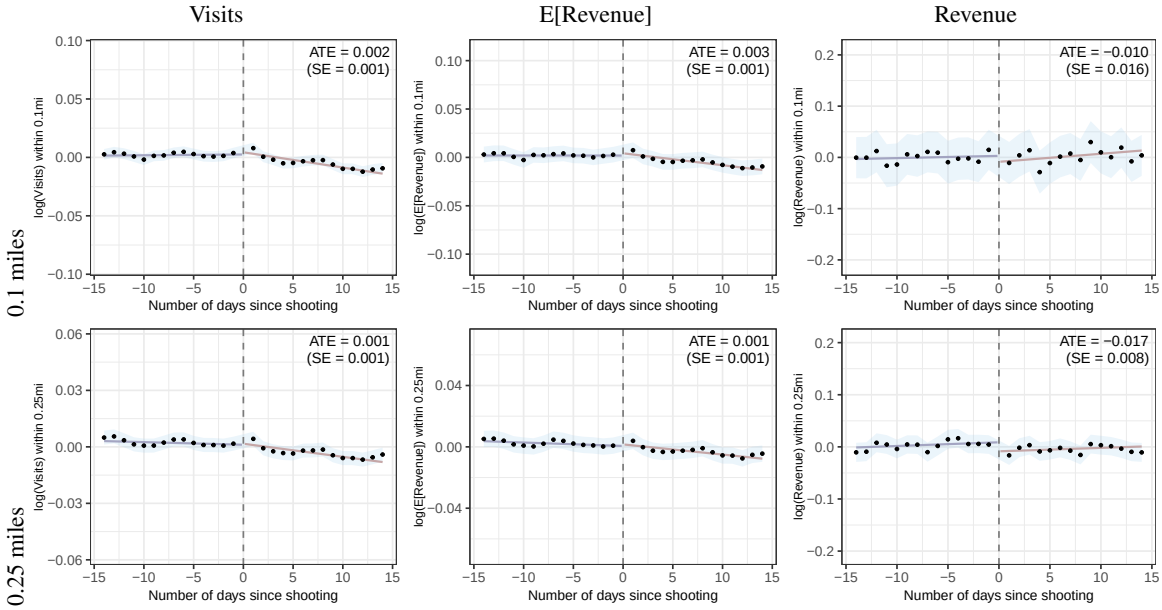
Notes: Each row varies one dimension away from the baseline specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot); all other settings are held at the baseline. Columns match Table A5. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Table A5: Robustness of the regression discontinuity estimate

	Visits	Predicted revenue	Observed revenue
Baseline	0.0006	0.0014*	-0.0136*
	(0.0005)	(0.0006)	(0.0057)
Panel A: Bandwidth			
7 days	0.0015	0.0032**	-0.0260
	(0.0011)	(0.0012)	(0.0142)
8 days (MSE-optimal)	0.0017	0.0034**	-0.0231
	(0.0010)	(0.0011)	(0.0130)
21 days	-0.0001	0.0006	-0.0084*
	(0.0006)	(0.0005)	(0.0033)
Panel B: Polynomial order			
Quadratic	0.0022*	0.0035**	-0.0188
	(0.0011)	(0.0011)	(0.0123)
Cubic	0.0047*	0.0070***	-0.0378
	(0.0019)	(0.0020)	(0.0257)
Panel C: Outcome in levels			
Levels (not logs)	-1.1002	-2.9645	0.0962
	(0.6756)	(12.5986)	(9.3568)
Panel D: Radius			
0.1 miles	0.0024*	0.0027*	-0.0100
	(0.0010)	(0.0013)	(0.0159)
0.25 miles	0.0010	0.0012	-0.0174*
	(0.0006)	(0.0008)	(0.0083)
Panel E: Gunshot threshold			
≥ 2 gunshots	-0.0001	0.0008	-0.0077
	(0.0009)	(0.0007)	(0.0069)
≥ 5 gunshots	-0.0003	0.0007	-0.0021
	(0.0011)	(0.0009)	(0.0083)
≥ 10 gunshots	-0.0013	-0.0008	-0.0025
	(0.0021)	(0.0015)	(0.0137)

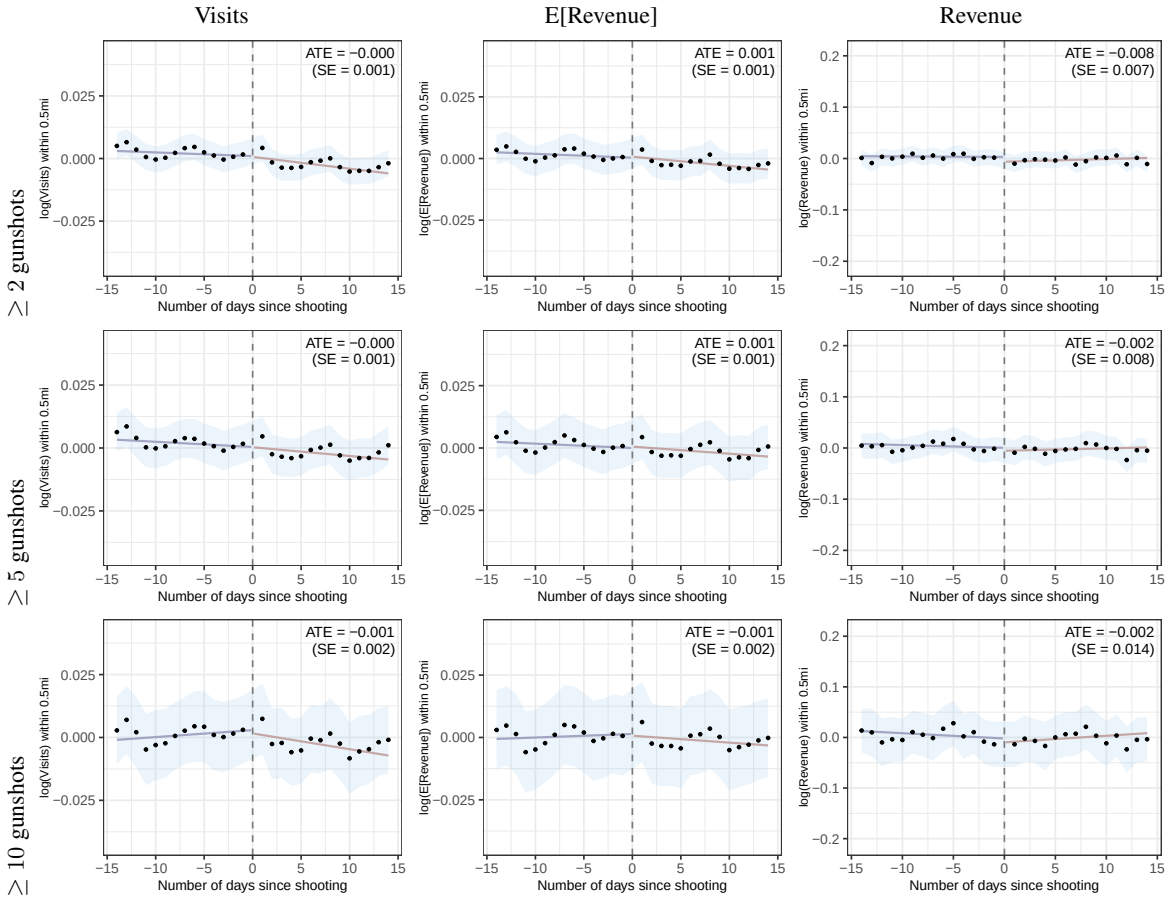
Notes: Each row reports the RD estimate with its standard error in parentheses, and deviates from the main specification in exactly one dimension. The main specification is a 0.5 mile radius, a bandwidth of 14 days, linear polynomial, day-of-week residualization, a log outcome, and events with at least one gunshot. Standard errors are Conley with a 2-mile cutoff. The levels row is in units of the outcome rather than log points, so its magnitude is not comparable to the other rows: visits are counts and predicted revenue is dollars, the latter constructed as a NAICS-specific dollars-per-visit rate times visits.

Figure A9: RD robustness: radius



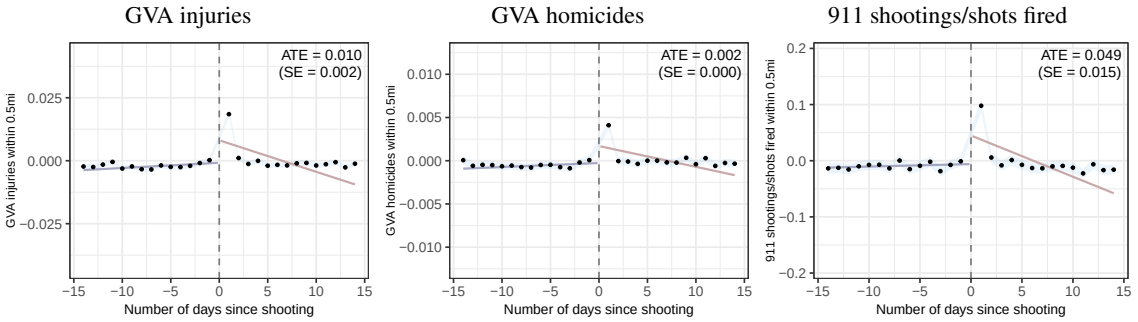
Notes: Each row varies one dimension away from the baseline specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot); all other settings are held at the baseline. Columns match Table A5. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Figure A10: RD robustness: gunshot threshold



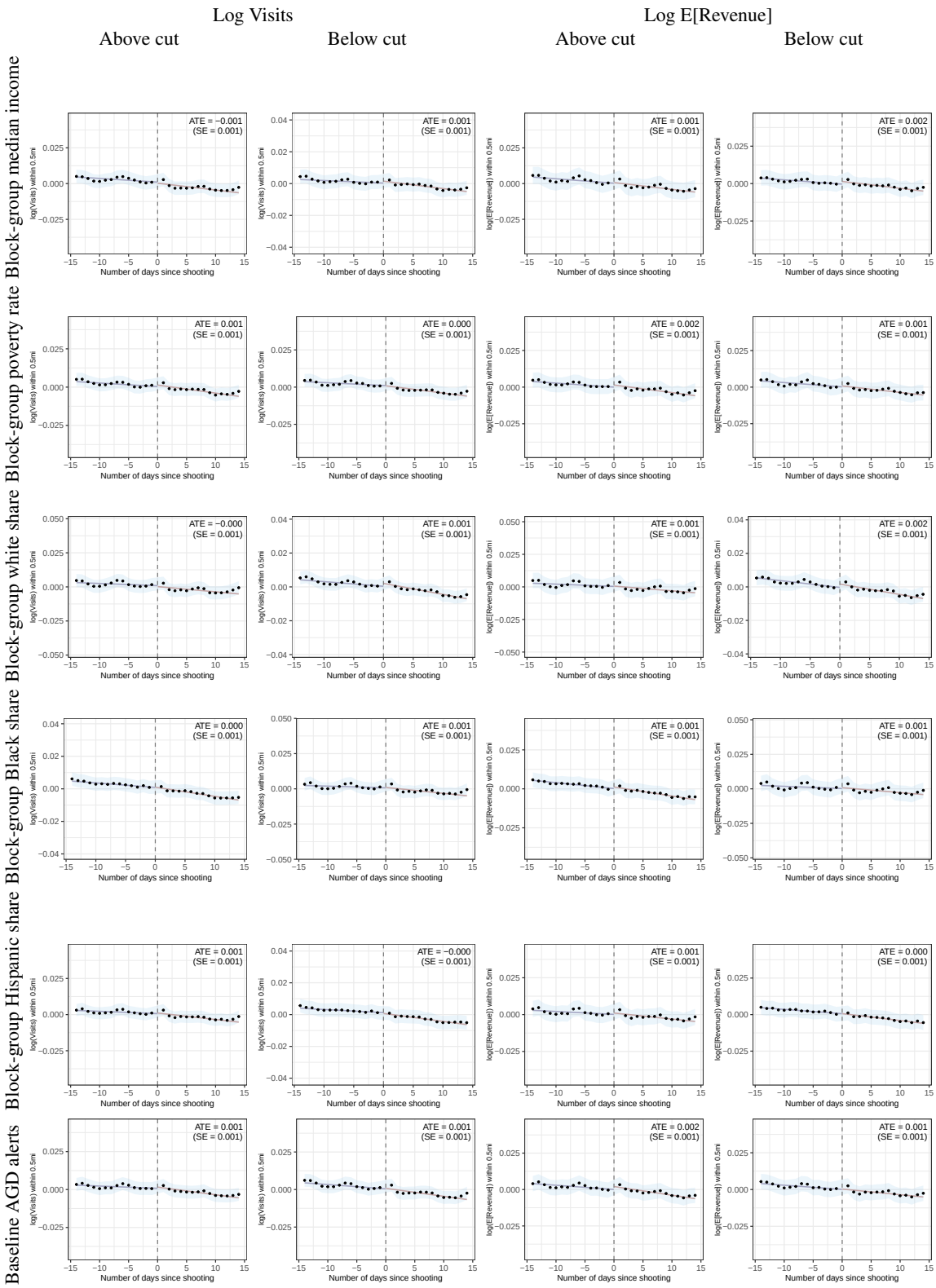
Notes: Each row varies one dimension away from the baseline specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot); all other settings are held at the baseline. Columns match Table A5. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Figure A11: RD estimates for violence outcomes



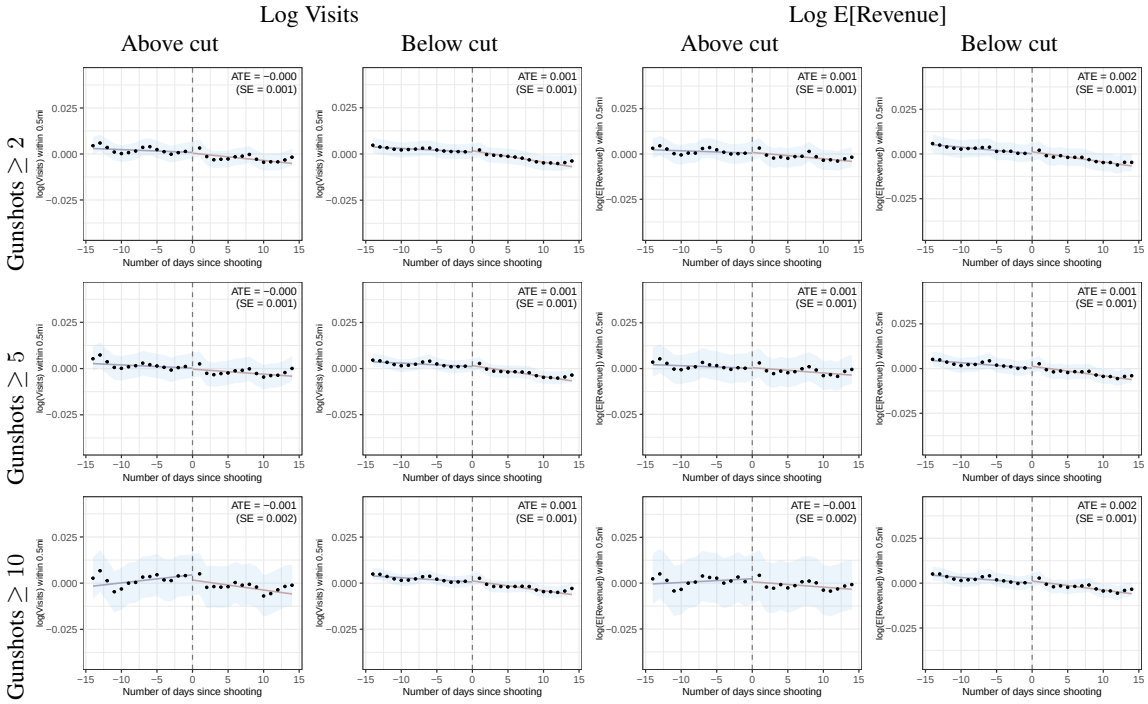
Notes: Baseline specification (0.5mi radius, linear, 14-day bandwidth, day-of-week residualized, ≥ 1 gunshot), estimated in levels rather than logs because these outcomes are zero on nearly every block-day. 911 covers only the 8 of 18 cities with block-level call geocoding and most of those records end during 2021, so it is a different and much smaller sample than the GVA outcomes and the two should not be compared directly. Results also displayed in Table A4.

Figure A12: RD heterogeneity: by neighborhood characteristics



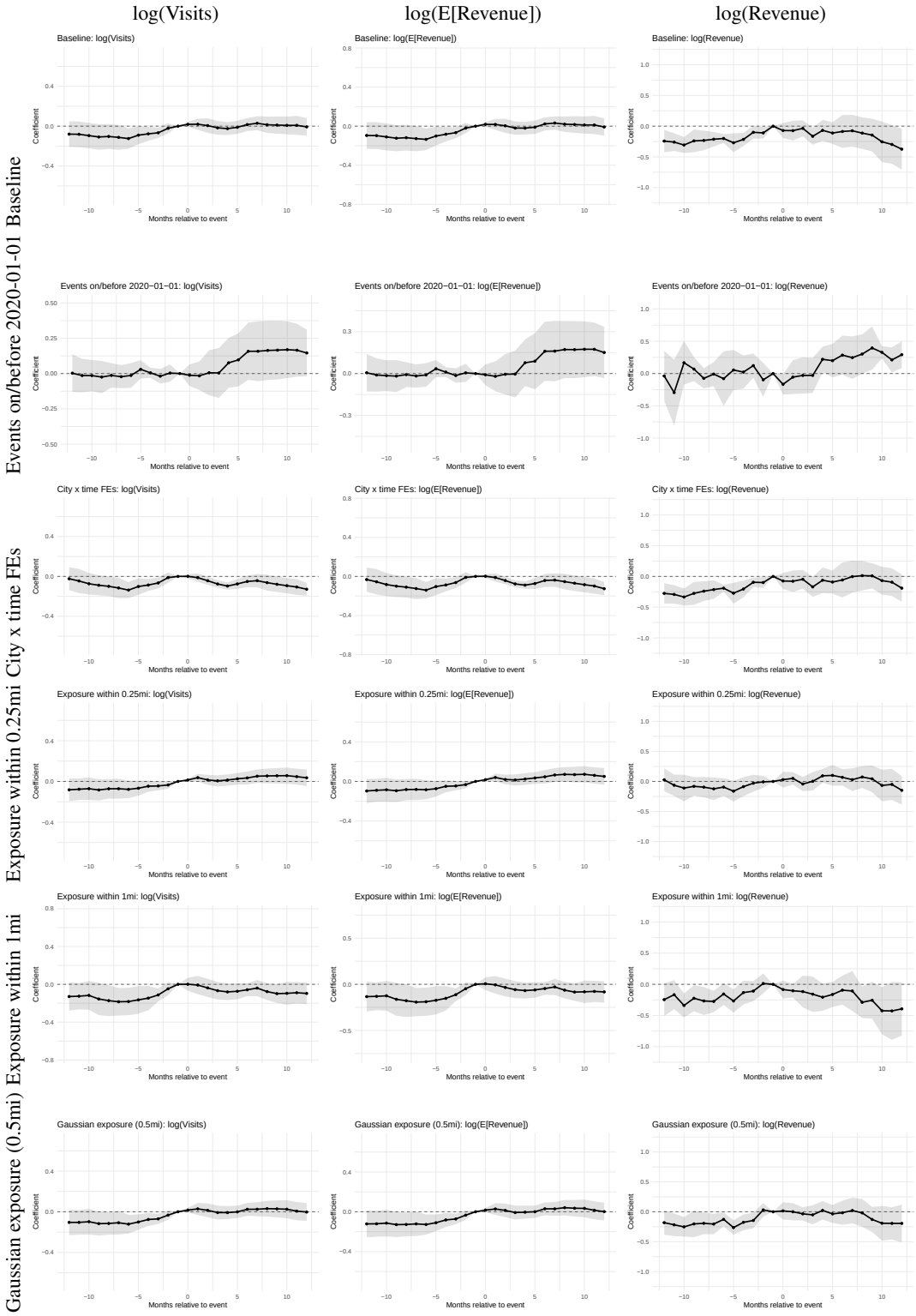
Notes: Each row splits the main specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot) at the stated cut, and plots the two halves side by side for each outcome. Rows and columns match Table 4's heterogeneity panels. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Figure A13: RD heterogeneity: by incident intensity



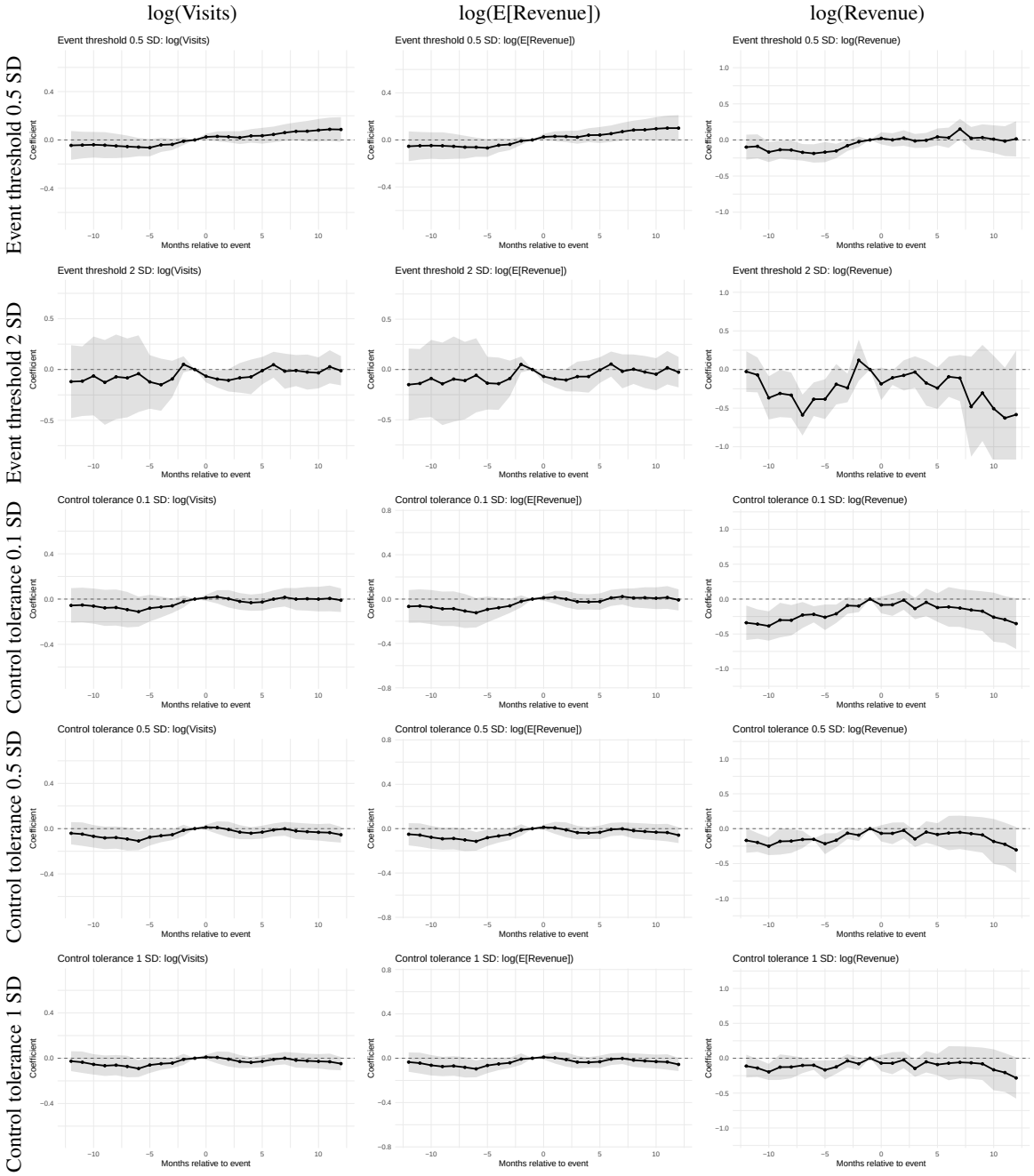
Notes: Each row splits the main specification (0.5mi radius, linear, 14-day bandwidth, logs, day-of-week residualized, ≥ 1 gunshot) at the stated cut, and plots the two halves side by side for each outcome. Rows and columns match Table 4's heterogeneity panels. Points are weighted binned means of the residualized outcome and lines are the fitted local polynomial either side of the event date; shaded bands are 95% confidence intervals.

Figure A14: Event study: log outcomes, sample and exposure definition



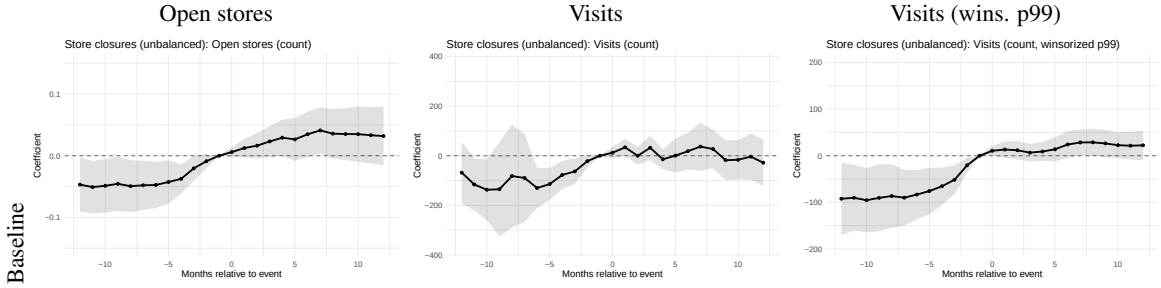
Notes: Each row varies one dimension away from the baseline specification (All events, 0.5mi exposure, 1 SD event threshold, 0.25 SD control tolerance, logs). Within each panel, the X-axis denotes event time. Points denote event study coefficients. Grey bands denote 95% confidence intervals constructed using Conley (1999) standard errors that allow for spatial dependence within a 2 mile radius. The Y-axis is scaled to 2 within-block standard deviation of the outcome in treated blocks in the pre-period.

Figure A15: Event study: log outcomes, event threshold and control tolerance



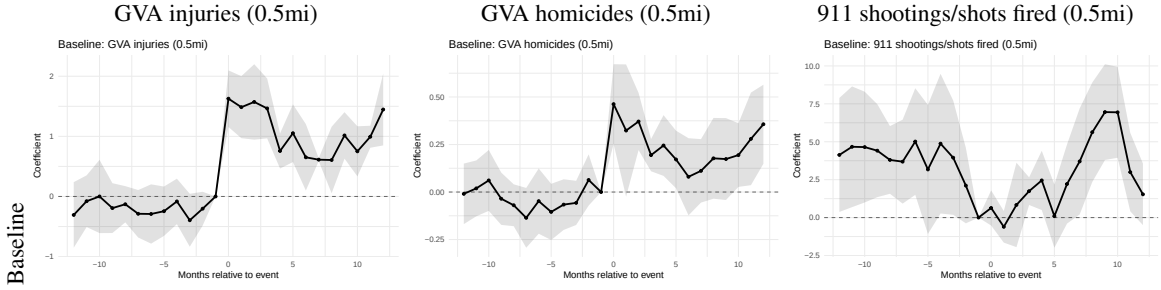
Notes: Each row varies one dimension away from the baseline specification (All events, 0.5mi exposure, 1 SD event threshold, 0.25 SD control tolerance, logs). Within each panel, the X-axis denotes event time. Points denote event study coefficients. Grey bands denote 95% confidence intervals constructed using Conley (1999) standard errors that allow for spatial dependence within a 2 mile radius. The Y-axis is scaled to 2 within-block standard deviation of the outcome in treated blocks in the pre-period.

Figure A16: Event study: levels outcomes, zero-visit blocks retained



Notes: Each column denotes an alternate outcome using the baseline specification. The left figure shows that our restriction of non-zero visits (and thus, no block having all stores close) does not drive our result. The middle column shows the outcome in levels retaining the zero-visit blocks, and the last column windsorizes block visits at the 99th percentile ensuring our results aren't driven by outliers. Within each panel, the X-axis denotes event time. Points denote event study coefficients. Grey bands denote 95% confidence intervals constructed using Conley (1999) standard errors that allow for spatial dependence within a 2 mile radius. The Y-axis is scaled to 2 within-block standard deviation of the outcome in treated blocks in the pre-period.

Figure A17: Event study: violence outcomes



Notes: Each column denotes an alternate violence outcome using the baseline specification. The final column is estimated on a subsample with only 21% of the events, so it is not comparable to the main results sample. Within each panel, the X-axis denotes event time. Points denote event study coefficients. Grey bands denote 95% confidence intervals constructed using Conley (1999) standard errors that allow for spatial dependence within a 2 mile radius. The Y-axis is scaled to 2 within-block standard deviation of the outcome in treated blocks in the pre-period.