

# The Impact of Police Shootings on Gun Violence and Civilian Cooperation

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## Abstract

This paper studies the effect of police-involved shootings on gun violence and civilian cooperation with police, as proxied by crime reports made via 911 calls. To distinguish between crime reporting and crime incidence, we use administrative data on 911 calls and ShotSpotter data from Minneapolis. Exploiting the variation in the timing and the distance to these incidents, we show that exposure to a police shooting increases gun-related crimes by 5-6 percent, and decreases shots reported by 1-2 percent. Taken together, this implies police shootings reduce civilian crime reports to police by 6-7 percent.

**Keywords:** Crime, Crime reporting, Police use of force

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# 1 Introduction

Law enforcement agencies in the US are substantially more involved in violent contact with citizens than those in other advanced industrial countries (Edwards et al. 2019), resulting in adverse consequences on institutional trust. In 2019, only 55 percent of civilians reported confidence in the police (Brenan 2021). Confidence fell further in the aftermath of George Floyd’s murder, when Minneapolis took part in an unprecedented vote on whether to dismantle their current police department. Although the proposal was rejected, it was a close race, with 44 percent of voters in favor (Kaste 2021). Such instances reflect the lack of civilian trust in police departments, which has important implications for how effectively police will be able to serve neighborhoods going forward.

However, while a sizeable literature has focused on estimating the deterrent effect of the number of police on crime (Levitt 1997; Di Tella and Schargrotsky 2004; Evans and Owens 2007; Draca et al. 2011; Chalfin and McCrary 2017; Mello 2019; Weisburst 2019), little is known about the effect of policing quality. This is especially true as it relates to police violence and its impact on civilian trust in the police. In this paper, we focus on the extent to which police use of force affects two aspects of public safety, gun violence and crime reporting rate as a measure of civilian cooperation.

The difficulty in identifying the effect of police shootings on civilian cooperation - as proxied by crime reporting- is that these shootings can have direct effects on crime as well as reporting. For example, following the police shooting of Michael Brown in 2014, there was a noticeable rise in violent crime in Ferguson, Missouri. This rise was attributed to a reduction in police activity as a result of public scrutiny, a phenomenon referred to as the “Ferguson Effect” (Lind 2016). Thus, most papers that study the effect of police violence on crime reporting rely on the volume of 911 calls as a proxy for reporting (Baumer 2002; Desmond et al. 2016; Zoorob 2020). However, the volume of 911 calls is a function of both crime incidence and the reporting rate. In the absence of a true measure of crime, it is unclear whether any observed changes in the volume of 911 calls are due to shifts in reporting behavior or changes

in the underlying level of crime.

We use a substantially different approach to overcome this problem. Specifically, we ask how police-involved shootings impact civilian reports of subsequent shootings. We do so because this enables us to use an objective measure of shootings – those that are detected by ShotSpotter devices. ShotSpotter is a system of audio sensors that detects and analyzes gunshot sounds and sends notifications to police departments with the exact time and location of each incident. Using ShotSpotter data, we observe the universe of gunshot crimes occurring in a certain geography and are able to estimate the effect of a police shooting on both gun violence and its reporting.

We utilize data from Minneapolis, Minnesota on 911 calls, ShotSpotter activations, and police-involved shootings from 2009 to 2019. Using the addresses of these incidents, we locate them in Census blocks in the city. During our sample period, Minneapolis experienced 57 unique police-involved shootings, most of which involved a Black individual (72 percent). To estimate the effect of police shootings, we exploit the variation in the location and the timing of these incidents in a difference-in-differences model, where we compare exposed Census blocks to other blocks over time.

Our results indicate that police shootings lead to a 5-6 percent increase in gun violence in exposed blocks relative to unexposed blocks and a 1-2 percent decrease in shots reported. We conclude that police shootings cause a 6-7 percent decrease in the reporting rate. These results are not sensitive to the choice of the comparison group, the length of the pre- and post-periods, or the sample of police shootings we consider. When we incorporate alternative estimators from Callaway and Sant’Anna 2021 and Sun and Abraham 2021 to account for potential biases in our two-way fixed effect estimation method, the results remain consistent.

Using detailed information on the location of police shootings, we estimate the effects of these shootings by neighborhood race. Our findings indicate that exposure to a police shooting has larger effects in Minority neighborhoods compared to White ones. While we estimate a 7 percent increase in ShotSpotter incidents and a 2 percent decrease in shots

reported in Minority neighborhoods, these effects are not statistically significant in White neighborhoods. This suggests that the increase in gun-related crimes and the decrease in the reporting rate are entirely driven by Minority neighborhoods. Additionally, we show heterogeneity in the effects depending on whether the shooting resulted in fatality. Interestingly, we find that the effects are larger when the shooting is nonfatal. Moreover, we explore long-run effects of police-involved shootings by focusing on those that occur prior to 2015. Our findings indicate that the increase in gun violence persists for at least 4 years after a police shooting. Conversely, shots reported remain unchanged.

Finally, we use police-initiated calls such as traffic stops and patrolling events, as well as arrest data, to investigate whether the increase in gun violence is caused by a reduction in police activity. Our difference-in-differences estimates reveal no evidence of a decrease in police activity in treated blocks relative to control blocks following a police shooting, indicating that the effects are not driven by “de-policing”.

Our paper contributes to a growing literature that studies the consequences of exposure to police use of force (Baumer 2002; Zoorob 2020; Ang 2021; Desmond et al. 2016; Legewie and Fagan 2019; Gershenson and Hayes 2018). The main contribution of this paper is that we can estimate the effect of police-involved shootings on crime incidence and crime reporting separately, overcoming a major hurdle in the criminal justice literature. In this regard, our paper also contributes to the literature on the under-reporting of crime (Carr and Doleac 2016, Jácome 2022, Miller and Segal 2019, Cho et al. 2023). Additionally, we are able to distinguish between police-initiated calls and citizen-initiated calls, which is fundamental for studying civilians’ behavior. In doing so, this paper is most closely related to Ang, Bencsik, Bruhn and Derenoncourt 2021, who study the impact of George Floyd’s murder on the ratio of shots reported to shots fired in multiple US cities.

Our setting and design have two main advantages relative to the existing literature. First, we focus on common, low-profile police shootings that are likely more representative of the majority of incidents involving police use of force on civilians. These are mostly

non-fatal and receive less media attention. In contrast, the killing of George Floyd and its aftermath arguably make it the most unique police-civilian event in decades. Videos of the incident circulated in an extremely rapid manner across social media, leading to record-breaking protests across the country. These protests were covered by US media more than any other protests in the past two decades (Heaney 2020). Indeed, using different data sources such as LexisNexis and Google Trends, we show that George Floyd’s death received significantly more attention than police shootings in Minneapolis, even the fatal shootings. Thus, while the impact of Floyd’s murder on policing and civilian reporting is certainly important and interesting in its own right, it is unusual relative to other police shootings and killings.

A second advantage of our study is our rich data. Specifically, we are able to differentiate between civilian-initiated and police-initiated 911 calls, which allows us to examine underlying mechanisms. This is important because incidents of police violence, particularly when highly publicized, may lead to subsequent protests and riots, prompting a reduction in policing activity. In this case, it is challenging to disentangle the effect of a police killing from the effect of other simultaneous events. However, we rule out any decreases in policing activity as a potential explanation for the results in our setting.

More broadly, this paper contributes to a growing literature on policing, including research on racial disparities in policing (e.g., Hoekstra and Sloan 2022; Chalfin et al. 2022; Goncalves and Mello 2021; Rim et al. 2020; West 2018), police misconduct (e.g., Goncalves 2020; Cunningham et al. 2021), diversity in policing (e.g., Ba et al. 2021), and police oversight (e.g., Cheng and Long 2018; Ba and Rivera 2019; Rozema and Schanzenbach n.d.).

Our results have important implications for policing and public policy. Exposure to police violence can jeopardize the relationship between law enforcement agencies and society. This is detrimental to public safety, given that the police cannot prevent or solve crimes without civilian cooperation. In addition to a decrease in crime reporting, crime levels, specifically gun violence, increase following these incidents, counteracting the primary

policing goal of preventing crime.

## 2 Data

We obtain data from the Minneapolis Police Department on 911 calls for service, ShotSpotter activation incidents, arrests, and police-involved shootings from 2009-2019. We supplement these with data from Fatal Encounters, LexisNexis, Google trends, and the 2010 Census for further analyses. As previously discussed, we focus on shooting crimes in order to distinguish between actual crime incidence and its reporting.

### 2.1 Outcome variables

To measure the number of gun-related crimes, we use publicly available ShotSpotter data obtained from the city’s open data website (Minneapolis Open Data 2020). These data include all ShotSpotter activation incidents that occurred between 2007 and 2019, along with their location and time. ShotSpotter devices record all gunfire incidents, whether reported or not, through audio sensors and artificial intelligence that discern sound frequencies.<sup>1</sup> The sensors detect the pulses and filter out background noises to rule them as a potential shooting. The device then analyzes the time and the angle of arrival to establish the location of the pulses. The system uses algorithms and machine learning to compare the sound to a database of gunfire sounds, and then determines whether the incident is gunfire. Finally, the system sends it into an “Incident Review Center” which makes the final confirmation. This process takes almost 60 seconds and provides 97 percent accuracy according to the company.<sup>2</sup>

ShotSpotter provides the unique advantage of capturing all gun-related crimes, regardless of whether they were reported to the police by civilians or officers. This feature implies that its detection of incidents does not depend on the reporting rate, allowing us to overcome

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<sup>1</sup>Source: <https://www.shotspotter.com/technology/>. Accessed on January 8, 2021.

<sup>2</sup>Source: <https://www.shotspotter.com/company/>. Accessed on January 8, 2021.

the challenge of under-reported crimes, a common limitation of other crime measures. In addition, the technology is proven to be highly accurate. For instance, a study funded by the National Institute of Justice shows that ShotSpotter detects actual gunshots with 99.6 percent accuracy (Goode 2012). Nevertheless, the technology might misclassify certain sounds, like fireworks, as gunshots. The misclassification could potentially explain why in Chicago, between 2010 and 2021, only 9.1 percent of ShotSpotter activation incidents resulted in a gun-related offense (Ferguson and Witzburg 2021). The discrepancy could also be a function of the lack of supplementary evidence, such as 911 calls or physical evidence at the scene.

As researchers, we are unable to distinguish between false positives and actual gunshots that don't result in an offense due to the lack of supporting evidence. However, any measurement error from false positives is plausibly uncorrelated with treatment status or timing. To the best of our knowledge, the algorithm is controlled by the ShotSpotter company, and it should operate uniquely across all devices at any point in time. Moreover, the decision to classify a sound as a gunshot is determined at the ShotSpotter Incident Review Center. Consequently, the likelihood of a false positive across exposed and unexposed blocks is random, and any resulting measurement error in ShotSpotter incidents will increase the standard errors without introducing bias. Despite these limitations, ShotSpotter is still considered the best measure of the true number of shootings against which we can compare the number of shootings reported by civilians. To minimize the potential imprecision resulting from measurement error, we follow Carr and Doleac 2016 and Ang et al. 2021 by excluding days when the likelihood of a false ShotSpotter activation is high, likely due to fireworks. These days includes New Year's Eve, New Year's day, and July 4th.

ShotSpotter devices were first introduced to the South Side police district in Minneapolis in 2007. Eventually, more devices were installed in the North Side, another area that is "troubled by gun violence" (Mannix and Nehil 2016). We were unable to acquire information about the exact location and the installation date of ShotSpotter devices in the city. Hence, we focus on Census blocks where we observe at least one ShotSpotter activation incident

in 2007, 2008, or 2009. We refer to these blocks as the “in-sample” blocks. This ensures that ShotSpotter devices were installed in all the blocks in our sample since the beginning of our sample period. Figure A1 shows the Census blocks that meet this criterion. They are mostly concentrated in the north west and south east side of the city, and they account for 17 percent of the total number blocks constituting the city (total of 976 in-sample blocks).

To measure the number of shots reported, we use 911 calls for service. Our data include more than 4.5 million events in total, where we observe the time, date, location, problem, and disposition of each call. We also observe the source of the call, whether it was citizen-initiated or officer-initiated. Making this distinction is pivotal to estimate changes in *civilians*’ reporting behavior. In some police departments, such as Minneapolis, calls for service data include both civilian-initiated calls and officer-initiated calls. A failure to distinguish between these two types could lead to falsely attributing a change in police behavior to civilians (Lehman 2021). In Minneapolis, we are able to make the distinction between these two types of calls, and we observe that 58 percent of the calls for service are civilian-initiated.

We focus on calls where citizens reported hearing gunshots to construct our main outcome variable of number of shots reported. Specifically, we identify calls where civilians explicitly reported “sounds of shots fired,” clearly indicating a gun-related offense. If civilians use different phrases to report gunshots, particularly in cases where the gunshots were part of or coincided with another incident, or if the call taker coded the incident differently, then our outcome variable would be considered a conservative measure of shots reported. However, since we identify reports of gunshots uniformly across both treatment and control groups, any potential error in measuring our outcome variable affects the standard errors without causing bias. Our preferred analysis uses shots reported as defined by “sounds of shots fired,” since we can only use the ShotSpotter data to measure crime incidence for gun-related crimes. Nonetheless, as a robustness check, we construct a broader measure of shots reported. This measure also includes incidents involving a gun (that do not explicitly mention shots fired) or a weapon (even if the type of the weapon is not specified), such as



calls reporting a “person with a gun or weapon” or “domestic violence involving a weapon.” Additionally, to provide a comprehensive understanding of how police shootings affect reporting of incidents beyond gunshot crimes, we also assess their impact on overall 911 calls for service, categorized by call priority level.

Finally, we use the officer-initiated calls, in addition to arrests data, in order to examine whether police activity changes in treated blocks after a shooting.<sup>3</sup> We collapse the data at the month and block level and focus on two outcomes in our main analysis: monthly ShotSpotter detected gunshots, and monthly gunshots reported through 911 calls. Our outcome variables show significant variation across months and blocks, with many blocks experiencing zero ShotSpotter incidents per month. To reduce the variance while still incorporating the zeros, we perform inverse hyperbolic sine transformations of the monthly number of ShotSpotter incidents and shots reported.<sup>4</sup>

Summary statistics in Table 1 show that, on average, there are 0.15 ShotSpotter incidents in a given block-by-month (column 1), and shooting crimes are more likely to occur in treated blocks, although the difference across the treated and control blocks is not statistically significant. On average, there are almost 0.13 shots reported in a block per month. To compute the reporting rate of gun shots, we divide monthly shots reported by monthly ShotSpotter incidents for each block. On average, only 20 percent of monthly gun shots are reported.<sup>5</sup> On average, there are 4 police-initiated calls and 1 arrest in a given block per month.

Lastly, we use data from the 2010 Census to examine heterogeneity across Census blocks (United States Census Bureau 2010). Table 1 shows that on average, treated blocks have a

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<sup>3</sup>Note that the arrests data are only available beginning 2010.

<sup>4</sup>This transformation is of the form:  $asinh(Y) = \ln(Y + \sqrt{1 + Y^2})$ . It is defined at zero and is interpreted similarly to a Log transformation.

<sup>5</sup>This computation is only possible for block-by-month observations with nonzero ShotSpotter incidents. Therefore, conditional on a ShotSpotter incident, civilians only report one out of five shootings. It should also be noted that the reporting rate documented in our study is not substantially different from findings in other settings. Using data from multiple cities, Carr and Doleac 2016 document the low reporting rate of gun violence. For instance, only 12 percent of gunshots in Oakland, CA and Washington, DC result in a 911 call.

higher percentage of Black population compared to non-treated ones (37 percent compared to 27 percent), and that the share of Hispanics is relatively low across blocks (7 percent in the entire sample).

## 2.2 Police-involved shootings

In order to identify treated blocks, we rely on administrative data of police-involved shootings that occurred between 2009 and 2019 in Minneapolis, obtained from the city’s open data webpage (Minneapolis Open Data 2020). Broadly, a police-involved shooting refers to a situation in which a police officer fires their weapon towards another person or an object (National Policing Institute 2024). In Minneapolis, police-involved shootings include incidents where an officer discharges their weapon in the direction of another person, whether fatal or not.

The data include information about the date and the time of each incident, location (latitude and longitude), the officer’s demographic characteristics, and the subject’s demographic characteristics. In addition, the data show the weapon used by the subject, if any.<sup>6</sup> In total, there were 57 unique police-involved shootings between 2009 and 2019.

We report summary statistics for the full sample of police-involved shootings in Table A2. Column (1) shows that victims of police shootings are less likely to be female (11 percent). Only six out of the 57 shootings involve a White victim. In line with national statistics, the majority of civilians involved are Black (72 percent).

One concern with police records is that police violence is often underreported (Collaborators et al. 2021), especially with respect to fatal incidents. To address this concern, we supplement our main dataset with data from Fatal Encounters (Fatal Encounters Database 2022), which serve two purposes. First, we match the incidents in the Fatal Encounters data to determine whether any fatal shootings are missing from the data provided by the Minneapolis Police Department. Through this process, we identify only one missing fatal

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<sup>6</sup>Information about the subject’s weapon is missing for 16 shootings.

shooting. Second, information on whether the shooting was fatal or not is missing for 26 percent of the shootings in our sample. Using Fatal Encounters, we identify any of the shootings reported by the police department as fatal if they appear in the Fatal Encounters dataset, and nonfatal otherwise. Within our sample period, 16 percent of the shootings were fatal.

An important characteristic of the police shootings in our sample is that they are not highly publicized. We provide descriptive evidence of that by using two main data sources: news coverage from LexisNexis (LexisNexis 2023), a publicly available archive of news articles, and Google Trends (Google Trends 2023). Using LexisNexis, we compute the number of news articles that mention the phrases “police shot”, “police shooting”, or “police involved shooting” in Minnesota over time. Similarly, we create a time series dataset of the number of articles that mention “George Floyd” over the same sample period.<sup>7</sup> Using Google Trends, we conduct two different exercises: first, we retrieve the search volume for the phrase “police shooting” relative to that for “George Floyd”. Second, we retrieve the search volume for the names of the victims in fatal police shootings relative to that for “George Floyd”.

We plot these data over time to compare the level of public attention garnered by police shootings relative to that of George Floyd, and we mark the dates of the actual shootings on the graphs (Figure A2). As can be seen from these graphs, the increase in news coverage, measured by both the number of articles mentioning “George Floyd” and the search volume for “George Floyd” after the incident, far exceeds the news coverage and search volume of police shootings after these incidents in Minneapolis. The same trend applies to the Google search volume of victims’ names after fatal police shootings (panel c).

## 2.3 Exposure to police-involved shootings

To identify treated blocks, we use spatial analysis in ArcMap, where we plot Census blocks and police shootings using their precise coordinates, and we identify the blocks that are

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<sup>7</sup>The data are available at the state-level only.

within “ $r$ ” miles from a police shooting as treated.<sup>8</sup> We use five different radii to define treatment, ranging from 0.1 to 0.5 miles, and later report the results using all the five definitions of treatment as explained in Section 3.

In Figure 1, we illustrate the geographical location of police-involved shootings by year of occurrence across the “in-sample” blocks in a neighborhood of the city (for a complete map, see Figure A1). As indicated on the map, some blocks are in close proximity to multiple police-involved shootings during the sample period. Generally, shootings in the city may occur within a few months from each other. As we show in Figure 1 panel (a), there are at least two police-involved shootings every year in the entire city. Depending on the radius used to define treatment, the percentage of treated blocks that experience more than one police shooting throughout the *entire* sample period varies between 13 percent and 51 percent.<sup>9</sup> In our analysis, we focus on the first shooting to determine the treatment date for each treated block. We expand on the incidence of multiple police shootings in section 4.

Moreover, some blocks are exposed to police shootings that happen as early as 2009 or as late as 2019, which are the first and last years in our data, respectively. This implies that for these blocks, we observe very few, if any, pre- or post-periods, depending on the date of the police shooting. Hence, we restrict the sample of shootings to those that occurred between 2010 and 2018. That is, blocks that were treated before 2010 or after 2018 are completely excluded from our sample. This allows us to observe at least one year of pre- and post-periods for each block in our sample. More importantly, this approach eliminates always-treated blocks, which are considered one of the “forbidden comparison groups” (Roth et al. 2023).

Defining treatment based on the first police shooting and restricting the sample of shootings to those occurring between 2010 and 2018 impacts the number of “effective” shootings

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<sup>8</sup>We use the “Select by Location” tool to select Census blocks that are within “ $r$ ” miles from a shooting. This tool creates buffers using the buffer distance (“ $r$ ” miles) around the shootings and returns all the Census blocks that intersect the buffer zones.

<sup>9</sup>Naturally, as the radius increases, the percentage of blocks experiencing multiple police shootings also increases. Specifically, the percentage of treated blocks that experience more than one police shooting is 13%, 18%, 29%, 42%, 51%, at radii of 0.1 miles, 0.2 miles, 0.3 miles, 0.4 miles, and 0.5 miles, respectively.

used to define treatment, depending on the radius chosen. As a result, not all 57 police-involved shootings that occurred between 2009 and 2019 are included in the main analysis. The first shooting restriction identifies the earliest treatment experienced by any given block between 2009 and 2019. Since treatment is determined based on being within “ $r$ ” miles from a shooting, the number of effective shootings following this restriction varies by radius. Consequently, the number of shootings will also vary after the second restriction which further excludes 2009 and 2019 shootings.

To illustrate, consider a simplified example where three independent police-involved shootings occurred in Minneapolis in 2009, 2013, and 2015. Suppose Minneapolis includes only two blocks, X and Y. Block X is within 0.1 miles of the 2013 shooting, while Block Y is within 0.1 miles of the 2015 shooting. When treatment is defined as being within 0.1 miles from a shooting, Block X is considered treated in 2013, and Block Y is considered treated in 2015. After the first restriction, which entails focusing on the first shooting, both the 2013 and 2015 shootings are included, since both blocks are only exposed to one shooting. After the second restriction, we are still left with *two* effective shootings, as none occurred in 2009 or 2019.

Now, suppose Block X is within 0.2 miles of two shootings, the 2013 and 2009 shootings, but it is more than 0.2 miles away from the 2015 shooting. Block Y is within 0.2 miles of the 2015 and the 2013 shootings, but more than 0.2 miles away from the 2009 shooting. Once we define the treatment at 0.2 miles, both blocks are now exposed to more than one shooting as the radius is larger. Specifically, Block X is considered exposed to the 2013 and the 2009 shootings, and Block Y is exposed to the 2015 and the 2013 shootings. When we impose the first restriction, we focus on the first shooting for each block to define treatment. Consequently, Block X is considered treated in 2009, and the 2013 shooting is disregarded. Yet, Block Y is treated by the 2013 shooting, while the 2015 shooting is disregarded. In this case, we are left with two shootings, the 2009 and the 2013 shootings. Finally, we impose the second restriction, which excludes shootings that occur in 2009 and 2019, so our final

sample consists of only *one* shooting, the 2013 one.

We formally present the number of effective shootings after these two restrictions across radii in Table A1. The first row indicates the number of effective shootings after using the first for each treated block. For example, after this restriction, the number of effective shootings included in the analysis using the 0.1 miles radius to define treatment is 31 (column 1). Subsequently, we drop the shootings that occurred in 2009 and 2019 as explained above, along with the associated treated blocks (since we would not want to include the blocks that are treated in 2009 and 2019 in the control group). Again, this further reduces the number of effective shootings at each radius, as demonstrated in the second row of Table A1.

In Table A2, we report the summary statistics for the full sample of police-involved shootings that occurred between 2009 and 2019 (column 1), in addition to the summary statistics for the shootings that are included in the analysis at each radius (columns 2-6). We also test for statistical differences between the sample of excluded police shootings and the effective (included) shootings at each radius using a t-test, and we report the p-value for each independent characteristic in square brackets. As shown in the table, the effective shootings at each radius are representative of the full sample. For instance, at the 0.1 miles radius (column 2), there are 23 effective police shootings, with the majority involving a Black civilian (76 percent) and being non-fatal. Furthermore, the p-values show that the characteristics of the effective shootings are not statistically different from those of the excluded shootings at each radius, except that the included shootings are less likely to target a White civilian, probably because such events are rare within the full sample of shootings.

### 3 Empirical Strategy

Estimating the causal effect of exposure to police shootings is difficult given that their occurrence is nonrandom. Police shootings are more likely to occur in blocks that have higher crime rates, are more hostile towards law enforcement agencies, and/or are socioeconomically

disadvantaged. To overcome this, we exploit the variation in the timing and the distance to police-involved shootings to estimate the effect of exposure to these events using a differences-in-differences approach.

As previously mentioned, we define exposure by the distance from a police-involved shooting. Theoretically, the distance at which the effect of a police shooting dissipates is unclear. There is no consensus about the distance at which the effect of exposure to violence fades out. Therefore, we use five different radii to define treatment, starting with 0.1 miles and incrementally increasing to 0.5 miles.<sup>10</sup>

This empirical strategy relies on comparing blocks that fall within “ $r$ ” miles of a police-involved shooting to those located more than “ $r$ ” miles away, before and after such an incident. Since we focus on less publicized police shootings, this strategy also relies on the assumption that Census blocks that are beyond “ $r$ ” miles from police shootings are not treated. If this assumption is not met, it does not pose a threat to the validity of our results, but it does lead our estimates to be attenuated.

To avoid attenuation bias, we use a set of common control blocks for all five radii. These are Census blocks that are more than 0.5 miles away from any shooting, i.e. blocks that are never treated, regardless of the radius used. For example, when using the 0.1 miles radius, we consider blocks within 0.1 miles of a shooting as treated, and those more than 0.5 miles away as the control group. Blocks between 0.1 miles and 0.5 miles of a shooting are completely excluded from the analysis, as they are the potentially “contaminated” blocks.<sup>11</sup>

Formally, we estimate the following model:

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<sup>10</sup>Studying the effect of violent crime on outcomes of public schools in Chicago, Casey et al. 2018 argue that the effect of local crime exposure dies out at a radius beyond 0.3 miles. On the other hand, Ang 2021 shows that the effect of police killings on student outcomes dissipates beyond 0.5 miles.

<sup>11</sup>Our approach allows the sample size to vary across radii for two reasons. First, as the treatment radius increases, the number of treated blocks also increases. Since we use a common control group for all five radii (i.e., the number of control blocks remains fixed across the different radius specifications), widening the radius also increases the sample size. Second, this expansion implies that a larger number of blocks are treated by 2009 or 2019 shootings at larger radii. Since we exclude shootings that occurred in 2009 and 2019, along with the blocks exposed to them, as detailed in subsection 2.3, this restriction also impacts the sample size (number of observations) differently across radii.

$$Y_{bt} = \beta_0 + \beta_1 * Treat_b \times Post_t + Month \times Year_t + Block_b + u_{bt} \quad (1)$$

where  $Y_{bt}$  is the inverse hyperbolic transformation of shots reported or ShotSpotter incidents in block  $b$  at month  $t$ .  $Treat_b$  is a dummy variable that indicates whether block  $b$  is treated.  $Treat_b \times Post_t$  is the treatment variable that takes the value one for treated blocks in the months following their exposure to a police shooting. The coefficient  $\beta_1$  measures the change in ShotSpotter and shots reported after a police shooting in exposed blocks, relative to that change in unexposed blocks. In all specifications, we include month-by-year and block fixed effects. We cluster the standard errors at the Census tract level to account for potential error correlations among geographically close blocks (Cameron and Miller 2015).

The plausibility of our empirical strategy relies on the parallel trends assumption. That is, the treated and the control blocks would have exhibited similar trends in the outcomes if the former were not exposed to police shootings. To examine the validity of this assumption, we estimate the following dynamic difference-in-differences model:

$$Y_{bt} = \alpha_0 + \sum_{\substack{\bar{j} \\ k=-\bar{j} \\ k \neq -1}} \gamma_k Treat_b * 1\{t - \tau = k\} + Month \times Year_t + Block_b + \epsilon_{bt} \quad (2)$$

where we replace our post exposure indicator ( $Post_t$ ) with a series of “event time” indicators spanning at most 118 months before and 116 months after a block experiences a police shooting at time  $\tau$ .<sup>12</sup> Including block and month-by-year fixed effects, the coefficients  $\gamma_k$  represent the effect over one-month bins.<sup>13</sup> We graph the estimated coefficients over time to examine the pre-trends. If our empirical strategy is valid, we expect to see no divergence in the pre-trends across treated and control blocks.

Our two-way fixed effects estimate from Equation 1,  $\beta_1$ , represents the weighted average of all possible 2\*2 comparisons between treated and control blocks in the sample. Since police

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<sup>12</sup>Since the treatment is staggered, the number of pre- and post-periods varies for each block depending on the timing of treatment. In our event study graphs, we plot 6 periods before and after treatment, with the last period including all months beyond 6 months on each side of the cutoff.

<sup>13</sup>We exclude the month right before the shooting happens ( $k = -1$ ) from the analysis.



shootings occur at different times, some of the 2\*2 comparisons comprise of using already-treated blocks as control for the later-treated blocks. According to recent literature (Roth et al. 2023; Goodman-Bacon 2021; De Chaisemartin and d’Haultfoeuille 2020; Sun and Abraham 2021), the two-way fixed effects model imposes the assumption that the treatment effect is constant over time and across groups.

However, we cannot rule out the presence of heterogeneous treatment effects in our setting. For example, the effect of a police shooting in an area where civilians mistrust the police might be smaller relative to the effect of a similar incident in other areas. Moreover, police shootings might be different in their nature. For instance, civilians might be more sympathetic towards an unarmed victim of a police shooting relative to an armed one. At the same time, the change in public attention to police violence over time as well as increased exposure to social media could introduce heterogeneity. We address this concern by utilizing the Callaway and Sant’Anna 2021 and the Sun and Abraham 2021 estimation methods. We report these results in the Appendix.

Another possible threat is a change in the composition and nature of shooting crimes before and after a police shooting. To illustrate this concern, consider the following example. Suppose there are two types of gunfire incidents: those that are always reported, such as incidents that result in an injury, and those that are never reported. If an increase in gunfire is caused by an increase in the latter, an estimated decrease in the reporting rate would be due to changes in the composition of incidents rather than a change in reporting behavior by civilians. To address this issue, we test for any change in the observed characteristics of ShotSpotter incidents before and after a police shooting. Specifically, we examine whether shootings are occurring at similar days of the week and at similar times during the day, before and after a police-involved shooting.<sup>14</sup>

Using the main generalized difference-in-differences equation, we estimate the effect of a police shooting on the day and the time of ShotSpotter incidents. The results are presented

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<sup>14</sup>There is evidence that violent crimes such as murder, assault and robbery are more likely to happen at night (Doleac and Sanders 2015).

in Table 2. In panel A, the outcome is Daytime, a dummy variable that takes the value 1 if the incident happens between 6 am and 6 pm, while in panel B, the outcome is weekend, a dummy variable that takes the value 1 if it happens on Saturday or Sunday. For all five specifications, we find no significant effect of exposure to a police shooting on any of these characteristics. As a result, to the extent that the timing of a shooting is a good proxy for other characteristics, this suggests that our results are not driven by a change in the composition of shootings.

In the next section, we explain how we estimate the effect on the reporting rate using the results from equation 1.

### 3.1 Interpretation

As previously explained, shots reported through 911 calls are only a fraction of the total gunshots occurring in a certain geography. We can write the number of shots reported (SR) as a function of ShotSpotter incidents (SS) and the willingness to report (WTR) as:

$$SR_{bt} = WTR_{bt} \times SS_{bt} \quad (3)$$

In our analysis, we do not directly estimate the effect of police shootings on the reporting rate, which is computed by dividing the number of shots reported by the number of gun crimes, since it can only be observed when the latter is different than zero. To avoid selection bias arising from conditioning on an endogenous variable, we estimate the effect on both outcomes separately. Next, we formally derive crime reporting in terms of crime incidence and the propensity to report, following Jácome 2022. As derived in Appendix A.1, we write the change in the reporting rate,  $\alpha$ , as  $\beta^{SR} - \beta^{SS}$ , and we formally test if this difference is statistically different than zero using a simple linear hypothesis test with the following null hypothesis:

$$H_0 : \beta^{SR} - \beta^{SS} = 0 \tag{4}$$

where  $\beta^{SS}$  is the effect of a police shooting on ShotSpotter incidents and  $\beta^{SR}$  is the effect of a police shooting on shots reported.

## 4 Results

We begin by plotting our two main outcome variables over time for different groups of blocks to illustrate the comparisons made in our empirical approach. We divide the sample into four main groups: treated blocks, never-treated blocks, later-treated blocks, and early-treated blocks. Treated blocks are those exposed to a police shooting at any time during our sample period. Never-treated blocks denotes blocks that never experience a police-shooting throughout the sample period; these serve as the “common control blocks” group that we use as a comparison group in all five specifications. “Later-treated” blocks experienced a police shooting after 2015 and serve as a comparison group for the “early-treated” blocks, i.e. blocks that were exposed to a police shooting before 2015.

For each of these four groups, we plot the average number of ShotSpotter incidents and shots reported over time. In all the figures, the x-axis is normalized relative to the treatment time, where the vertical line at 0 marks the time of treatment, and each time period is one month long. When plotting the data for the control blocks (never-treated and later-treated blocks), we first normalize the time periods (in this case, months of the year) around each treatment date separately and for each block.<sup>15</sup> Then, we stack the normalized periods and compute the average of our outcome variables across control blocks for each period.

We present the figures using the 0.4 miles radius to define treatment, with additional figures for all other radii reported in subsection A.2 (Figure A3, Figure A4, Figure A5, and

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<sup>15</sup>To create the normalized time periods for the never-treated blocks, the treatment dates are the dates of all the police-involved shootings in our sample period. To create the normalized time periods for the later-treated blocks, the treatment dates are the dates of the police-involved shootings that occurred before 2015.

Figure A6). As can be seen in Figure 2, treated blocks exhibit an immediate increase in the average number of ShotSpotter incidents after a police shooting, while shots reported show a slight decrease (panel a). In contrast, the average number of shots reported and ShotSpotter incidents after a police shooting in never-treated blocks remain unchanged (panel b). We also observe an increase in ShotSpotter incidents along with a slight drop in shots reported in the “early-treated” blocks after a police shooting (panel c), whereas both outcomes remain unchanged in “later-treated” blocks following a police shooting (panel d).<sup>16</sup>

These figures reveal three takeaways. First, they show that both outcome variables vary steadily in exposed blocks before the treatment date, which suggests that the timing of the police shootings is not driven by changes in crime rates in treated blocks and is indeed random. Second, after a police shooting, there are more ShotSpotter activations in treated blocks. Third, the change in shots reported following a police shooting does not mirror the increase in ShotSpotter incidents, which suggests a decrease in the reporting rate. Next, we turn to estimating the dynamic differences-in-differences model (Equation 2).

## 4.1 Event-study results

We plot the results of equation 2 to examine the dynamic effects of a police shooting on exposed blocks for the five different specifications. Each panel represents a radius and displays the estimated coefficients for both variables over time. We use one-month bins to visually assess the short-term changes in both outcomes. We omit the first pre-period in all estimations ( $k=-1$ ).

First, all five panels show that there is no evidence of pre-trends for both outcomes (Figure 3). This provides comfort that the two groups would also be unlikely to diverge post-treatment, except due to exposure to a shooting, supporting the parallel trends assumption. Second, the figures show an immediate increase in ShotSpotter incidents after a police shooting, particularly using the 0.3-0.5 miles radii. This increase persists over the

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<sup>16</sup>Note that the number of shots reported can be higher than the number of ShotSpotter incidents in a given time period, possibly due to multiple individuals reporting the same incident.

first five months after exposure. In contrast, the number of shots reported slightly decreases over time or remains unchanged.

## 4.2 Difference-in-differences results

In this section, we present our difference-in-differences estimates. Unless specified otherwise, we use a common control group for all the estimations. We also control for month-by-year fixed effects and block fixed effects, and we cluster the standard errors at the Census tract level to account for any correlations across adjacent blocks.

Our primary results are presented in Table 3 for all five radii. Panels A and B of Table 3 show the effect of a police shooting on shots reported and ShotSpotter incidents, respectively. If police shootings have no impact on civilian trust as proxied by crime reporting, we would expect to see effects of similar magnitude across both panels. For each radius, we calculate the difference between the effect on shots reported and ShotSpotter incidents to estimate the effect on the reporting rate following subsection 3.1.

Panel (A) indicates that a police shooting leads to a small decrease in the number of shots reported, ranging from 1 to 2 percent, although the results are noisy depending on the radius used. In contrast, blocks exposed to a police shooting experience an increase in ShotSpotter incidents that ranges from 5 to 6 percent and is highly statistically significant across all radii. For instance, in column 4, blocks within 0.4 miles of a police shooting experience a 1 percent decrease in shots reported ( $p < 0.05$ ) alongside a 6 percent increase in ShotSpotter incidents ( $p < 0.01$ ).

As we show in Appendix A.1, the effect on shots reported represents a lower bound for the effect on the reporting rate, or the propensity of civilians to report gunshots. We report the difference in the effect on shots reported and ShotSpotter incidents at the bottom of Table 3, which represents the true effect on the reporting rate. Across all five radii, the difference between the estimates in Panel A and Panel B is negative. Importantly, the estimated differences are statistically significant at conventional levels across all radii.

For example, the estimate at the 0.4 miles radius indicates that a police shooting causes a 7 percent reduction in the reporting rate, which is significant at the 1 percent level ( $p\text{-value} = 0.000552$ ).<sup>17</sup> Overall, our first set of results show that after a police shooting, ShotSpotter activation incidents increase by 5-6 percent and the reporting rate decreases by 6-7 percent in exposed blocks relative to unexposed blocks.<sup>18</sup> Importantly, Figure 3 depicts a consistent positive effect of shootings on ShotSpotter incidents, suggesting that our difference-in-difference estimates are not solely driven by specific time periods.

In Table A4, we demonstrate the robustness of our results to using control groups that vary with the radius used. For instance, blocks located within a 0.4 miles radius from a police shooting experience a 6 percent increase in ShotSpotter incidents and a 6 percent decrease in the reporting rate, relative to blocks that are more than 0.4 miles away from a police shooting (column 4). Unsurprisingly, the estimates are less precise using smaller radii, potentially due to the spillover effects of a police shooting in nearby control blocks.

It is possible that the description of gunshots by civilians could have changed following a police shooting, especially if for example, gunshots become more frequently associated with other types of crimes like domestic violence. This could lead to a compositional change in the reported 911 calls, which might not be captured by our shots reported measure. To alleviate this concern, we use a broader definition of shots reported to include incidents that do not explicitly mention “shots fired” but do indicate the involvement of a weapon, even when the weapon is not limited to guns. As shown in panel A of Table A6, the effect of a police shooting on the broader measure of shots reported aligns to a large extent with the results in Table 3 in terms of magnitude and significance, which shows that the results are not sensitive to how we define the outcome variable.

We also examine the reporting of incidents beyond gunshot crimes by estimating the

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<sup>17</sup>To put this in a better context, we calculate the average reporting rate for the exposed blocks before exposure to be 22 percent. This means on average, for every 100 gunshots, 22 of them were being reported by civilians. A 7 percent decrease in the reporting rate implies that the reporting rate becomes 20 percent, which means 2 more gunshots go unreported (for every 100 gunshots, 20 are reported compared to 22).

<sup>18</sup>Results remain robust when clustering at the Census block level, as shown in Table A3.

effects on the overall volume of 911 calls for service, distinguishing between low and high priority calls. Low priority calls typically include matters which individuals may refrain from contacting the police about, such as business checks, noise disturbances, theft, or reports of unwanted/suspicious persons. In contrast, high priority calls typically pertain to more serious incidents like assault, stabbing, business robberies, or burglary. Table A6 shows that a police shooting does not affect the overall volume of 911 calls or the volume of low priority calls. An exception is the effect on low priority calls using the 0.1 miles radius, which is significant at the 5 percent level. Furthermore, there are no discernible effects of a police shooting on the volume of high priority calls using the 0.1 and 0.5 miles radii, although a marginally significant negative effect is observed using the 0.2-0.4 miles radii. These findings are consistent with our main results on shots reported. Nevertheless, interpreting these results – in the absence of a measure of crime incidence similar to that provided by the ShotSpotter data – is less straight forward, since these outcomes are a function of both, crime incidence and crime reporting.

Overall, the results in this section indicate that after a police shooting, ShotSpotter incidents increase by 5-6 percent, while the number of shots reported decreases by 1-2 percent. Taken together, this suggests that a police shooting causes a 6-7 percent decrease in the reporting rate. Additionally, these effects are highly local, as shown by their consistency in blocks situated between 0.1 and 0.5 miles from a police shooting, which is in line with Ang 2021.

### **4.3 Alternative estimators**

As previously discussed, recent literature suggests that the two-way fixed effects model may be biased in the presence of staggered treatment timing and heterogeneous treatment effects (Goodman-Bacon 2021; De Chaisemartin and d’Haultfoeuille 2020; Sun and Abraham 2021; Callaway and Sant’Anna 2021).

To overcome any bias arising from heterogeneous effects in our setting, we present

estimates of average treatment effects on the treated using methods developed by Callaway and Sant’Anna 2021 and Sun and Abraham 2021. These approaches relax assumptions regarding the homogeneity of the effects and are more transparent about the comparison groups used. Specifically, both Callaway and Sant’Anna 2021 and Sun and Abraham 2021 exclude always-treated units from the analysis and avoid using already-treated units as controls for future-treated ones. In all specifications, we include month-by-year and block fixed effects and cluster the standard errors at the Census tract level.

We first estimate the effect of exposure to a police shooting on our main outcomes of interest using Callaway and Sant’Anna 2021. Here, we specify that the control group comprises only never-treated blocks, which essentially are the common control blocks used in our main analysis.<sup>19</sup> We have a sizeable number of never-treated blocks that is sufficient to estimate the model without having to use “later-treated” blocks as a control group (Callaway and Sant’Anna 2021).<sup>20</sup>

The plotted estimates are provided in Appendix A.2. Figure A7 and Figure A8 show the estimated coefficients of the effect of police shootings and the 95% confidence intervals for each month before and after treatment. Consistent with our previous findings, we observe no evidence of diverging pre-trends for shots reported across both groups. Additionally, we see a slight decrease in the number of shots reported after exposure, though not statistically different from zero (Figure A7). As for ShotSpotter incidents, we again detect no evidence of pre-trends across both groups, but we observe an increase in these incidents after a police shooting. This aligns with our results from subsection 4.1.

We report the aggregation of all post-treatment effects – the overall ATT for all groups across all time periods – for each radius in Table A7. These results are consistent with our main findings and are larger in magnitude. Specifically, we estimate an 8 to 11 percent

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<sup>19</sup>In addition to using the never-treated group as a comparison group, we use the improved doubly robust estimator based on inverse probability of tilting and weighted least squares. We also specify the option “long2”, which requires the estimation of long gaps rather than short gaps in the event-studies.

<sup>20</sup>The results are robust when using the not-yet treated group as the control group. These results are available upon request.



increase in ShotSpotter incidents in exposed blocks following a police shooting with a negative but statistically insignificant effect on shots reported. This translates into a 9-13 percent decrease in the reporting rate, that is statistically significant for four out of five radii.

In a second check, we use the Sun and Abraham 2021 estimation method, which relies on the last-treated blocks as the control group. Given that we focus on shootings that occurred between 2010 and 2018 (subsection 2.3), the group of last-treated blocks consists of those treated in 2018. Table A8 reports the results and shows an increase in gun violence in exposed blocks relative to control blocks following a police shooting. Except when using the 0.1 mile radius, a police shooting leads to a 4 to 7 percent decrease in the reporting rate.

Overall, our results remain valid when we account for the potential biases in the two-way fixed effect estimator. The findings obtained using alternative estimators are similar to our main results, suggesting that the latter are robust to heterogeneous treatment effects.

## 4.4 Long-run effects

Our findings suggest that following a police-involved shooting, treated blocks experience an increase in gunshot crimes and a decrease in the reporting rate, which persist for at least 6 months after exposure (Figure 3). Understanding how persistent these effects are is important from a public safety perspective. Are neighborhoods exposed to police violence permanently less safe? To investigate the persistence of these effects beyond the 6-month window, we estimate the impact of a police shooting over five years following exposure. To do so, we focus on shootings that occurred before 2015. Using the 0.4 miles radius to define treatment, this leaves us with 17 unique police-involved shootings. Importantly, we exclude blocks that were treated during or after 2015 from this analysis.

We then estimate the dynamic difference-in-differences effects using Equation 2, and we report the results in Figure 4. As seen in panel (a), there is an immediate and statistically significant increase in ShotSpotter incidents following a police shooting, and this increase persists for up to five years after exposure. Conversely, panel (b) indicates no significant

change in shots reported following exposure to a police shooting. As indicated on the figures, the overall (difference-in-difference) effect is an 8 percent increase and a 1 percent decrease in ShotSpotter incidents and shots reported after a police shooting, respectively.

To quantify the dynamic changes in the outcomes of interest over time, we estimate the difference-in-differences model using 6-month intervals after treatment. The effect of a police shooting on ShotSpotter incidents remains consistently positive and statistically significant throughout the post-treatment period. Conversely, we find a negative and statistically significant effect on the number of shots reported within 6 months from a police shooting, but the effect fades out beyond that (Table A9). These two sets of results indicate that both the increase in gun-related crimes and the decrease in reporting rate persist even beyond a two-year period following a police shooting.

There are two possible explanations for these persistent effects. First, treated blocks may be exposed to subsequent shootings following their first exposure, as almost 50% of the treated blocks in this analysis experience at least one future police shooting. Second, the persistent increase in ShotSpotter incidents could also be due to the systematic increase in the installation of ShotSpotter devices in exposed blocks, which could lead to a mechanical increase in the number of ShotSpotter incidents detected. Unfortunately, we were not able to obtain information about where and when these devices were installed across the city. While our short-run results in subsection 4.1 are less sensitive to these concerns (as we elaborate in section 5), we cannot rule out the possibility that the persistence of these effects in the long-run might be driven by a systematic increase in ShotSpotter devices installed in treated blocks compared to the comparison blocks.

## 4.5 Repeated exposure

As mentioned in section 2, some treated blocks are exposed to more than one police-involved shooting during the sample period, particularly as we expand the radius that defines treatment. A question remains as to whether the estimated effects are driven by repeated expo-

tures.

First, while some blocks are exposed to multiple police shootings, the likelihood that a block is treated again within a year of the first shooting is very low. For instance, when using the 0.4 miles radius to define treatment, none of the treated blocks are exposed to a subsequent police shooting within 6 months of the first shooting, and less than 1 percent of treated blocks are treated within one year of the first shooting.<sup>21</sup> Conditional on experiencing multiple police shootings, the average time between the first and the second police shootings is 3 years, irrespective of the radius used. Given that our event study graphs (Figure 3) show effects within 0-6 months of a shooting, we are confident that in the short run, our main results are picking up the effect of a single police shooting.

To further confirm that our results capture the effect of a single shooting, we estimate a difference-in-differences model while accounting for subsequent shootings in the post period. Specifically, we estimate our main difference-in-differences equation, and we control for the period after a subsequent police shooting for blocks that are treated again. As we show in Table A5, after the first police shooting and before any subsequent shootings, treated blocks experience a 1-2 decrease in shots reported relative to unexposed blocks, while ShotSpotter incidents increase by 5-6 percent depending on the radius used.<sup>22</sup> These results confirm that—at least in the short-run — the estimates reflect the effect of a single police shooting, consistent with the fact that a very small percentage of blocks experience subsequent shootings within a year of the first shooting.

Additionally, these results demonstrate subsequent shootings have similar and even larger effects. For instance, at the 0.4 miles radius, after a second police shooting, ShotSpotter incidents increase by an additional 8 percent in treated blocks relative to untreated blocks, while shots reported remain unchanged. Given that the likelihood of a subsequent shooting increases in the long-run, exposure to future police shootings might partially explain the

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<sup>21</sup>The percentage of treated blocks experiencing a subsequent shooting within a year of the first one is 1%, 1%, 4%, 5%, and 10%, at the 0.1, 0.2, 0.3, 0.4, and 0.5 miles radius, respectively.

<sup>22</sup>The results hold for 4 out of 5 radii. The effects at the 0.5 miles radius are noisy, but they still show an increase in ShotSpotter incidents and a decrease in shots reported.

persistence of the effects found in the long-run analysis of subsection 4.4.

## 5 Robustness Checks

In this section, we examine the robustness of our results to various changes such as manipulating the length of the pre- and post-periods, controlling for time trends across groups, changing the samples used in the analysis, and using different transformations of the outcome variables. All of the results reported in this section use the 0.4 miles radius.

As discussed in section 3, we restrict our shootings to those that occurred between 2010 and 2018. This approach aims to prevent the inclusion of always-treated blocks and ensure that all blocks in the sample have at least one year of pre- and post-periods. However, there is no consensus regarding the optimal length of pre- or post periods. Importantly, excluding some shootings may be problematic if the included ones are different in nature from those excluded. For instance, it is possible that the shootings that occurred in 2019 have a greater impact on civilians compared to the included ones.

However, as we show in Table A2, the police shootings that are included in the analysis at each radius, which all exclude the 2009-2019 shootings, are not different in terms of observable characteristics from the excluded police shootings. We take a step further, and we demonstrate that the estimates for both outcomes are not sensitive to the length of the pre- and post periods, nor the number of shootings included in the sample. Specifically, we use seven different lengths of the pre- and post-periods, starting with 1 year on each side of the cutoff, which we use in our main specification. We then incrementally increase the length of the pre- and post periods by 6 months on each side of the cutoff until reaching four years.<sup>23</sup>

We estimate Equation 1 for each length and outcome, separately, and present the difference-in-differences coefficients with their 95% confidence intervals in Figure 5. As shown

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<sup>23</sup>The number of shootings included in each specification is as follows: 24, 22, 21, 21, 16, 13, and 9 for the 1, 1.5, 2, 2.5, 3, 3.5 and 4-year long pre- and post-periods, respectively.

in the figure, the effects of a police shooting on ShotSpotter incidents are robust across different lengths of pre- and post-periods. Specifically, the magnitude of the coefficients varies between 5 percent and 10 percent, but the point estimates are not statistically distinguishable from each other. The coefficients for shots reported are either statistically insignificant or marginally negative, which is comparable to the main results in Table 3. Overall, these results demonstrate that our findings are not sensitive to the selection of police shootings included in the analysis.

Another potential threat to our identification is the differential time trends across treatment and control blocks. To account for differential time trends at finer geographies, we include tract-by-month fixed effects. Column (1) of Table A10 shows that our results remain unchanged in magnitude and significance. Specifically, we estimate a 6 percent increase and a 7 percent decrease in ShotSpotter and the reporting rate, respectively. Both estimates are significant at the 1 percent level.

As previously discussed, our in-sample blocks are blocks that experienced at least one ShotSpotter activation between 2007 and 2009 to ensure that ShotSpotter devices were already installed in these blocks throughout the entire sample period. However, it is still possible that the police department installs ShotSpotter devices differentially across areas. For instance, they might install more ShotSpotter devices in areas that are exposed to police shootings and/or that experience higher levels of crime. If this occurs after a police shooting, then the increase in ShotSpotter incidents that we observe would be due to an increase in the number of devices in treated areas rather than a true increase in gun-related crimes. Although we do not have information on the exact date and time of the department's installation of these devices, we argue that it is unlikely that the effects are driven by the installation of new devices, due to the immediate changes in ShotSpotter incidents after a police shooting as seen in the event study graphs (Figure 3, Figure 4). Particularly for radii greater than or equal to 0.3 miles, ShotSpotter incidents increase in the first month after  $t=0$ .

As an additional check, we estimate the short-run effects of a police shooting by limiting our post period to two months after a shooting. This relies on the assumption that it takes the city more than two months to approve the police department’s request to increase coverage, amend the contract with the ShotSpotter company, and install the devices. Column (2) of Table A10 shows that within two months of a police shooting, exposed blocks experience a 3 percent increase in ShotSpotter incidents and a 1 percent decrease in shots reported, although the latter is not statistically significant. Taken together, this implies that a police shooting causes a 4 percent decrease in the reporting rate in the short run. These findings indicate that, in the short-run, the increase in ShotSpotter incidents is not a result of the installation of devices in treated areas. These results also confirm that in the short-run, when nearly no block is exposed to a subsequent police shooting, we are indeed capturing the effect of a single police shooting.

Kahn-Lang and Lang 2020 argue that for a valid difference-in-differences design, the treatment and control groups should be similar in levels as well as in trends. Although we show that both treatment and control groups exhibit similar levels of ShotSpotter incidents and shots reported in Table 1, we further restrict our sample to “high crime” areas to address concerns regarding the comparability of the treatment and the control groups. These areas are defined as blocks that experienced *more than* one shooting annually in 2009 and 2010.<sup>24</sup> This is also relevant for the concern of installing more ShotSpotter devices alluded to above. If the police department is more likely to install ShotSpotter devices in “high crime” areas, then the effect of that should be equivalent across treatment and control blocks when using this sample.

Column (3) of Table A10 shows that our results remain unchanged when we restrict our sample to “high crime” areas. Specifically, we estimate a statistically significant 8 percent increase in ShotSpotter incidents, and an insignificant effect on shots reported. As before, this implies that the reporting rate decreases by about 9 percent after a police shooting.

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<sup>24</sup>The 50<sup>th</sup> percentile of ShotSpotter incidents between 2009 and 2010 is 1 per Census block.

We further estimate the short-run effects in the “high crime” sample and present the results in column (4). Our results remain consistent. In fact, we estimate the largest increase in ShotSpotter incidents (12 percent), indicating that the reporting rate decreases by 12 percent within two months after a police shooting in “high crime” areas.

Finally, all our analyses use the inverse hyperbolic transformation of the outcome variables. We demonstrate that our results are not sensitive to the choice of different functional forms. Specifically, in Table A11, we show that the results are robust to using a continuous measure of the outcome variables (column 1), the logarithmic transformation,  $\log(y+1)$ , of the outcome variables (column 2), and binary variables equal to one if a given block-by-month had at least one ShotSpotter incident or one shot reported (column 3).

The results in this section demonstrate that our estimates are robust to various sample and treatment variations. Notably, we show that the increase in gun-related crimes occurs within a short period after a police shooting, and the effect is largest in “high crime” blocks.

## 6 Heterogeneous Effects

Using our police-involved shootings dataset, we observe the location of each shooting, whether it was fatal or not, and whether the civilian was armed or not. In this section, we explore the differential effects of a police shooting by type, using the 0.4 miles radius to define treatment. We report the event study graphs for each heterogeneity analysis in the appendix (Figure A9, Figure A10 and Figure A11).

First, we examine heterogeneous effects across different types of neighborhoods. Given that the majority of police-involved shootings in Minneapolis affect the African American community, we ask whether police shootings have a disproportionately higher impact in Minority relative to White neighborhoods. Using 2010 Census data, we define a Census block as White (Minority) if more than 50 percent of its population is White (Minority).<sup>25</sup> We estimate the effect of a police shooting on White vs Minority neighborhoods separately, by

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<sup>25</sup>A block is considered a Minority block if more than 50 percent of its population is Black or Hispanic.

comparing exposed White (Minority) blocks to unexposed White (Minority) blocks. Importantly, both treated and control blocks have the same racial composition in each analysis. That is, when estimating the effect of a police shooting in Minority neighborhoods, both treated and control blocks have more than 50 percent Minority civilians.

We report the results for Minority and White neighborhoods in Table 4, columns (1) and (2), respectively. Furthermore, we report the p-value of the t-test, where we compare the coefficients across both columns. Panel A shows that the effect on shots reported is negative and only statistically significant in the Minority neighborhoods, even though the coefficients across the neighborhoods are not statistically different from each other (p-value = 0.33). However, Panel B shows that there is a 7 percent increase in ShotSpotter incidents in Minority neighborhoods relative to a 1.4 percent insignificant increase in White neighborhoods, and the coefficients are statistically different from each other at the 5 percent level (p-value = 0.0262). Thus, Minority neighborhoods exhibit a decrease in the reporting rate of 9 percent following exposure to police shootings. This can also be seen in the event study graphs (Figure A9), where Minority neighborhoods experience a slight decrease in shots reported (panel a) and an increase in ShotSpotter incidents (panel b) after a police shooting, contrary to the White neighborhoods where both outcomes seem unchanged. This indicates that the main results are entirely driven by Minority neighborhoods.

Second, we divide the sample of police-involved shootings by whether the involved civilian was armed or not. Ex-ante, one might expect that the effect of the police shooting an unarmed civilian might be larger compared to shooting an armed one. This is also shown in Ang 2021, who found that the effect of police killings on students' outcomes is twice as large when the civilian is unarmed. However, our estimates indicate that this is not the case in our setting. Columns (3) and (4) show that the effect is being driven by police-involved shootings targeting an armed civilian.<sup>26</sup> For instance, a police shooting an armed subject

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<sup>26</sup>In specifications of columns (3) to (6), we estimate the effect of a shooting with a certain characteristic by comparing blocks that were exposed to a shooting with that characteristic to blocks that were not exposed to any other shooting.



causes a 6 percent increase in ShotSpotter incidents.<sup>27</sup>

Finally, we test whether fatal shootings have a larger effect relative to nonfatal ones. Surprisingly, fatal shootings have no effect on both ShotSpotter and shots reported (column 5). However, nonfatal shootings cause a 7 percent increase in ShotSpotter incidents and a 1 percent decrease in shots reported, both statistically significant at the 1 percent and the 5 percent level, respectively (column 6). Taken together, this implies that nonfatal shootings cause an 8 percent decrease in the reporting rate as well. As can be seen in Figure A11, both fatal and nonfatal shootings have no effect on shots reported. However, ShotSpotter incidents increase beginning the first month after a nonfatal police shooting, in contrast to fatal police shootings. While one might worry that fatal police incidents are underreported, which could impact the results, we show in section 2 that only one fatal shooting is missing from our dataset, compared to Fatal Encounters, so it is unlikely that underreporting is driving our results. It is important to emphasize, however, that only 3 out of 23 included shootings are fatal, which affects the statistical power of the analysis estimating the effects of fatal shootings.

The results indicate that police shootings have a higher effect on gun-related crimes and the reporting rate in Minority neighborhoods. They also show that shooting an armed civilian and nonfatal shootings have a larger effect when compared to other types of shootings.

## 7 Mechanisms

Our study shows that exposure to police shootings leads to an increase in gun-related crimes and a decrease in the crime reporting rate. While the latter implies a change in civilian trust or willingness to cooperate with the police, there are several potential explanations for the increase in gun violence. One such explanation is the “Ferguson Effect,” where police officers reduce their effort in patrolling or deterring crime to avoid further public scrutiny,

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<sup>27</sup>It is important to note that the information revealing whether the civilian was armed or not is missing for 8 out of the 23 shootings included in the sample. We exclude these shootings along with the blocks exposed to them.

especially following highly publicized incidents (Cheng and Long 2022; Premkumar 2021).

Although our sample of shootings consists of less publicized incidents (Figure A2), we test whether police behavior changes in exposed blocks after a police shooting. We examine the effect of police shootings on police effort, measured by police-initiated calls for service and arrests.<sup>28</sup> In particular, we estimate the difference-in-differences equation 1 using the 0.4 mile radius, where the outcomes are the inverse hyperbolic transformations of the monthly number of police-initiated calls and arrests. We provide the results in Table 5. Columns (1) and (2) show that a police shooting does not have a statistically significant effect on police-initiated calls or arrests, respectively.

Additionally, we plot the dynamic difference-in-differences results using equation 2 for police-initiated calls and arrest using the 0.4 mile radius. Reassuringly, both panels of Figure A12 show no evidence of pre-trends, implying that police activity was uniform across treated and control blocks before treatment. In line with Table 5, there is no change in police calls or arrests in treated blocks relative to control blocks following exposure.

Overall, these results provide evidence that our main findings are not driven by changes in policing practices in treated versus control blocks.

## 8 Discussion

In this paper, we provide causal evidence of the impact of police shootings on gun violence and a measure of civilian cooperation with the police: crime reporting. Using data on gunshots reported through 911 calls and those detected by ShotSpotter in Minneapolis, we employ a difference-in-differences methodology, exploiting the variation in the location and the time of police-involved shootings. Since ShotSpotter data offer an objective measure of gunfire incidents in Minneapolis, we are able to isolate the effect of police-involved shootings on crime incidence from that on crime reporting, overcoming a major hurdle in the criminal justice

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<sup>28</sup>Our data on the universe of 911 calls for service differentiate between officer-initiated and civilian-initiated calls. We obtain the data on the universe of arrests between 2010 and 2019 from the Minneapolis Police Department.

literature. Indeed, our findings demonstrate the significance of this issue, as police shootings are found to be followed by an increase in crime incidence as measured by ShotSpotter.

Moreover, we show that the effects of police shootings are highly localized and persist for multiple years. This suggests that exposed blocks experience increased levels of gun violence for at least four years after a shooting occurs. We also show that the effects are immediate and are largest in “high crime” areas. Importantly, our findings are robust to using different control groups, different lengths of pre- and post-periods, and different sample blocks.

The granularity of our data allows us to explore heterogeneous effects across different neighborhoods and types of shootings. We show that a police shooting has larger effects in Minority neighborhoods compared to White neighborhoods. The former experience a large and significant increase in gun-related crimes compared to the latter, and thus a significant decrease in the reporting rate. Additionally, we show that nonfatal shootings have larger effects compared to fatal ones. Finally, we also explore the potential for “de-policing” as a mechanism for the increase in gun violence by examining police-initiated 911 calls for service, such as traffic stops and patrolling events, and arrest data. Our results show that this is not the case, and that the increase in gun violence is not driven by changes in police effort.

Our work speaks directly to the policy debate on policing and civilians’ trust. This debate has intensified in the wake of the murder of George Floyd in Minneapolis, which has led to a 50 percent decrease in 911 calls per gunshot (Ang, Bencsik, Bruhn and Derenoncourt 2021). Our results are in line with Ang, Bencsik, Bruhn and Derenoncourt 2021, albeit reasonably smaller in magnitude. In our paper, we focus on less publicized incidents of police violence. As we have shown, these incidents receive significantly lower news coverage compared to George Floyd, leading to smaller and more local impacts. In contrast, the highly-publicized killing of George Floyd has led to nationwide protests and social media campaigns, instigating geographically dispersed and wide-ranging implications. These include a decline in policing effort, arrests and police-initiated calls for service, as shown in Mikdash and Zaiour 2022.

The extent to which our findings extrapolate to reporting of other types of crime — for which there is no objective measure independent of reporting— is an open question. However, our results indicate that police violence has important negative effects on civilian cooperation with the police. Moreover, violent encounters with the police may counteract the positive effects of policing, by increasing gun violence and reducing civilians’ cooperation. The latter is especially critical, given that police heavily rely on cooperation from the public in order to solve past crimes and deter future ones. Importantly, we demonstrate that this is true even for police shootings that are not publicized, which is the case for the majority of such incidents.

## **9 Acknowledgements**

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## **10 Declaration of competing interest**

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors. We have no conflicts of interest to disclose.

## **11 Data availability**

All data are publicly available at the sources cited in the text. Data from the Minneapolis Police Department are available on their Open Data website or through a Freedom of Information Act (FOIA) submission. Use the URLs provided to locate the sources of the other data. Data not readily accessible through the provided sources will be made available upon

request.

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# Tables and Figures

Table 1: Summary Statistics I

	(1) Entire Sample	(2) $\leq 0.4$ miles	(3) $> 0.5$ miles
<b><u>Block Characteristics</u></b>			
Percent White	34.34 (21.70)	32.24 (20.39)	37.46 (23.18)
Percent Black	32.85 (20.81)	36.59 (21.79)	27.26 (17.86)
Percent Hispanic	7.242 (8.097)	6.377 (7.581)	8.532 (8.651)
Total Population	113.2 (81.40)	111.9 (84.06)	115.1 (77.23)
<b><u>Outcomes</u></b>			
ShotSpotter	0.145 (0.465)	0.192 (0.538)	0.0751 (0.313)
Shots Reported	0.125 (0.414)	0.159 (0.472)	0.0754 (0.301)
Arrests	0.930 (2.128)	0.914 (1.947)	0.954 (2.373)
Police-initiated Calls	4.377 (9.501)	4.198 (8.968)	4.644 (10.24)
Observations	64416	38544	25872

Standard deviations in parentheses.

Notes: This table shows the summary statistics for the Census block characteristics, in addition to the mean and standard deviation of outcome variables at the block-by-month level. Column (2) shows the summary statistics for Census blocks that are within a 0.4 miles distance from a police shooting (treated blocks), while column (3) shows the summary statistics for Census blocks that are more than 0.5 miles away from any police shooting (i.e., the common control blocks that are never treated). The sample is restricted to blocks that have non-zero total population.

Table 2: Effect of a Police Shooting on ShotSpotter Incidents Characteristics

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Day Time</b>					
After a Police Shooting	0.0299 (0.032)	0.0103 (0.017)	0.0110 (0.015)	0.0109 (0.016)	0.0125 (0.021)
Outcome Mean	0.175	0.175	0.175	0.175	0.175
<b>Panel B: Weekend</b>					
After a Police Shooting	-0.0183 (0.016)	0.0127 (0.019)	-0.00581 (0.019)	-0.0181 (0.022)	-0.0156 (0.019)
Outcome Mean	0.372	0.372	0.372	0.372	0.372
Observations	24298	24298	24298	24298	24298

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the effect of a police shooting on the characteristics of ShotSpotter incidents. We estimate equation (1), where  $Y_{bt}$  is the day/time of each ShotSpotter incident. Day time is a dummy variable that takes the value 1 if the incident happens between 6 am and 6 pm. Weekend is a dummy variable that takes the value 1 if it happened on a Saturday or a Sunday. All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level.

Table 3: Effect of a Police Shooting on Shots Reported and ShotSpotter

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Shots Reported</b>					
After a Police Shooting	0.00557 (0.009)	-0.0201*** (0.007)	-0.0119* (0.007)	-0.0125** (0.006)	-0.0106 (0.009)
Observations	44352	59532	71412	76956	80256
<b>Panel B: ShotSpotter</b>					
After a Police Shooting	0.0641** (0.024)	0.0548*** (0.018)	0.0575*** (0.015)	0.0611*** (0.020)	0.0474** (0.022)
Observations	44352	59532	71412	76956	80256
Difference	-0.0585	-0.0748	-0.0695	-0.0736	-0.0580
SE Difference	0.0262	0.0195	0.0165	0.0213	0.0237
P-value (Difference = 0)	0.0253	0.000122	0.0000258	0.000552	0.0144

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the difference-in-differences estimates from Equation 1, using blocks that are more than 0.5 miles away from any shooting as a control group for all five radii. Panel (A) shows the effect of a police shooting on shots reported, while Panel (B) shows the effect on ShotSpotter incidents. We also estimate the difference between the effect on ShotSpotter and the effect on shots reported, which represents the effect on the reporting rate. We include the p-values for the Wald tests, where  $H_0: \beta_1^{SS} - \beta_1^{SR} = 0$ . All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level.

Table 4: Heterogeneous Effects of Police-involved Shootings

	<u>Neighborhood</u>		<u>Subject</u>		<u>Shooting</u>	
	(1) Minority	(2) White	(3) Armed	(4) Unarmed	(5) Fatal	(6) Nonfatal
<b>Panel A: Shots Reported</b>						
After a Police Shooting	-0.0184** (0.009)	-0.00771 (0.006)	-0.0145 (0.011)	-0.0134 (0.010)	-0.00572 (0.008)	-0.0128** (0.006)
Observations	26004	26664	54912	37620	36432	73524
P-value (Difference = 0)		0.330		0.942		0.471
<b>Panel B: ShotSpotter</b>						
After a Police Shooting	0.0697*** (0.021)	0.0145 (0.013)	0.0591** (0.025)	0.00294 (0.016)	0.0000396 (0.012)	0.0660*** (0.022)
Observations	26004	26664	54912	37620	36432	73524
P-value (Difference = 0)		0.0262		0.0614		0.00735

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the heterogeneity tests results using the 0.4 miles radius to define treatment. First, we estimate the effect of a police shooting in Minority neighborhoods and White neighborhoods, separately. A Census block is defined to be White (Minority) if more than 50 percent of its population are White (Minority). Second, we estimate the effect of shooting an armed civilian and an unarmed civilian, separately. The information revealing whether the civilian was armed or not is missing for 16 shootings in the sample. In columns (3) and (4), we exclude the shootings where the information about the civilian's weapon is missing, along with the blocks associated with them. Finally, we estimate the effect of a fatal shooting and a nonfatal shooting, separately. In all columns, control blocks are those that did not experience any shootings within the time period. We use a t-test to compare the coefficients across even and odd-numbered columns within each group of estimates, and we report their p-values. All regressions include block and month-year fixed effects. Standard errors are clustered at the Census tract level.

Table 5: Effect of a Police Shooting on Police Activity

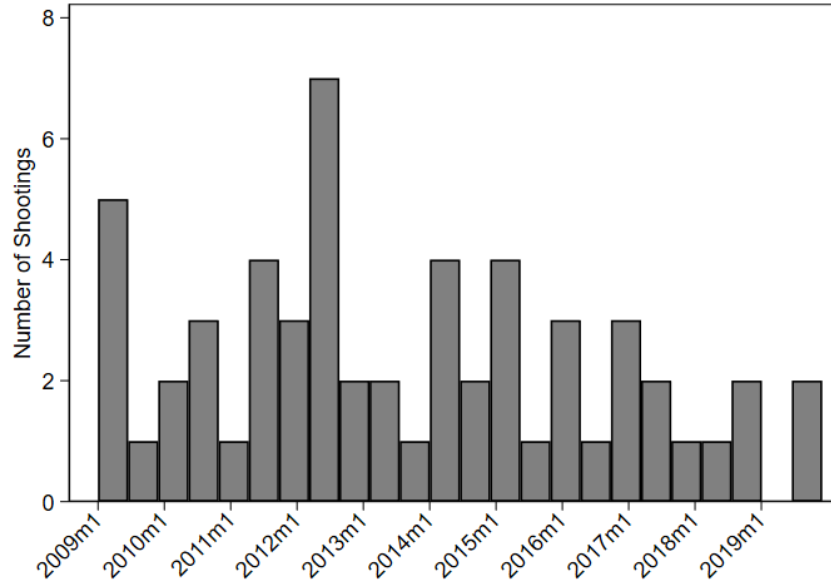
	(1)	(2)
	Police-Initiated Events	Arrests
After a Police Shooting	0.0650 (0.0395)	0.0122 (0.0322)
Observations	76956	69960

Standard errors in parentheses

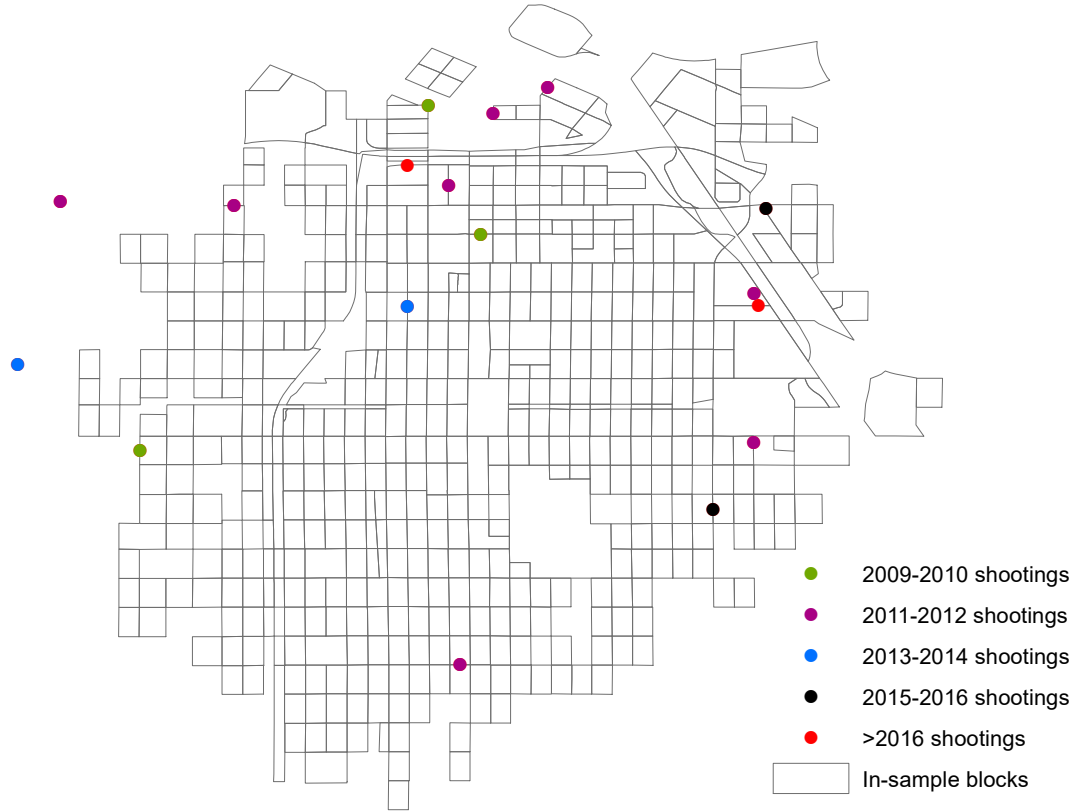
\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$

Notes: This table shows the difference-in-differences effect of a police shooting on police activity, as measured by the inverse hyperbolic transformations of police calls and arrests using Equation 1. We use the 0.4 miles radius to define treatment. All regressions include block and month-year fixed effects. Standard errors are clustered at the Census tract level.

Figure 1: Police-involved Shootings in Minneapolis



(a) Frequency over time

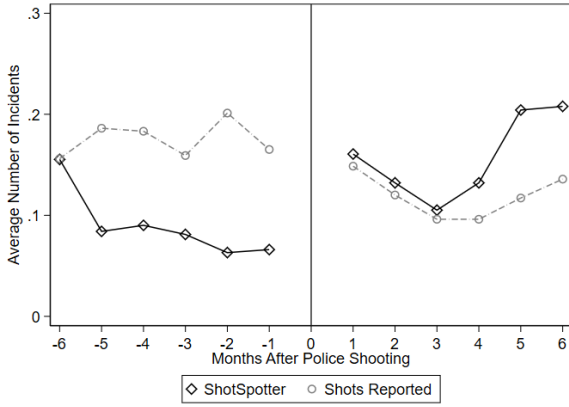


(b) Location

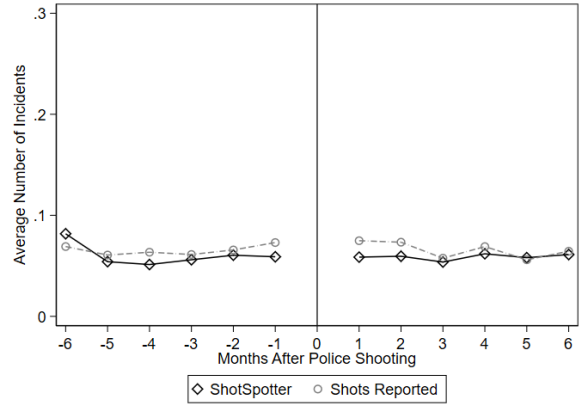
Notes: Panel (a) shows the distribution of all the police-involved shootings that occurred between 2009 and 2019 in Minneapolis. Each bin is 6 months long. Panel (b) shows a snapshot of a neighborhood in the city, along with the geographical location of some of these police-involved shootings by year of occurrence. For context, the length of a Census block can vary between 0.1 and 0.3 miles. For a full map of the city showing all 57 shootings, refer to Figure A1.



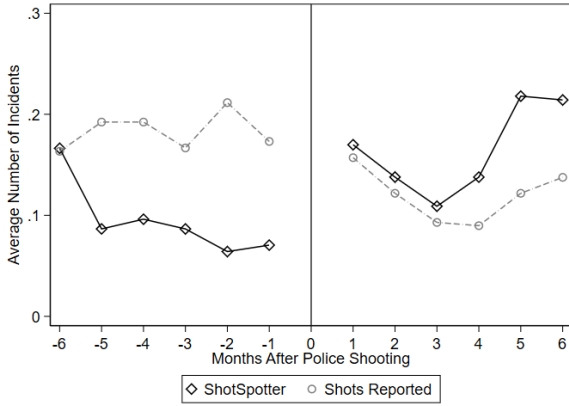
Figure 2: ShotSpotter and Shots Reported over Time



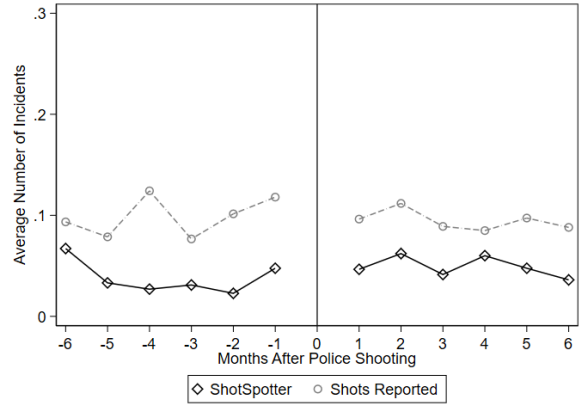
(a) Treated blocks



(b) Never-treated blocks



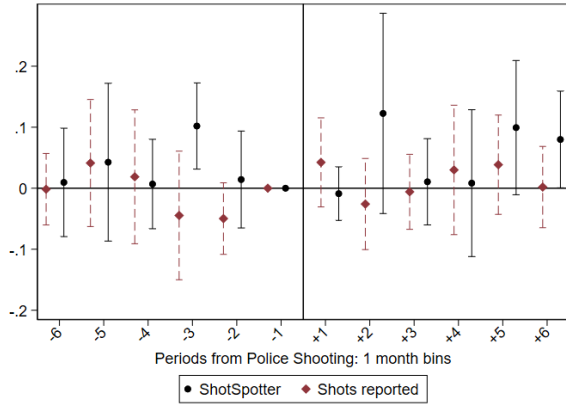
(c) Early-treated blocks



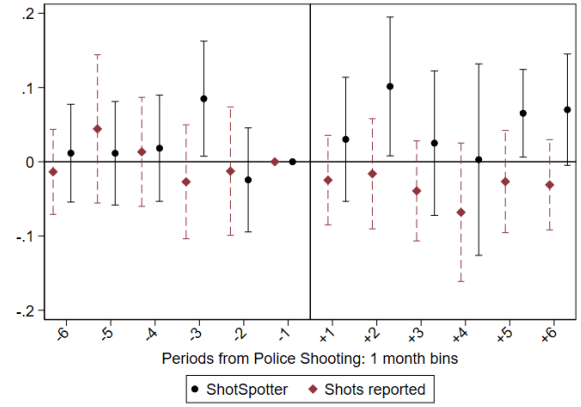
(d) Later-treated blocks

Notes: These figures show the average number of ShotSpotter incidents and shots reported over time for four groups: (a) treated blocks, (b) never-treated blocks, (c) early-treated blocks, and (d) later-treated blocks. Never-treated blocks are our common control group, i.e. the blocks that are more than 0.5 miles away from any police shooting. Early-treated blocks are blocks that are exposed to a police shooting before 2015, while the later-treated blocks are those that are exposed to a police shooting after 2015 and are used as a control group for the early-treated blocks. In all figures, the x-axis is normalized relative to the treatment time, where 0 marks the time of treatment, and each period is one month long. To plot the data for the control blocks, we first normalize the time periods around each shooting date separately and for each block. Then, we stack the normalized periods and compute the average of our outcome variables across control blocks for each period. To create the time periods for the never-treated blocks, the treatment dates are the dates of all the police-involved shootings between 2010-2018. To create the time periods for the later-treated blocks, the treatment dates are the dates of the police-involved shootings that occurred before 2015. In all figures, we use the 0.4 miles radius to determine treatment status. The figures for other treatment definitions are reported in subsection A.2.

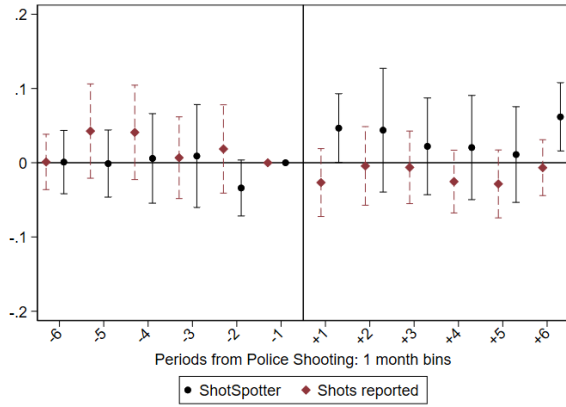
Figure 3: Event-study Analysis



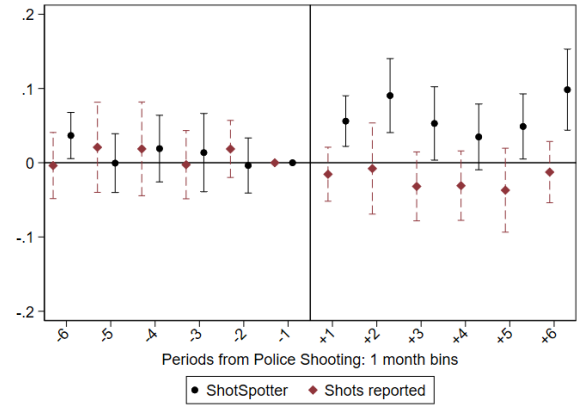
(a) 0.1 miles



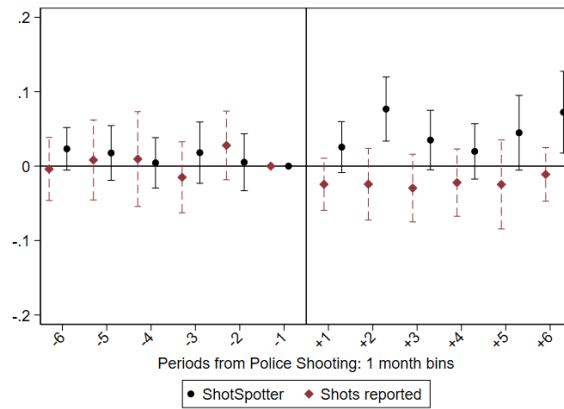
(b) 0.2 miles



(c) 0.3 miles



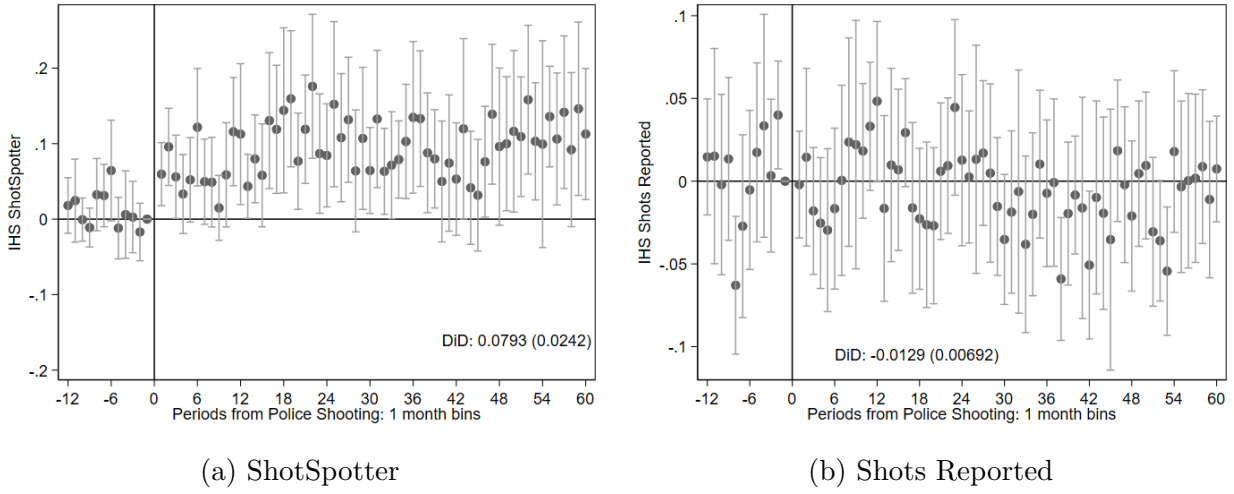
(d) 0.4 miles



(e) 0.5 miles

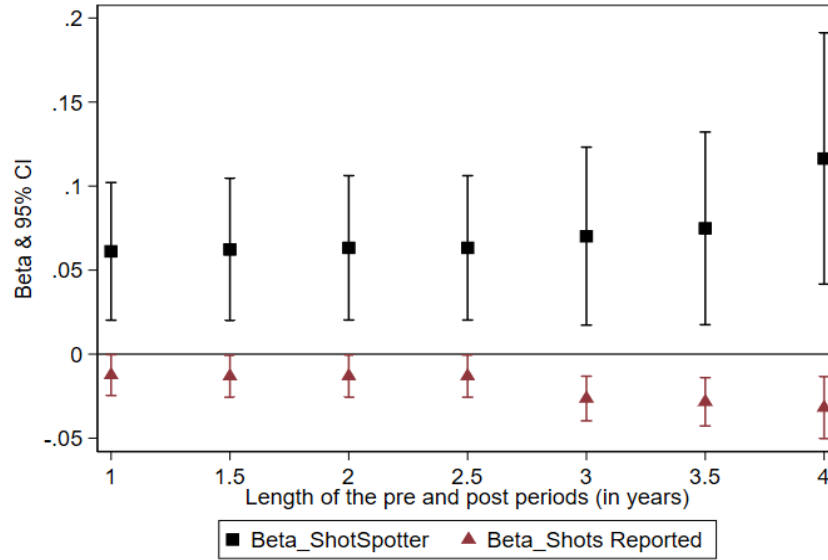
Notes: These figures show the estimated coefficients and the 95 percent confidence intervals using Equation 2 for all five definitions of treatment, where the outcome is the inverse hyperbolic transformation of ShotSpotter incidents (black) and shots reported (maroon). Census block fixed effects as well as month-by-year fixed effects are included. Standard errors are clustered at the tract level. Each period is one month long, and period -1 is excluded.

Figure 4: Long-run Effect of a Police Shooting on Shots Reported and ShotSpotter



Notes: These figures show the long-run effect of a police shooting on ShotSpotter and shots reported, using the 0.4 miles radius to define treatment. We focus on the shootings that occur before 2015. We estimate the dynamic difference-in-differences effects using Equation 2, and we report the coefficients and the 95% confidence intervals for both outcomes. Each period is one-month long, and period -1 is the omitted period. Additionally, we estimate the difference-in-difference estimates using Equation 1. We include these estimates along with their standard errors in parentheses. We control for month-by-year fixed effects and block fixed effects, and we cluster the standard errors at the tract level.

Figure 5: Difference-in-differences Estimates by the Length of the Pre- and Post-periods



Notes: This figure represents the coefficients from the difference-in-differences estimates for each outcome over different lengths of the pre- and post-periods, using the 0.4 miles radius. The shortest period used is 1-year on each side of the cutoff (main estimates). We gradually increase the pre- and post-periods by six months until we reach 4 years on each side of the cutoff. We estimate the difference-in-differences separately for each length, and we report the coefficients and the 95% confidence intervals in maroon for shots reported and black for ShotSpotter. It should be noted that the number of shootings included varies by the length used. Specifically, there are 24, 22, 21, 21, 16, 13, and 9 shootings at the 1, 1.5, 2, 2.5, 3, 3.5 and 4-year long pre- and post-periods, respectively.

## A Online Appendix

### A.1 Derivation of the Effect of a Police Shooting on the Reporting Rate

In our analysis, we do not directly estimate the effect of police shootings on the reporting rate. In this subsection, we discuss how our results allow us to infer the direction of the effect of police-involved shootings on the crime reporting rate. Let  $\beta^{SR}$ ,  $\beta^{SS}$ , and  $\alpha$  be the effect on shots reported (SR), ShotSpotter (SS) and willingness to report (WTR) respectively. For simplicity, assume equation 1 is a simple 2x2 difference-in-difference equation. When the outcome is the inverse hyperbolic transformation of shots reported through 911 calls,  $\beta^{SR}$  would be estimating the effect of exposure to police violence in a given block,  $b$ , in the following way:

$$\beta^{SR} = E[\underbrace{(IHS\_SR_{b,1} - IHS\_SR_{b,0})}_{\text{Treated Blocks}} - \underbrace{(IHS\_SR_{c,1} - IHS\_SR_{c,0})}_{\text{Control Blocks}}] \quad (5)$$

However, as previously explained, the shots reported through 911 calls are only a fraction of the total gunshots occurring in a certain geography. Since we have a true measure of the total gunshots (those detected by ShotSpotter, SS), we can write the number of shots reported as a function of ShotSpotter incidents (SS) and the willingness to report (WTR) as such:

$$SR_{bt} = WTR_{bt} \times SS_{bt} \quad (6)$$

Plugging equation 6 into equation 5, we further derive  $\beta^{SR}$  as follows<sup>29</sup>:

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<sup>29</sup>SR and SS are inverse hyperbolic sine transformations of the number of gunshots. The transformation is defined as follows:  $\log(y_i + (y_i^2 + 1)^{1/2})$ . That is almost equal to  $\log(2) + \log(y_i)$ . Thus, we can perform the decomposition below.

$$\begin{aligned}
\beta^{SR} = & \underbrace{(E[IHS\_WTR_{b,1} - IHS\_WTR_{b,0}])}_{(a)} + \underbrace{E[IHS\_SS_{b,1} - IHS\_SS_{b,0}]}_{(b)} \\
& \underbrace{\hspace{10em}}_{\text{Treated blocks}} \\
& - \underbrace{(E[IHS\_WTR_{b,1} - IHS\_WTR_{b,0}])}_{(c)} + \underbrace{E[IHS\_SS_{b,1} - IHS\_SS_{b,0}]}_{(d)} \\
& \underbrace{\hspace{10em}}_{\text{Control blocks}}
\end{aligned} \tag{7}$$

In the above equation, terms (b) minus (d) reflect  $\beta^{SS}$ , the effect of police violence on all gunshot crimes that are detected by ShotSpotter. Studies are not usually able to estimate this portion of the equation because of the absence of a true measure of crime.<sup>30</sup> In our case, we are able to estimate this portion because of the ShotSpotter data.

Finally, terms (a) minus (c) reflect  $\alpha$ , the effect of police violence on the willingness to report. Using equation 7, we can deduce that the change in crime reporting behavior can be derived according to the following equation:

$$\alpha = \beta^{SR} - \beta^{SS} \tag{8}$$

---

<sup>30</sup>In Jácome 2022, the author estimates the effect of the 2015 Priority Enforcement Program (PEP) on Hispanic crime reporting in Dallas. The outcome used is the log number of incidents reported by Hispanic and non-Hispanic individuals. The author does not have a true measure of crime, but rather only observes the crime that was reported. Thus, the author touches upon a similar discussion to show that her estimates are underestimated. Our discussion differs because we have a true measure of gunshots, and we can estimate all parts of the equation.

## A.2 Tables and Figures

Table A1: Number of Police-involved Shootings at Each Radius

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
Restriction 1: focus on first shooting	31	32	34	30	28
Restriction 2: use shootings that occurred between 2010 & 2018	23	24	26	23	21

Notes: This table shows the number of effective police-involved shootings used to define treatment at each radius after each data restriction. There are 57 unique police shootings in Minneapolis between 2009 and 2019. Treatment is determined based on the first police shooting for each treated block. After this restriction, the number of police shootings that remain for each radius are presented in the first row. Next, shootings from 2009 and 2019 are excluded, and we display the remaining number of shootings in the second row.

Table A2: Summary Statistics II

	(1) All Shootings	(2) 0.1 miles	(3) 0.2 miles	(4) 0.3 miles	(5) 0.4 miles	(6) 0.5 miles
Female	10.71 (31.209)	4.35 [0.21]	4.17 [0.18]	3.85 [0.13]	4.35 [0.21]	4.76 [0.27]
Black	72.22 (45.211)	76.19 [0.61]	81.82 [0.20]	79.17 [0.32]	76.19 [0.61]	78.95 [0.43]
White	11.11 (31.722)	4.76 [0.24]	0.00 [0.03]	0.00 [0.02]	0.00 [0.04]	0.00 [0.06]
Hispanic	1.85 (13.608)	4.76 [0.21]	4.55 [0.23]	4.17 [0.27]	4.76 [0.21]	5.26 [0.18]
Fatal	15.79 (36.788)	13.04 [0.65]	12.50 [0.57]	15.38 [0.94]	13.04 [0.65]	9.52 [0.33]
Number	57	23	24	26	23	21

Notes: Column (1) presents the mean characteristics for all 57 police-involved shootings that occurred in Minneapolis between 2009-2019, along with the standard error in parentheses. Columns (2)-(6) provide summary statistics for the shootings included in the analysis at each radius. We compare the police shootings included at each radius to the excluded sample of shootings, and we report the corresponding p-value of the t-test for each characteristic in square brackets. Sex and race information are missing for one and three shootings, respectively.



Table A3: Difference-in-differences Estimates using Block-Level Clustering

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Shots Reported</b>					
After a Police Shooting	0.00557 (0.009)	-0.0201*** (0.007)	-0.0119** (0.006)	-0.0125** (0.005)	-0.0106* (0.006)
Observations	44352	59532	71412	76956	80256
<b>Panel B: ShotSpotter</b>					
After a Police Shooting	0.0641*** (0.016)	0.0548*** (0.010)	0.0575*** (0.008)	0.0611*** (0.008)	0.0474*** (0.007)
Observations	44352	59532	71412	76956	80256
Difference	-0.0585	-0.0748	-0.0695	-0.0736	-0.0580
SE Difference	0.0186	0.0122	0.00998	0.00952	0.00914
P-value (Difference = 0)	0.00160	0	0	0	0

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the difference-in-differences estimates from Equation 1. Panel (A) shows the effect on shots reported, while Panel (B) shows the effect on ShotSpotter. We also estimate the difference between the effect on ShotSpotter and the effect on shots reported, which represents the effect on the reporting rate. We include the p-values for the Wald tests, where  $H_0: \beta_1^{SS} - \beta_1^{SR} = 0$ . All regressions include block and month-year fixed effects, and standard errors are clustered at the Census block level.

Table A4: Difference-in-differences Estimates using a Varying Control Group

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Shots Reported</b>					
After a Police Shooting	0.0167** (0.008)	-0.0104 (0.007)	-0.00292 (0.007)	-0.00711 (0.006)	-0.0106 (0.009)
Observations	122628	113124	103356	90156	80256
<b>Panel B: ShotSpotter</b>					
After a Police Shooting	0.0323 (0.023)	0.0355** (0.016)	0.0468*** (0.013)	0.0567*** (0.018)	0.0474** (0.022)
Observations	122628	113124	103356	90156	80256
Difference	-0.0156	-0.0459	-0.0497	-0.0638	-0.0580
SE Difference	0.0241	0.0175	0.0147	0.0189	0.0237
P-value (Difference = 0)	0.517	0.00872	0.000713	0.000746	0.0144

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the difference-in-differences results from Equation 1, using blocks that are beyond “r” miles away from a police-involved shooting as a control group. Panel (A) shows the effect on shots reported, while Panel (B) shows the effect on ShotSpotter. We also estimate the difference between the effect on ShotSpotter and the effect on shots reported, which represents the effect on the reporting rate. We include the p-values for the Wald tests, where  $H_0: \beta_1^{SS} - \beta_1^{SR} = 0$ . All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level.

Table A5: Difference-in-differences using Subsequent Police Shootings

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Shots Reported</b>					
After 1st police shooting	0.00492 (0.009)	-0.0216*** (0.007)	-0.0117* (0.006)	-0.0128** (0.005)	-0.0119 (0.008)
After 2nd police shooting	0.0105 (0.027)	0.0210 (0.024)	-0.00226 (0.014)	0.00245 (0.012)	0.00758 (0.007)
Observations	44352	59532	71412	76956	80256
<b>Panel B: ShotSpotter</b>					
After 1st police shooting	0.0615** (0.024)	0.0495*** (0.017)	0.0496*** (0.013)	0.0498** (0.020)	0.0328 (0.022)
After 2nd police shooting	0.0433 (0.062)	0.0731* (0.037)	0.0866*** (0.022)	0.0813*** (0.024)	0.0865*** (0.029)
Observations	44352	59532	71412	76956	80256

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the difference-in-differences estimates using the following equation:  $Y_{bt} = \alpha_0 + \alpha_1 * Treat_b \times Post1_t + \alpha_2 * Treat_b \times Post2_t + MonthYear_t + Block_b + u_{bt}$ , where  $Post1_t$  equals 1 if  $t \geq t(\text{first police shooting})$  and 0 otherwise, and  $Post2_t$  equals 1 if  $t \geq t(\text{second police shooting})$ . Panel (A) shows the effect on shots reported, while Panel (B) shows the effect on ShotSpotter. All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level.

Table A6: Effect of a Police Shooting on 911 Calls

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Broader Shots Reported Measure</b>					
After a Police Shooting	0.0108 (0.015)	-0.0235** (0.011)	-0.0167* (0.009)	-0.0156* (0.009)	-0.0118 (0.011)
Observations	44352	59532	71412	76956	80256
<b>Panel B: All Calls</b>					
After a Police Shooting	0.0136 (0.033)	-0.0309 (0.028)	-0.0316 (0.024)	-0.0328 (0.026)	-0.0159 (0.023)
Observations	44352	59532	71412	76956	80256
<b>Panel C: Low Priority Calls</b>					
After a Police Shooting	0.0679** (0.028)	0.0118 (0.014)	0.0106 (0.012)	0.00433 (0.013)	0.00572 (0.012)
Observations	44352	59532	71412	76956	80256
<b>Panel D: High Priority Calls</b>					
After a Police Shooting	0.00155 (0.027)	-0.0427* (0.022)	-0.0464** (0.018)	-0.0420** (0.018)	-0.0218 (0.017)
Observations	44352	59532	71412	76956	80256

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the difference-in-differences results from Equation 1. Panel (A) shows the effect on the inverse hyperbolic transformation on the volume of a broader measure of shots reported. This measure also includes incidents involving a gun (that do not explicitly mention shots fired) or a weapon (even if the weapon is unspecified), such as calls about a “person with a gun or weapon” or “domestic violence involving a weapon.” Panel (B) shows the effect on the inverse hyperbolic transformation of the overall volume of 911 calls. Panel (C) shows the effect on the inverse hyperbolic transformation of the volume of low priority calls reporting noise disturbance, theft reports, unwanted/suspicious person, etc, and Panel (D) shows the effect on the inverse hyperbolic transformation of the volume of high priority calls reporting assault, stabbings, robbery of businesses, etc. All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level.

Table A7: Difference-in-Difference Effects using Callaway and Sant’Anna 2021

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Shots Reported</b>					
After a Police Shooting	0.021 (0.026)	-0.020 (0.027)	-0.001 (0.019)	-0.017 (0.022)	-0.018 (0.020)
Observations	44352	59532	71412	76956	80256
<b>Panel B: ShotSpotter</b>					
After a Police Shooting	0.095** (0.039)	0.112*** (0.028)	0.093*** (0.028)	0.104*** (0.030)	0.084*** (0.030)
Observations	44352	59532	71412	76956	80256
Difference	-0.074 (0.047)	-0.131 (0.039)	-0.094 (0.034)	-0.120 (0.038)	-0.102 (0.036)
P-value	0.119	0.001	0.006	0.001	0.005

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ 

Notes: This table shows the difference-in-differences results using the Callaway and Sant’Anna 2021 procedure. We estimate the overall ATT effect using the doubly robust estimator developed by Sant’Anna and Zhao 2020. We use the never-treated blocks as a control group in each estimation. Panel (A) shows the effect on shots reported, while Panel (B) shows the effect on ShotSpotter. We also estimate the difference between the effect on ShotSpotter and the effect on shots reported, which represents the effect on the reporting rate. We include the p-values for the Wald tests, where  $H_0: \beta_1^{SS} - \beta_1^{SR} = 0$ . All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level. We perform the estimation using the “csdid” command, which is provided by the STATA package created by Rios-Avila et al. 2021.

Table A8: Difference-in-Difference Effects using Sun and Abraham 2021

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
<b>Panel A: Shots Reported</b>					
After a Police Shooting	0.021* (0.012)	-0.027 (0.017)	0.002 (0.013)	-0.002 (0.014)	-0.004 (0.012)
Observations	44352	59532	71412	76956	80256
<b>Panel B: ShotSpotter</b>					
After a Police Shooting	0.051*** (0.015)	0.043*** (0.012)	0.041*** (0.013)	0.068*** (0.012)	0.046*** (0.013)
Observations	44352	59532	71412	76956	80256
Difference	-0.030 (0.019)	-0.070 (0.021)	-0.039 (0.018)	-0.070 (0.018)	-0.050 (0.018)
P-value	0.113	0.001	0.034	0.000	0.004

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ 

Notes: This table shows the difference-in-differences results using the Sun and Abraham 2021 procedure. We implement the interaction weighted (IW) estimator. Panel (A) shows the effect on shots reported, while Panel (B) shows the effect on ShotSpotter. We also estimate the difference between the effect on ShotSpotter and the effect on shots reported, which represents the effect on the reporting rate. We include the p-values for the Wald tests, where  $H_0: \beta_1^{SS} - \beta_1^{SR} = 0$ . All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level. We perform the estimation using the “eventstudyinteract” command, which is provided by the STATA package created by Sun 2021.

Table A9: Long-run Difference-in-differences Effects on Shots Reported and ShotSpotter

	(1) ShotSpotter	(2) Shots Reported
<6 months	0.0546*** (0.0147)	-0.0237*** (0.00825)
6-11 months	0.0523** (0.0209)	0.0138 (0.0110)
12-17 months	0.0805*** (0.0281)	-0.0118 (0.00803)
18-23 months	0.101*** (0.0303)	-0.00740 (0.0116)
$\geq 24$ months	0.0874*** (0.0300)	-0.0174* (0.00891)
Observations	70488	70488

Standard errors in parentheses

\*  $p < .10$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ 

Notes: This table shows the effect of a police shooting on ShotSpotter and shots reported. To estimate the long-run impact, we use police shootings that occurred before 2015 in order to observe at least 5 years after a police shooting for each treated block. This analysis separately estimates the effect of a police shooting within 6 months, 6-11 months, 12-17 months, 18-23 months, and 24 months of a police shooting. All regressions include block and month-year fixed effects, and standard errors are clustered at the Census tract level.

Table A10: Robustness Checks

	(1)	(2)	(3)	(4)
<b>Panel A: Shots Reported</b>				
After a Police Shooting	-0.0119* (0.006)	-0.0120 (0.011)	-0.0123 (0.009)	-0.0000183 (0.018)
Observations	76956	51794	17688	11172
<b>Panel B: ShotSpotter</b>				
After a Police Shooting	0.0610*** (0.021)	0.0326** (0.015)	0.0751*** (0.024)	0.116*** (0.040)
Observations	76956	51794	17688	11172
Difference	-0.0728	-0.0446	-0.0874	-0.116
SE Difference	0.0216	0.0188	0.0262	0.0441
P-value (Difference = 0)	0.000743	0.0175	0.000839	0.00830
Tract*Month FE	Y	N	N	N
Short Run (0-2 months)	N	Y	N	Y
High Crime	N	N	Y	Y

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Notes: This table shows the difference-in-differences results. It also shows the p-values of the Wald tests, where  $H_0: \beta_1^{SS} - \beta_1^{SR} = 0$ . In all specifications, we include block and month-by-year fixed effects. In column (1), we also include tract-by-month fixed effects. In column (2), we restrict the post-period to 2 months after a shooting to estimate the short-run effects. In column (3), we restrict the sample of blocks to “high crime areas”, i.e. blocks that had more than one shooting in 2009 and 2010, and in column (4) we estimate the effects in “high crime areas” in the short run. Standard errors are clustered at the tract level in all specifications.



Table A11: Difference-in-differences Estimates using Different Outcome Transformations

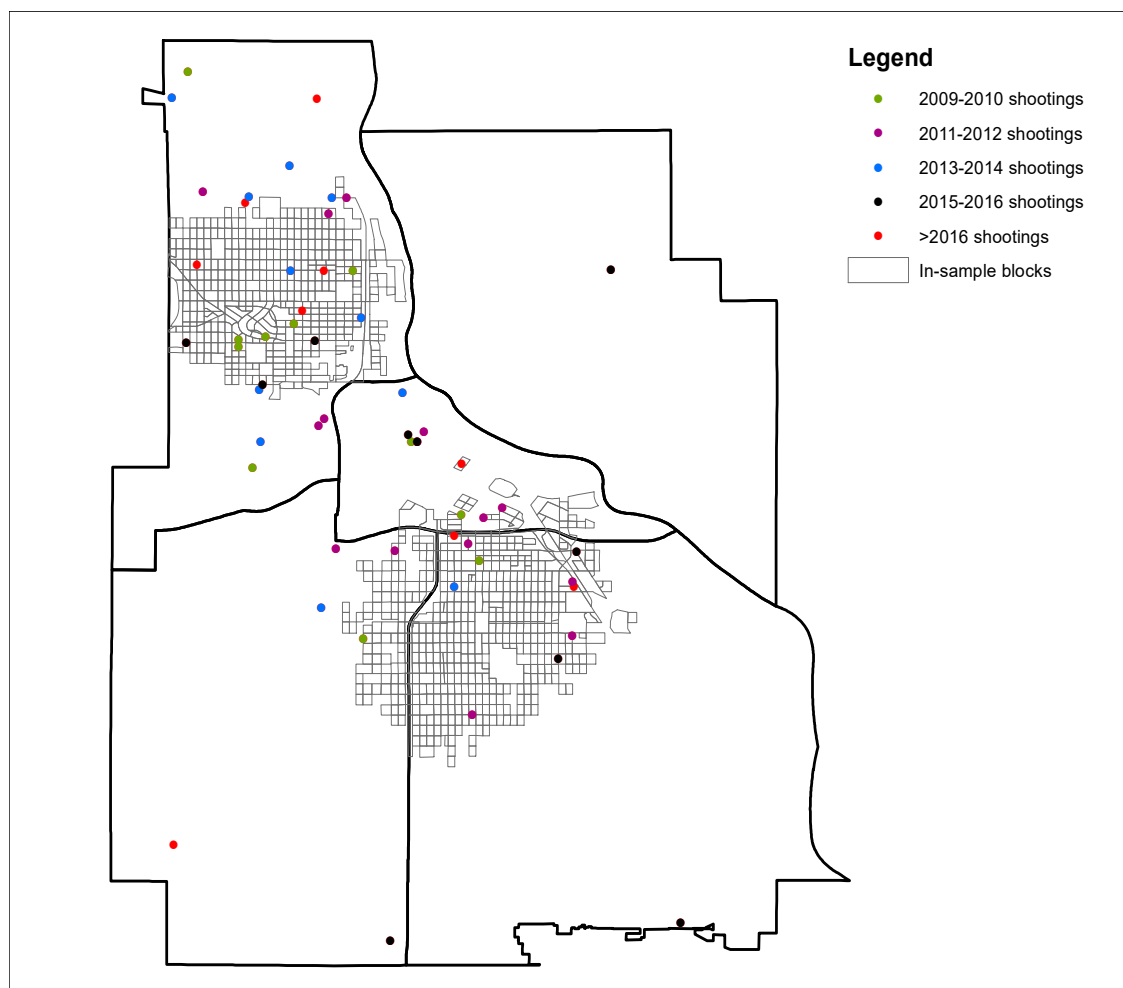
	(1)	(2)	(3)
<b>Panel A: Shots Reported</b>			
After a Police Shooting	-0.0175** (0.008)	-0.00957** (0.005)	-0.00957 (0.006)
Observations	76956	76956	76956
<b>Panel B: ShotSpotter</b>			
After a Police Shooting	0.0815*** (0.028)	0.0473*** (0.016)	0.0535*** (0.018)
Observations	76956	76956	76956
Levels	Y	N	N
Log(y+1)	N	Y	N
Extensive Margin	N	N	Y

Standard errors in parentheses.

\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ 

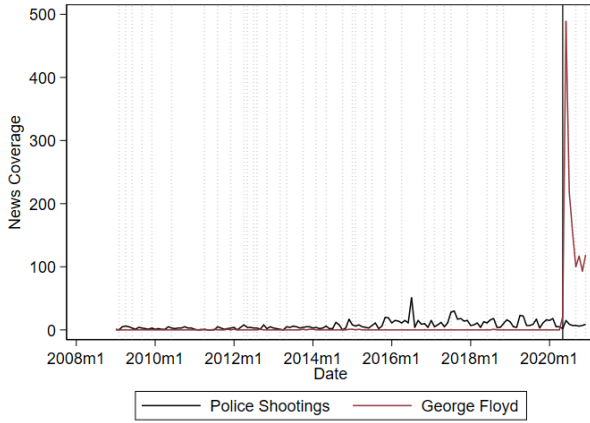
Notes: This table shows the difference-in-differences results, using different functional forms to transform the outcome variables. Panel (A) shows the effect on shots reported, while Panel (B) shows the effect on ShotSpotter. In column (1), the outcomes are presented in their original levels. Column (2) applies the  $\log(y+1)$  transformation on the outcomes. In column (3), the outcomes are dummy variables with a value of 1 if ShotSpotter incidents or reported shootings are larger than zero. In all specifications, we include block and month-by-year fixed effects. Standard errors are clustered at the Census block level in all specifications.

Figure A1: Police-involved Shootings in Minneapolis

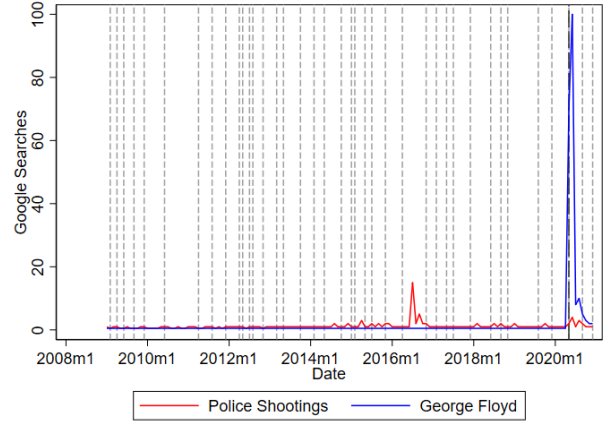


Notes: This map shows the geographical distribution of *all* police-involved shootings in Minneapolis, in addition to the “in-sample” blocks, i.e. the blocks that experience at least 1 ShotSpotter incident between 2007 and 2009. The thick black lines outline the boundaries of the city’s five police precincts. Each dot represents a police shooting, color-coded according to the year it occurred. For context, the length of a Census block can vary between 0.1 and 0.3 miles.

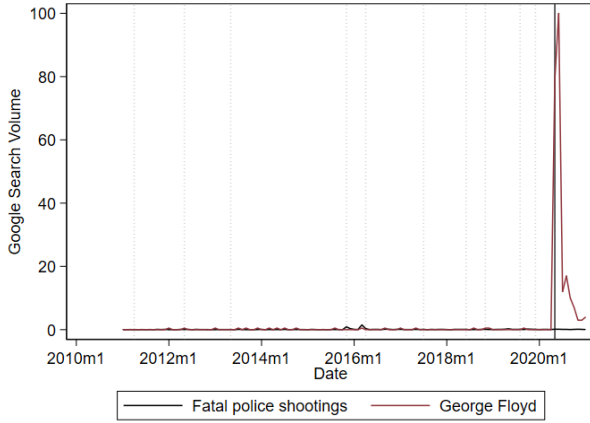
Figure A2: News Coverage and Google Trends for Police Shootings vs George Floyd



(a) LexisNexis



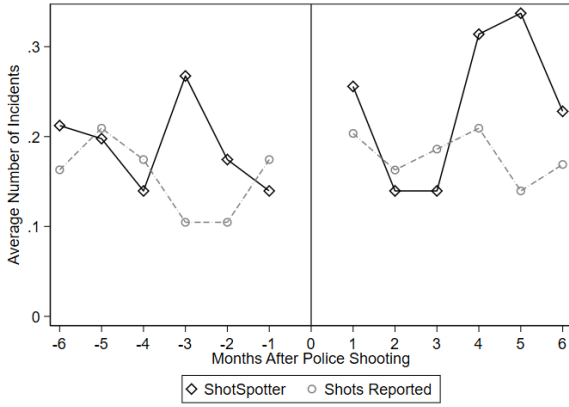
(b) Google search volume for “police shooting”



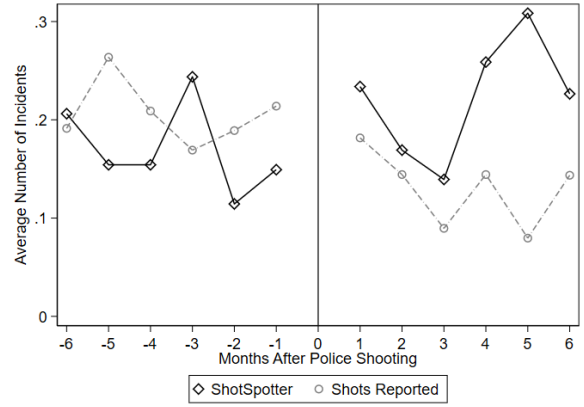
(c) Google search volume for victim names in fatal police shootings

Notes: The purpose of these figures is to demonstrate the relative level of public attention given to police shootings in comparison to George Floyd, using data from LexisNexis and Google trends. In panel (a), we download the number of news articles that included the terms “police shooting”, “police shot”, or “police-involved shooting”, and “George Floyd” in Minnesota overtime. In panel (b), we download the Google search volume for the term “police shooting” vs “George Floyd”. In panel (c), we download the Google search volume for victims’ names in fatal police shootings vs “George Floyd”. The y-axes in panels (b) and (c) represent the search interest as calculated by Google. Note that Google calculates the search volume (or the interest level) relative to the highest point on the chart; “a value of 100 is the peak popularity for the term. A value of 50 means that the term is half as popular. A score of 0 means that there was not enough data for this term”. In all three panels, we plot the time series of the number of articles and/or Google trends of police shootings and compare it to news coverage or Google search volume for George Floyd. The dashed vertical lines represent the actual dates of police-involved shootings in Minneapolis, while the black dashed line represents the date of George Floyd’s killing.

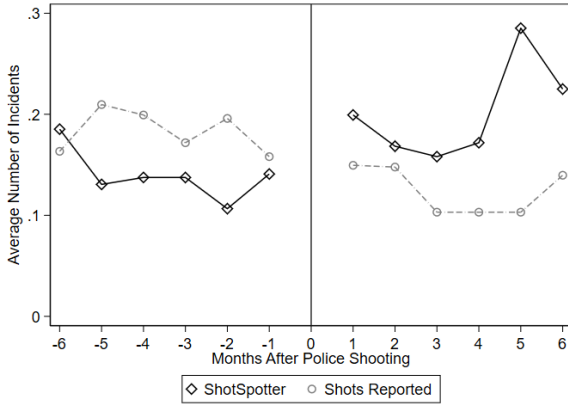
Figure A3: ShotSpotter and Shots Reported over Time (Treated Blocks)



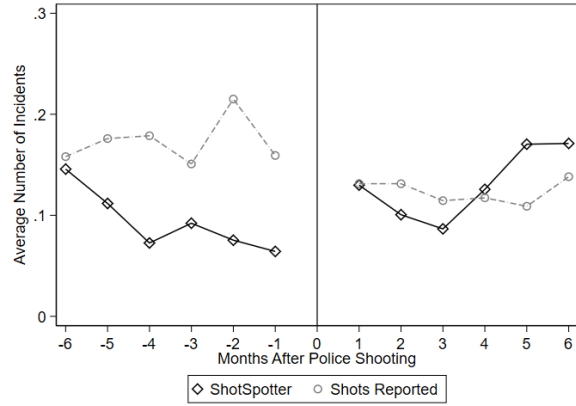
(a) 0.1 miles



(b) 0.2 miles



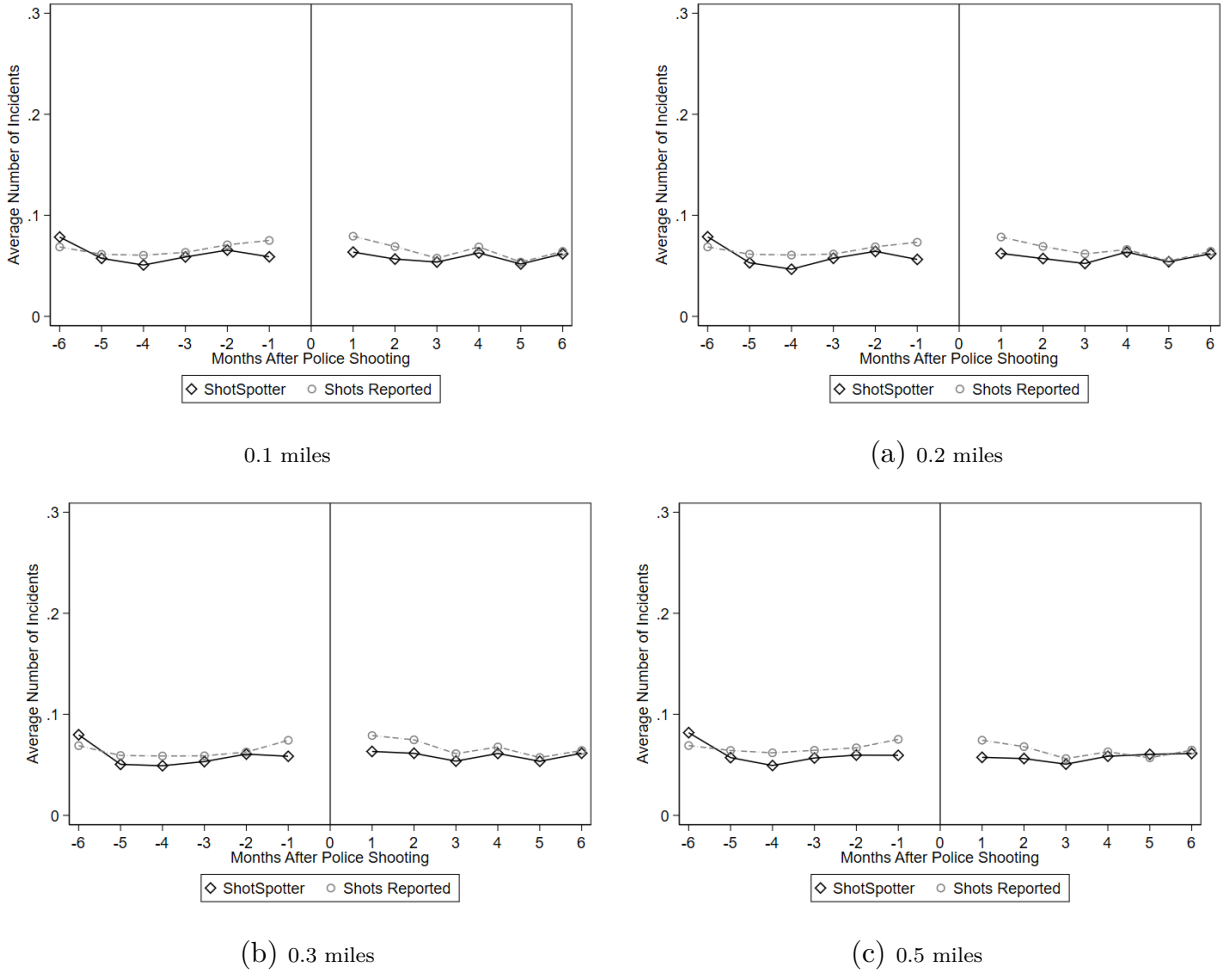
(c) 0.3 miles



(d) 0.5 miles

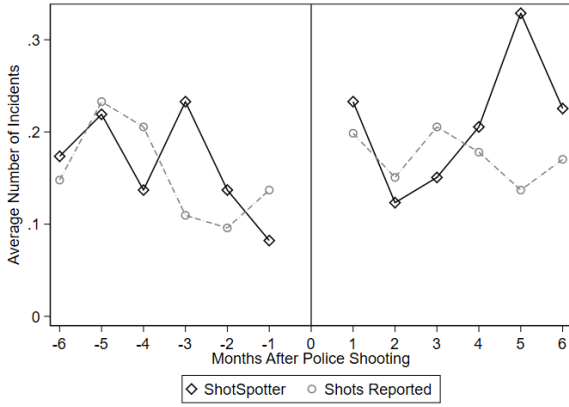
Notes: Each figure represents the average number of shots reported and ShotSpotter incidents over time for treated blocks. Specifically, each point is the one-month average of a given outcome across all treated blocks. In all figures, the x-axis represents the time since a police shooting. Each time period is one-month long, and 0 marks the police shooting date.

Figure A4: ShotSpotter and Shots Reported over Time (Never-treated Blocks)

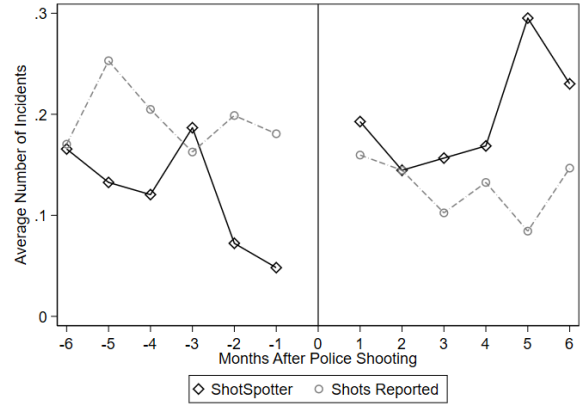


Notes: Each figure represents the average number of shots reported and ShotSpotter incidents over time for never-treated blocks, our common control group, i.e. the blocks that are more than 0.5 miles away from any police shooting. Specifically, each point is the one-month average of a given outcome across all never-treated blocks. In all figures, the x-axis is normalized relative to the treatment time, where 0 marks the time of the treatment, and each period is one month long. To create the time periods, the treatment dates are the dates of the “effective” police-involved shootings at each radius. Note that since this is the common control group, the blocks are the same across all panels, and what changes is the sample of police shootings used as treatment.

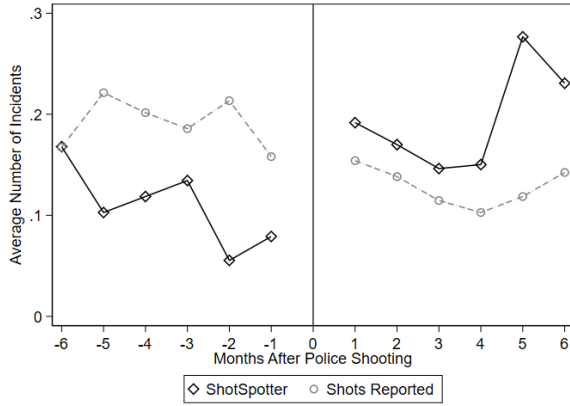
Figure A5: ShotSpotter and Shots Reported over Time (Early-treated Blocks)



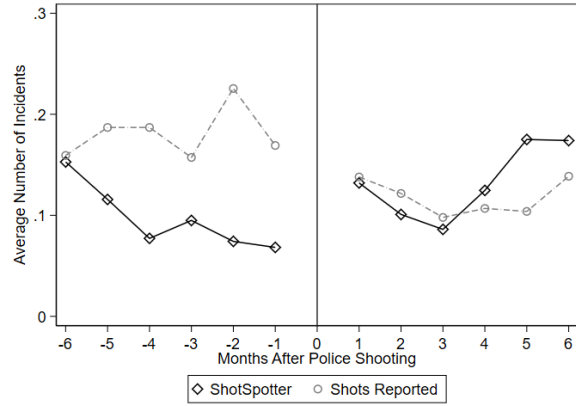
(a) 0.1 miles



(b) 0.2 miles



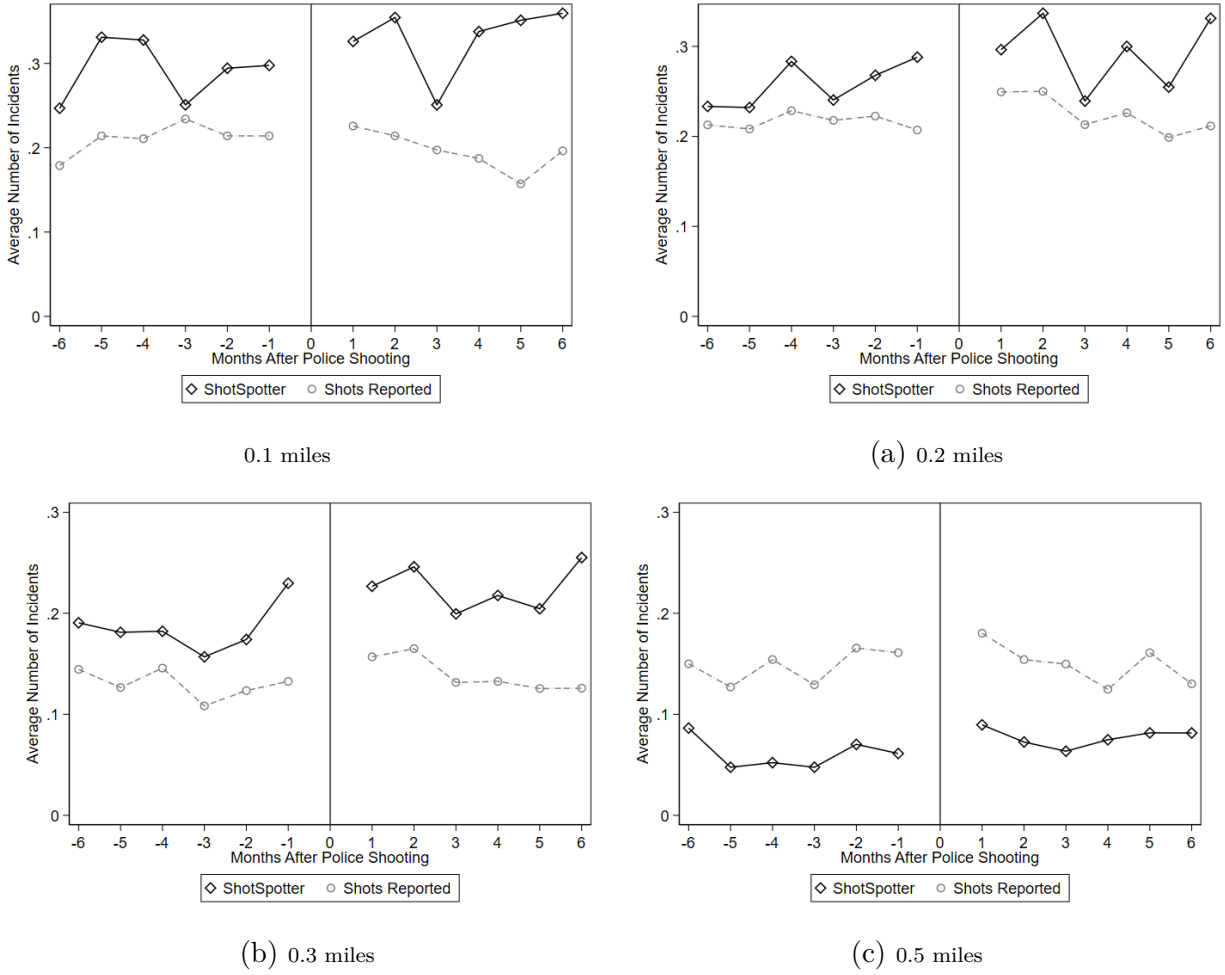
(c) 0.3 miles



(d) 0.5 miles

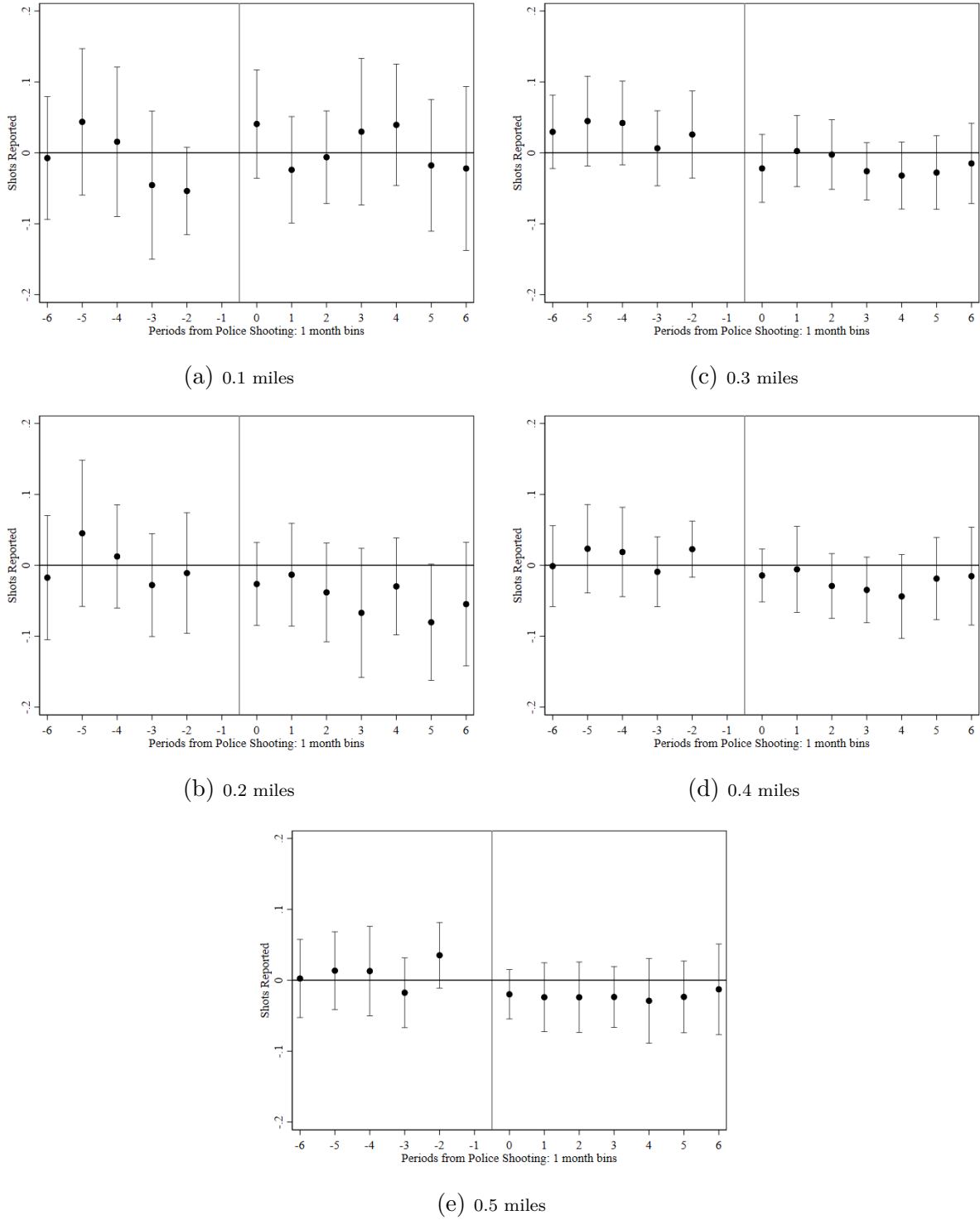
Notes: Each figure represents the average number of shots reported and ShotSpotter incidents over time for early-treated blocks, i.e. blocks that are exposed to a police shooting before 2015. Specifically, each point is the one-month average of a given outcome across all early-treated blocks. The x-axis represents the time since a police shooting, and each time period is one-month long. The vertical line at 0 represents the date of the police shooting.

Figure A6: ShotSpotter and Shots Reported over Time (Later-treated Blocks)



Notes: Each figure represents the average number of shots reported and ShotSpotter incidents over time for later-treated blocks, i.e. blocks that are exposed to a police shooting after 2015. Specifically, each point is the one-month average of a given outcome across all later-treated blocks, which are used as a control group for the early-treated blocks. In all figures, the x-axis is normalized relative to the treatment time, where 0 marks the time of the treatment, and each period is one month long. To create the time periods, the treatment dates are the dates of the police-involved shootings that occurred before 2015.

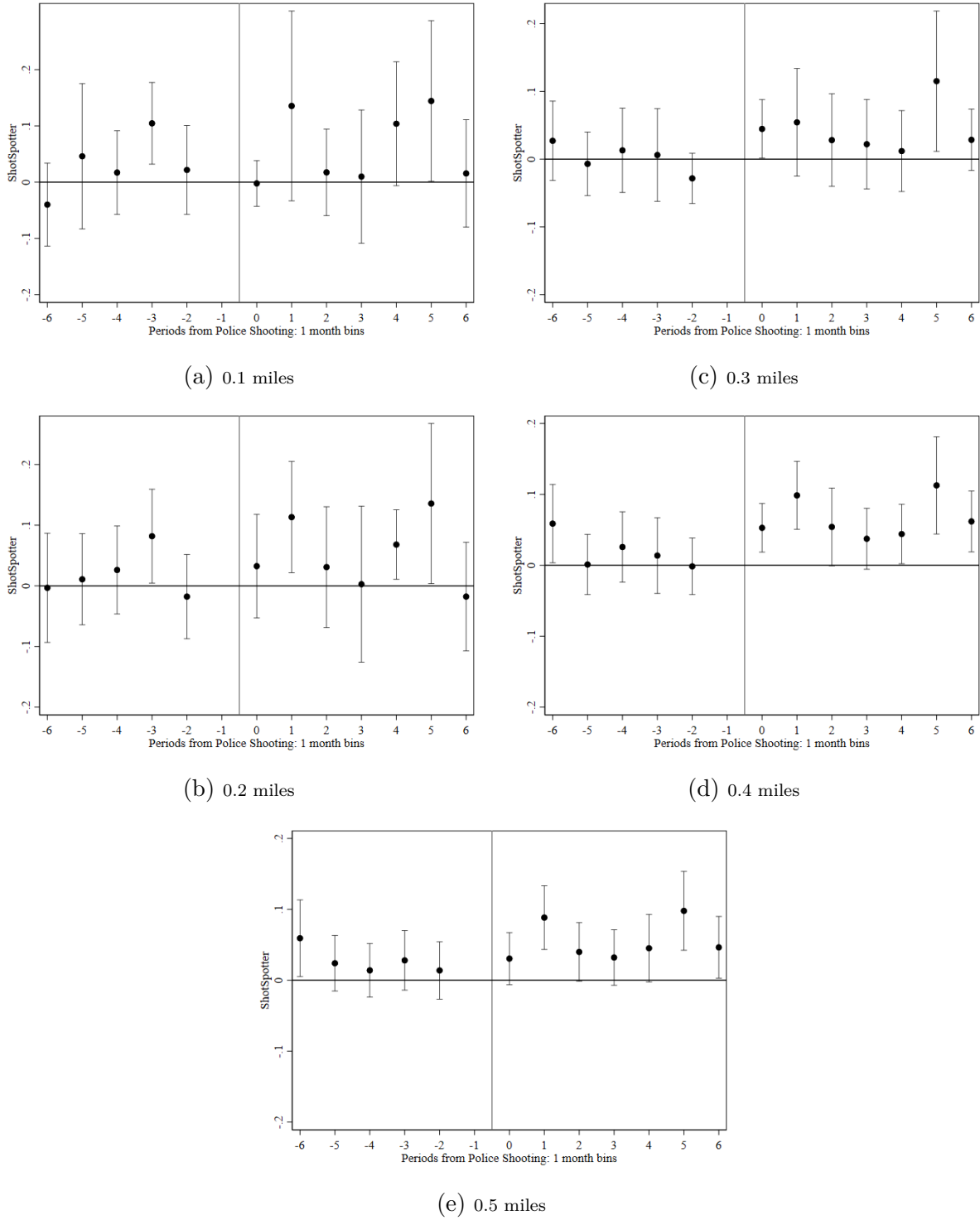
Figure A7: Event-study Analysis of Shots Reported using Callaway and Sant'Anna 2021



Notes: These figures show the average causal estimates and 95 percent confidence intervals estimated using the procedure of Callaway and Sant'Anna 2021, where the outcome is the inverse hyperbolic transformation of shots reported. Each period is one month long. We use the never-treated blocks as a control group in each estimation.

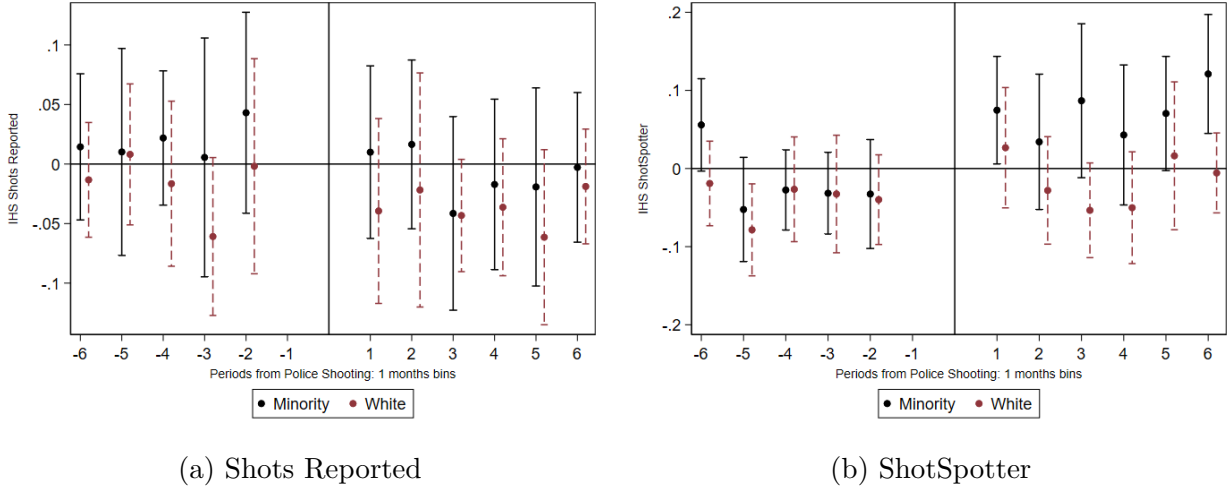


Figure A8: Event-study Analysis of Shots Reported using Callaway and Sant'Anna 2021



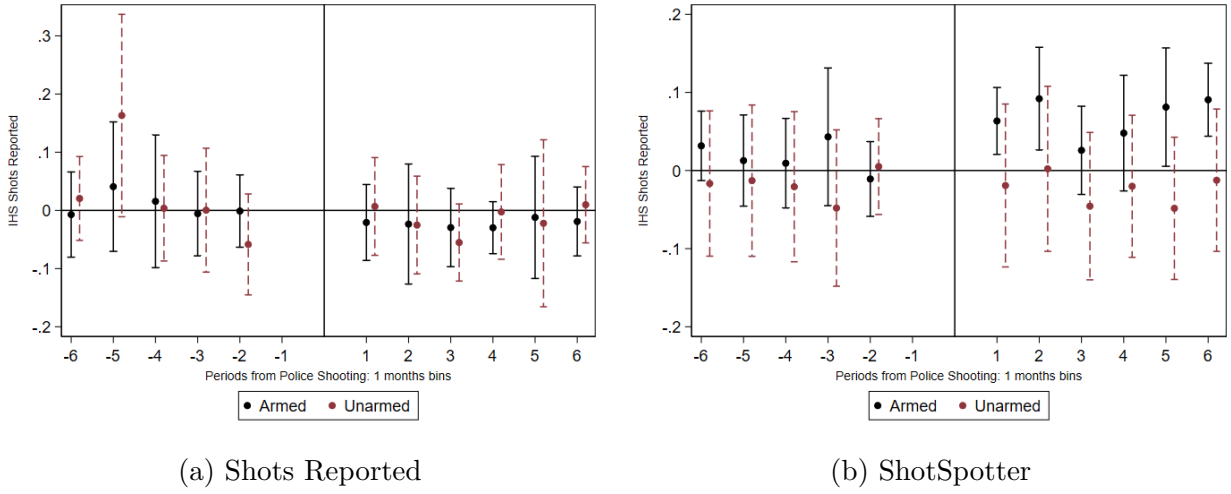
Notes: These figures show the average causal estimates and 95 percent confidence intervals estimated using the procedure of Callaway and Sant'Anna 2021, where the outcome is the inverse hyperbolic transformation of ShotSpotter incidents. Each period is one month long. We use the never-treated blocks as a control group in each estimation.

Figure A9: Effect of a Police Shooting by Race



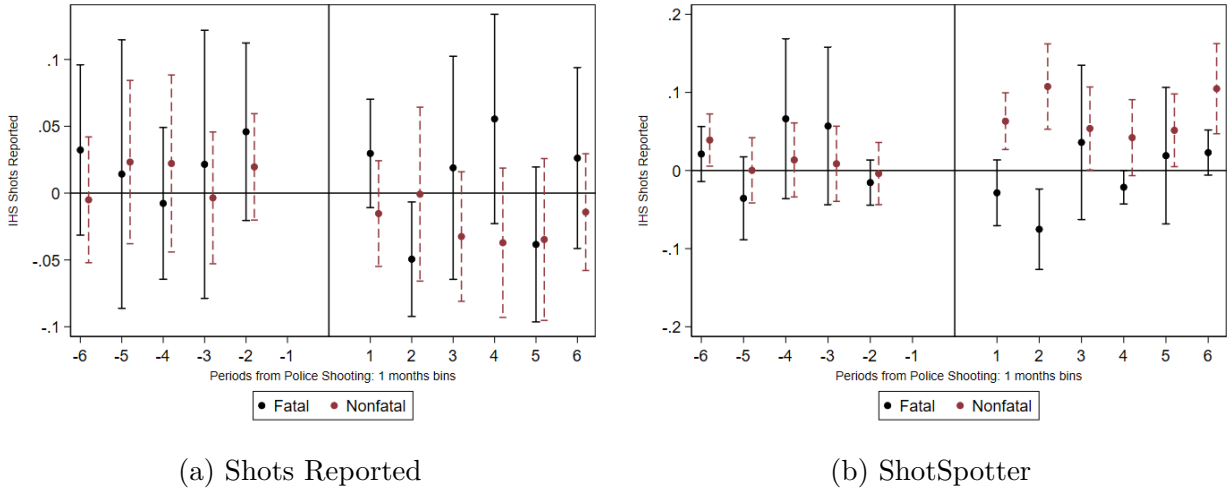
Notes: This figure shows the estimated coefficients and 95 percent confidence intervals from event study regressions of Equation 2 in Minority neighborhoods and White neighborhoods, separately. A Census block is defined to be White (Minority) if more than 50 percent of its population are White (Minority). Census block fixed effects as well as month-by-year fixed effects are included. Standard errors are clustered at the tract level. Each period is one month long, and period -1 is excluded.

Figure A10: Effect of a Police Shooting by Victim's Weapon



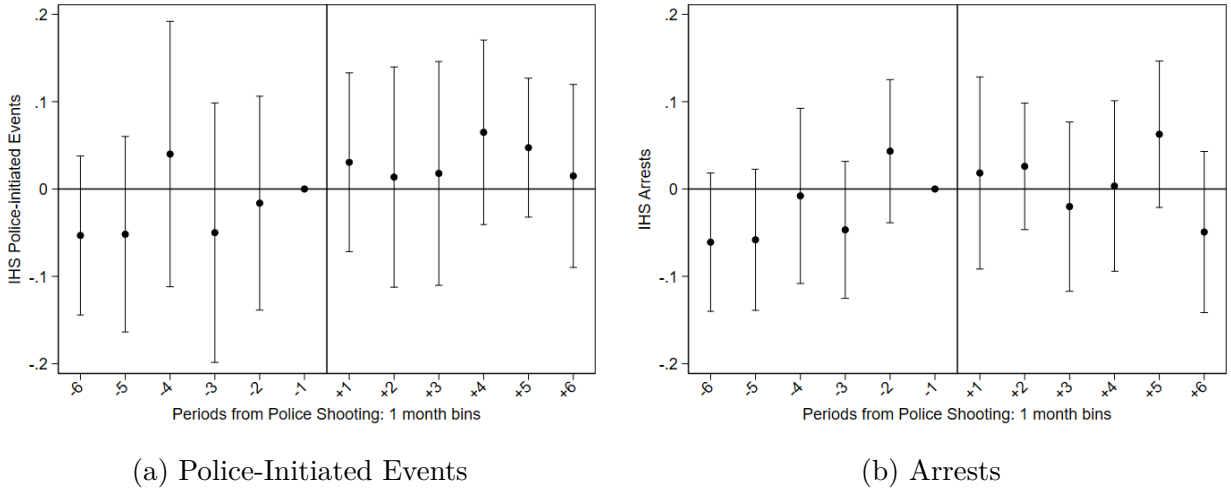
Notes: This figure shows the estimated coefficients and 95 percent confidence intervals from event study regressions that estimate the effect of shooting an armed civilian and an unarmed civilian, separately. We exclude the shootings where the information about the civilian's weapon is missing, along with the blocks associated with them. Census block fixed effects as well as month-by-year fixed effects are included. Standard errors are clustered at the tract level. Each period is one month long, and period -1 is excluded.

Figure A11: Effect of a Police Shooting by Fatality of Shooting



Notes: This figure shows the estimated coefficients and 95 percent confidence intervals from event study regressions that estimate the effect of shooting fatal shooting and a nonfatal shooting, separately. Census block fixed effects as well as month-by-year fixed effects are included. Standard errors are clustered at the tract level. Each period is one month long, and period -1 is excluded.

Figure A12: Effect of a Police Shooting in Police Activity



Notes: These figures show dynamic difference-in-differences estimates using Equation 2 using the 0.4 miles radius, where the outcomes are the inverse-hyperbolic transformation of police-initiated calls and arrests. Each period is one month long, and period -1 is excluded. Census block fixed effects as well as month-by-year fixed effects are included. Standard errors are clustered at the tract level.