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 while exposure to a police shooting increases gun-related crimes by 3-6 percent, it has no effect on shots reported. Taken together, this implies police shootings reduce civilian crime reports to police by 4-6 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 are those in other advanced industrial countries (Edwards et al. 2019). This has led to adverse consequences on institutional trust. In 2019, only 55% 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, as 44 percent of the voters were in favor (Kaste 2021). This reflects 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 Schargrodsky 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, interpreting a change in the volume of 911 calls as a change in the behavior of crime reporting can lead to false conclusions.

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. By using the addresses of these incidents, we locate them in Census blocks in the city. In our sample period, Minneapolis experienced 57 unique police-involved shootings, most of which involved a Black individual (71 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 3-6 percent increase in gun violence in exposed blocks relative to unexposed blocks. However, there is no significant effect on shots reported. We conclude that police shootings cause a 4-6 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 across all estimators.

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. Specifically, we estimate a 6 percent increase in ShotSpotter incidents in Minority neighborhoods, while the effect in White neighborhoods is statistically insignificant. In both types of neighborhoods, we estimate a non-significant effect on shots reported. This suggests that the increase in gun-related crimes and the decrease in the reporting rate is entirely driven by Minority neighborhoods. Additionally, we show heterogeneity in the effects depending on whether the shooting was fatal or not. 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. This suggests that exposed blocks become more dangerous compared to unexposed ones for at least four years, and that the decrease in civilians' willingness to report gun shots as a result of a police shooting may well so be permanent.

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. 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 a recent working paper by Ang, Bencsik, Bruhn and Derenoncourt 2021. In that study, the authors construct the ratio of shots reported to shots fired using ShotSpotter data and

911 calls to study the effect of George Floyd's murder on crime reporting.

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 police shootings and killings of 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, including the fatal ones. 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 for 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 American Community Survey (ACS) for further analyses. As previously discussed, we focus on shooting crimes in order to distinguish between actual crime incidence and its reporting.

2.1 Police-involved shootings

In order to identify treated blocks, we rely on administrative data of police-involved shootings between 2009 and 2019 in Minneapolis, obtained from the city's open data webpage[1]. The data document incidents where an officer was involved in any shooting, whether fatal or not, and include information about the date and the time of the 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.[2] 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 A1. Column (1) shows that victims of police shootings are less likely to be female (11 percent) and White (11 percent). In line with national statistics, the majority of civilians involved are Black (71 percent).

[1] https://opendata.minneapolismn.gov

[2] This information is missing for 15 shootings.

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, we supplement our main dataset with data from Fatal Encounters[3], 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, we identify only one missing fatal 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, 26 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[4], a publicly available archive of news articles, and Google Trends.[5] 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.[6] 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 given to police shootings relative to that of George Floyd, and we mark the dates of the actual shootings on the graphs (Figure A6). As can be seen from these graphs, the increase in both news coverage, as measured by the number of articles that mention "George Floyd," and search volume for "George Floyd" after the incident, is significantly greater compared to the news

[3]Fatal Encounters Database. https://fatalencounters.org/. Retrieved on December 12, 2022.
[4]LexisNexis. http://academic.lexisnexis.com. Retrieved on Jan 12, 2023.
[5]Google Trends. https://www.google.com/trends. Retrieved on January 12, 2023.
[6]The data are available at the state-level only.

coverage and search volume of police shootings after these incidents in Minneapolis. This is also true for the Google search volume of victims' names after fatal police shootings.

2.2 Exposure to Police 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 within "r" miles from a police shooting as treated.[7] We use five different radii to define treatment and later report the results using all the five definitions of treatment as explained in Section 3. Some Census blocks can be exposed to more than one police shooting over the sample period. In these cases, we consider the date of the first shooting as the treatment date.

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. To avoid bias arising from including always-treated blocks, we balance the sample by restricting the shootings to those that happened between 2010 and 2018. This allows us to observe at least one year before and after treatment for all the treated blocks. We thus include 23 police shootings in our main analysis.[8]

We report the characteristics of the shootings in Table A1. Besides the characteristics of the shootings in the full sample discussed previously, we report the characteristics of the shootings in our balanced sample (at the 0.4 miles radius), and we compare them to the characteristics of the shootings that occur in 2009 and 2019, i.e. the excluded ones. We report the difference along with its standard error in column (4). As shown in the table, excluded shootings are not statistically different from those that are included.

[7]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.

[8]Given that we consider the first shooting only to determine exposure, the number of shootings used to define treatment may vary slightly depending on the radius used. The minimum number of shootings included is 22 (at a radius of 0.5 miles), and the maximum is 27 (at a radius of 0.3 miles).

2.3 Outcome variables

To measure the number of gun-related crimes, we use publicly available ShotSpotter data from the city's open data website.[9] 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.[10] 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.[11]

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 not able to acquire information about the exact location and the date of installation of ShotSpotter devices in the city. Hence, we only include Census blocks where we observe at least one ShotSpotter activation incident in 2007, 2008, or 2009. 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% of the total number blocks constituting the city (total of 978 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

[9] https://opendata.minneapolismn.gov
[10] https://www.shotspotter.com/technology/
[11] https://www.shotspotter.com/company/

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 sounds of gunshots for our main outcome variable. Finally, we use the officer-initiated calls, in addition to arrest data, in order to examine whether police activity changes in treated blocks after a shooting.

We collapse the data at the month and block level and focus on two main outcomes: monthly ShotSpotter detected gunshots, and monthly gunshots reported through 911 calls. Our outcome variables show great variation across months and blocks, and many blocks report 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.[12]

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 (Table 1). 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.[13] On average, there are 5 police-initiated calls and 1 arrest in a given

[12]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.

[13]To compute the reporting rate, we divide the number of shots reported by civilians by the number of shooting incidents detected by ShotSpotter for each block-per-month. This computation is only possible for block-month observations with nonzero ShotSpotter incidents. Therefore, conditional on a ShotSpotter incident, civilians only report one out of five shootings. Given that the means of the variables are similar, this means that there may be cases where civilians report a shooting that is not captured by ShotSpotter, which indicates measurement error. ShotSpotter may report incidents that are not shootings and may miss others. 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. Given that ShotSpotter

block per month.

Lastly, we use data from the American Community Survey (ACS) to examine heterogeneity across Census blocks. Table 1 shows that on average, treated blocks have a higher percentage of Black population compared to non-treated ones (36.5 percent compared to 29.5 percent), and that the share of Hispanic population is relatively low across blocks (7.3 percent Hispanic in the entire sample).

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.

Figure A1, in Appendix A.2, shows the geographical distribution of all the police shootings that occurred between 2009 and 2019 (highlighted in red), in addition to the ones that we include in our main analysis, i.e. those that occurred between 2010 and 2018 (highlighted in green). As mentioned in subsection 2.1, we focus on shootings that occurred between 2010 and 2018 in order to ensure that we observe at least 1 year of pre and post periods for all treated blocks. Thus, blocks that were treated before 2010 or after 2018 are completely excluded from our sample. We define exposure by the distance from a shooting. Since it is not clear what the optimal distance is, we use multiple definitions of treatment. Beginning with a 0.1 miles distance, we define blocks that fall within that radius as treated. We use four other distances, the largest of which is 0.5 miles. We then compare blocks that are

incidents constitute a left-hand side variable, measurement error does not seem so problematic to us. We further discuss the concern of measurement error in section 3. 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.

within "r" miles from these events to those that are not, before and after a police shooting. Specifically, we estimate the following model:

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

where Y_{bt} is the inverse hyperbolic transformation of shots reported or ShotSpotter incidents in block b at month t. $Treat_b \times Post_t$ is the treatment variable that takes the value one for treated blocks after their exposure to a police shooting. The coefficient β_1 measures the change in ShotSpotter and shots reported after a police shooting in exposed blocks, relative to that change in unexposed blocks. 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:

$$Y_{bt} = \alpha_0 + \sum_{t=-6}^{6} \gamma_t Treat_b \times MonthsYr_t + Month \times Year_t + Block_b + \epsilon_{bt} \tag{2}$$

where $MonthsYr_t$ are indicator variables for every month-year period before and after a police shooting. Including block and month-by-year fixed effects, the coefficients γ_t represent the effect over one-month bins.[14] 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.

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. Studying the effect of violent crime on outcomes of public schools in Chicago, Casey et

[14] We exclude the month right before the shooting happens (t = -1) from the analysis.

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. Since we focus on less publicized police shootings, our analysis relies on the assumption that Census blocks that are beyond "r" miles from police shootings are not treated. To the extent that this assumption is not met, it does not pause a threat to the validity of our results. However, it does lead our estimates to be attenuated.

In order to address this concern, 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. the control group when using the 0.5 miles radius. 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. 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. Since we argue that the effects are localized, we expect to see larger effects for smaller radii using this approach.

Our two-way fixed effects estimator from equation (1) relies on several assumptions that might not hold in our setting. Particularly, β_1 represents the weighted average of all possible 2*2 comparisons between blocks in the sample. Since police 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 (Goodman-Bacon 2021; De Chaisemartin and d'Haultfoeuille 2020; Sun and Abraham 2021), this requires 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 the 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 can be invalid. 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.[15]

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 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.

Although ShotSpotter data provide the peculiar advantage of observing a true measure of gunshots, it has some limitations. First, there aren't many studies that have tested the accuracy of ShotSpotter devices in detecting gunshots. One study by the National Institute of Justice showed that almost 99.6 percent of gunshots were detected by the device in 2006 (Goode 2012). More recently, a report by the Office of Inspector General in the city of

[15]There is evidence that violent crimes such as murder, assault and robbery are more likely to happen at night (Doleac and Sanders 2015).

Chicago shows only 9.1 percent of ShotSpotter activation incidents that occurred between January 1, 2020 and May 31, 2021 resulted in a gun-related offense in Chicago (Ferguson and Witzburg 2021). Following Carr and Doleac 2018, we drop the dates where the likelihood of a false ShotSpotter activation is the highest. These include New Year's Eve and the Fourth of July.

This measurement error could cause our estimates to be biased if the probability of a "false" ShotSpotter activation is correlated with treatment. 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, using the algorithm explained in subsection 2.3.

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 as a function of ShotSpotter incidents (SS) and the willingness to report (WTR) as:

$$SR_{bt} = WTR_{bt} \times SS_{bt} \tag{3}$$

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, α, 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 β^{SS} is the effect of a police shooting on ShotSpotter incidents and β^{SR} is the effect of a police shooting on shots reported.

4 Results

We start by plotting the raw averages of the outcome variables over time for the exposed blocks. Results for ShotSpotter incidents and shots reported are shown in Figure 1 for all radii. Specifically, each figure shows average ShotSpotter and shots reported with respect to the treatment date. Each data point is the one-month average of a given outcome across all treated blocks, and each time period is one-month long. Even though the graphs are noisy when looking at the smaller radii, the figures show that there is an increase in ShotSpotter incidents after exposure. Depending on the radius, shots reported either do not change or slightly decrease after t=0.

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, which holds true for four out of the five radii considered. Third, the change in shots reported following a police shooting does not mirror the increase in ShotSpotter incidents. Next, we turn to estimating the dynamic differences-in-differences model, where we control for month-by-year fixed effects and block fixed effects.

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 (t=-1). As previously mentioned, we include month-by-year fixed effects, block fixed effects and cluster our standard errors at the tract level to account for any correlations across adjacent blocks.

First, all five panels show that there is no evidence of pre-trends for both outcomes. 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, except for the 0.1 miles radius, there is an increase in ShotSpotter incidents after a police shooting, while shots reported slightly decrease over time or remain unchanged. We hypothesize that when using a small radius such as the 0.1 miles, the control group may contain blocks that are potentially exposed and are thus "contaminated." The fact that the increase in ShotSpotter activations becomes more pronounced as the treatment radius increases supports this hypothesis. A more detailed discussion of this follows in subsection 4.2.

4.2 Difference-in-differences results

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.[16] 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.

[16] As previously mentioned, we cluster the standard errors at the census tract level in all of our analyses. However, we replicated Table 3 by clustering at the Census block level. The results do not change and are available upon request.

Except when using the 0.1 mile radius, Panel (A) shows no change in shots reported following a police shooting. In contrast, exposed blocks experience an increase in ShotSpotter incidents after a police shooting. This increase ranges from 3 to 6 percent and is statistically significant across the specifications of columns 2 through 5.

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 in 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. For example, the estimate for the 0.4-mile radius indicates that a police shooting causes a 6.3 percent reduction in the reporting rate, which is significant at the 1 percent level (p-value = 0.000827).[17] Importantly, the estimated differences are statistically significant at the 1 and 5 percent levels in columns (2) through (5). The only estimated difference that is not significant at the conventional levels is the smallest radius of 0.1 miles, which has a p-value of 0.595 percent. We conclude that after a police shooting, ShotSpotter activation incidents increase by 3 to 6 percent, and the reporting rate decreases by 4 to 6 percent in exposed blocks relative to unexposed blocks.

Exploring the differential effects by radius, we highlight three main takeaways. First, we estimate the largest effects to be at the 0.4 miles radius, implying that our treatment effects are highly local. Second, the effect decreases slightly at the 0.5 miles radius, although the difference across these two radii is not statistically different. Third, and more importantly, the results using the 0.1 miles radius in column (1) show that there is a significant increase in shots reported and no effect on ShotSpotter incidents. This result could suggest the presence of attenuation bias, which is discussed in Section 3 and will be examined further next.

While the advantage of the approach used in Table 3 is that it examines effects across several possible distances, it also has disadvantages. The primary one is that at smaller

[17]To put this in a better context, we calculate the average reporting rate for the exposed blocks before exposure to be 0.2. This means on average, for every 100 gunshots, 20 of them were being reported by civilians. A 6.3 percent decrease in the reporting rate implies that the reporting rate becomes 0.18, which means 2 more gunshots go unreported (for every 100 gunshots, 18 are reported compared to 20).

radii, estimates are attenuated to the extent that treatment impacts areas outside of that radius. This is illustrated in column (1) of Table 3, as previously mentioned. In order to address this issue, in Table 4, we use Census blocks that are more than 0.5 miles away from a police shooting as the control group (i.e., the same as the definition in Table 3 for a radius of 0.5 miles). As a result, the control group is fixed for all five radii, while we still allow the definition of the treatment group to vary from 0.1 miles to 0.5 miles.

Results from panel A of Table 4 indicate that following a police shooting, there is a one to two percent decline in shots reported by civilians, though the effect is inconsistent across the different radii. In contrast, there is clear and compelling evidence across all five radii of an increase in ShotSpotter incidents following a police shooting (Panel B of Table 4). Perhaps unsurprisingly, the effect is largest using the 0.1 mile radius (6.3 percent), and then remains at 5 to 6 percent for the 0.2 to 0.4 miles radii, before dropping by around 20 percent to 4.7 percent when using the largest radius of 0.5 miles. All five estimates are statistically significant at the five percent level.

The results in this section indicate that after a police shooting, ShotSpotter incidents increase by 3 to 6 percent, while the number of shots reported remain unchanged. Taken together, this suggests that a police shooting causes a 4 to 6 percent decrease in the reporting rate. Additionally, the effects are highly localized, with the effects begin dissipating beyond the 0.4 miles radius.

4.3 Alternative estimators

As previously discussed, the two-way fixed effects difference-in-difference estimator may be biased when there are 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 issues arising from heterogeneous effects in our setting, we present estimates of average treatment effects using methods developed by Callaway and Sant'Anna

2021 and Sun and Abraham 2021. These estimators have the advantage of excluding always treated units from the analysis and avoiding the use of already treated units as controls for future treated units. They thereby eliminate the source of bias in the two-way fixed effect estimator and are robust to heterogeneous treatment effects. In all specifications, we include month-by-year and block fixed effects and cluster the standard errors at the Census tract level.

Using the Callaway and Sant'Anna 2021 estimation method, we define the control group for later treated blocks as the combination of never treated and not-yet treated blocks. We provide the figures of the plotted estimates 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. As before, we see no evidence of diverging pre-trends for shots reported across both groups, and we see no changes in the number of shots reported after t=0 (Figure A7). As for ShotSpotter incidents, the estimates are noisy for the smaller radii (0.1 and 0.2 miles). However, using the 0.3, 0.4, and 0.5 miles, we again see no evidence of pre-trends across both groups and an increase in ShotSpotter incidents after a police shooting. This is consistent with our results from Equation 2.

Additionally, we report the aggregation of all post-treatment effects – the overall ATT for all groups across all time periods – for each radius in Table A3. These results are consistent with our main findings and are larger in magnitude. Specifically, we estimate a 6 to 9 percent increase in ShotSpotter incidents in exposed blocks following a police shooting. This translates into a 6 to 10 percent decrease in the reporting rate that is statistically significant for columns 2 through 5.

In a second check, we use the Sun and Abraham 2021 estimation method, which relies on the never treated and last treated blocks as the control group. Table A4 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, that leads to a 3 to 6 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 police shootings, treated blocks experience an increase in gunshot crimes, which persists for at least 6 months after exposure (as shown in the event study graphs of Figure 2). However, it is unclear whether these changes persist beyond six months after a police shooting. To investigate this, we focus on the early shootings in our sample period. Specifically, we restrict our sample to shootings that occurred before 2015, to observe the outcome variables for at least five years after a police shooting. Importantly, we exclude blocks that were treated during or after 2015 from the analysis.

We then estimate the dynamic difference-in-differences effects using Equation 2 and the 0.4 miles radius to define treatment, and we report the results in Figure 3. As seen in panel (a), there as an immediate and statistically significant increase in ShotSpotter incidents following a police shooting, and this increase persists for up to four years after exposure. Conversely, panel (b) indicates no significant change in shots reported following exposure to a police shooting. This suggests that the effect of police shootings on gun violence persists for at least four years, and may well be permanent.

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, and changing the sample used in the analysis . All of the results reported in this section use the 0.4 miles radius.

As mentioned in section 3, we limit our sample to shootings that occurred between 2010 and 2018 to maximize the number of shootings included and to ensure that there's at least one year of pre- and post-periods for all blocks in the sample. However, this sample selection might threaten the validity of our results if the included shootings are different in nature from excluded ones, or if the results are sensitive to the length of the pre- or post-period. For instance, it is possible that the shootings that occurred in 2019 have a greater impact on civilians compared to the included ones.

To address this concern, first, we report the characteristics for the 2010-2018 shootings (column 2) and compare them to the characteristics of the excluded ones (column 3) in Table A1. We test whether the characteristics are statistically different across these groups and report the results in column (4). As can be seen, none of the characteristics are statistically different across these groups at conventional levels.

Second, 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 pre- and post periods by 6 months on each side of the cutoff until reaching four years.[18]

We estimate Equation 1 for each length and outcome, separately, and present the difference-in-differences coefficients with their 95% confidence intervals in Figure 4. As shown in the figure, the effect of a police shooting on ShotSpotter incidents is robust across different lengths of pre- and post-periods. Specifically, the magnitude of the coefficient varies between 5 percent and 10 percent, but the estimates are not statistically distinguishable from each other. The coefficient for shots reported is either statistically insignificant or marginally negative.

Another potential threat to our identification is the differential time trends across treatment and control blocks, which is especially salient in our analysis given that our sample

[18] 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.

period covers ten years. To account for differential time trends at finer geographies, we include tract-by-month fixed effects. Column (1) of Table A2 shows that our results remain unchanged in magnitude and significance when we control for tract-by-month fixed effects. Specifically, we estimate a 6 percent increase and a 6 percent decrease in ShotSpotter and the reporting rate, respectively. Both estimates are significant at the 1 percent level.

As previously discussed, our sample blocks are blocks that experienced at least one ShotSpotter activation between 2007 and 2009 to ensure that ShotSpotter devices were installed in all blocks throughout the 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 an 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 against this possibility, due to the immediate changes in ShotSpotter incidents after a police shooting as seen in the event study graphs (Figure 2, Figure 3). Particularly for radii greater 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 A2 shows that within two months of a police shooting, exposed blocks experience a 4 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 5 percent decrease in the reporting rate in the short run.

Finally, 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.[19] 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 A2 shows that our results remain unchanged when we restrict our sample to "high crime" areas. Specifically, we estimate a statistically significant 6 percent increase in ShotSpotter incidents, and an insignificant effect on shots reported. As before, this implies that the reporting rate decreases by 7 percent after a police shooting.

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 (10 percent), indicating that the reporting rate decreases by 12 percent within two months after a police shooting in "high crime" areas.

The results in this section demonstrate that our estimates are robust to variations in pre- and post-period length, the shootings used as treatment, controlling for differential time trends across geographical areas, and the sample of blocks included in the analysis. 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. We report the event study graphs for

[19] The 50^{th} percentile of ShotSpotter incidents between 2009 and 2010 is 1 per Census block.

each heterogeneity analysis in the appendix (Figure A3, Figure A4 and Figure A5).

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 ACS Census data, we define a Census block as White (Minority) if more than 50 percent of its population is White (Minority).[20] We estimate the effect of a police shooting on White vs Minority neighborhoods separately, by comparing exposed White (Minority) blocks to unexposed White (Minority) blocks using the 0.4 miles radius. 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% Minority civilians.

We report the results for Minority and White neighborhoods in Table 5, 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, but statistically insignificant in both neighborhoods. However, Panel B shows that there is a 6.4 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 = 0.0418). Thus, Minority neighborhoods exhibit a decrease in the reporting rate of 7.8 percent following exposure to police shootings. This can also be seen in the event study graphs (Figure A3), where both White and Minority neighborhoods exhibit no changes in shots reported after a police shooting (panel a), while ShotSpotter incidents diverge across these two groups in the post period, especially after three months of exposure.

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

[20] A block is considered a Minority block if more than 50% of its population is Black or Hispanic.

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 whether the civilian is armed or not does not impact the response to a police shooting. It should be noted that information on the civilians' weapon is missing for 15 shootings in the sample, which means that more information is needed to better understand whether certain characteristics of police shooting victims matter.

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 6 percent increase in ShotSpotter incidents and have no effect on shots reported. Taken together, this implies that nonfatal shootings cause a 6 percent decrease in the reporting rate as well. As can be seen in Figure A5, both fatal and nonfatal shootings have no effect on shots reported. However, ShotSpotter incidents increase beginning the first month after a police shooting, in contrast to fatal police shootings. While one might worry that fatal police incidents are underreported, which could impact the results in columns (5) and (6), 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.

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 nonfatal shootings have a larger effect when compared to nonfatal 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 explanations is the "Ferguson Effect," where police officers reduce their effort in patrolling or deterring crime to avoid further public scrutiny, especially following highly publicized incidents (Cheng and Long 2022; Premkumar 2021).

Although our sample of shootings consists of less publicized incidents (Figure A6), 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.[21] 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 6. Column (1) shows that following a police shooting, treated blocks experience a marginally significant six percent increase in police calls, while column (2) indicates no change in the arrests.

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 A2 show no evidence of pre-trends, implying that police activity was uniform across treated and control blocks before treatment. Following exposure, there is no change in police calls or arrests in treated blocks relative to control blocks.

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

[21]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 2009 and 2019 from the Minneapolis Police Department.

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 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, and this is not due to underreporting of fatal encounters with the police. 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.

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Tables and Figures

Table 1: Summary Statistics I

	(1) Entire Sample	(2) ≤0.4 miles	(3) >0.4 miles
Block Characteristics			
Percent White	33.42	32.25	34.62
	(21.50)	(20.35)	(22.55)
Percent Black	33.05	36.55	29.47
	(20.59)	(21.76)	(18.64)
Percent Hispanic	7.371	6.364	8.403
	(8.097)	(7.571)	(8.478)
Total Population	118.3	111.9	124.9
	(146.4)	(83.92)	(189.9)
Outcomes			
ShotSpotter	0.150	0.192	0.108
	(0.472)	(0.538)	(0.388)
Shots Reported	0.132	0.159	0.104
	(0.429)	(0.472)	(0.376)
Police-initiated Calls	4.628	4.187	5.079
	(10.10)	(8.955)	(11.13)
Arrests	0.910	0.862	0.959
	(2.100)	(1.915)	(2.272)
Observations	76428	38676	37752

Standard deviations in parentheses.

Notes: This table shows the summary statistics for 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 0.4 miles away from any police shooting (control blocks). 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
Panel A: Day Time					
After a Police Shooting	0.0104	-0.00291	0.00845	0.0133	0.0203
	(0.036)	(0.019)	(0.017)	(0.016)	(0.021)
Outcome Mean	0.189	0.189	0.189	0.189	0.189
Panel B: Weekend					
After a Police Shooting	-0.00916	0.00367	-0.0212	-0.0242	-0.00772
	(0.020)	(0.020)	(0.021)	(0.020)	(0.016)
Outcome Mean	0.372	0.372	0.372	0.372	0.372
Observations	21693	21693	21693	21693	21693

Standard errors in parentheses.

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

Notes: This table presents the effect of a police shooting on ShotSpotter incidents' characteristics. 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
Panel A: Shots Reported					
After a Police Shooting	0.0179**	-0.00968	-0.00280	-0.00695	-0.0104
	(0.008)	(0.007)	(0.007)	(0.006)	(0.009)
Observations	122892	113388	103620	90420	80520
Panel B: ShotSpotter					
After a Police Shooting	0.0307	0.0342**	0.0457***	0.0563***	0.0470**
	(0.023)	(0.016)	(0.013)	(0.018)	(0.022)
Observations	122892	113388	103620	90420	80520
β^{SR} - β^{SS}	-0.0128	-0.0439	-0.0485	-0.0632	-0.0573
	(0.0241)	(0.0176)	(0.0147)	(0.0189)	(0.0237)
P-value	0.595	0.0125	0.000955	0.000827	0.0157

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. In all five columns, we restrict police shootings to those that occur between 2010 and 2018 in order to observe at least 1 year of pre- and post-periods for all blocks in the sample. 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: β_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. It is important to note that the number of shootings varies slightly across radii: there are 25 police shootings when using the 0.1 and the 0.2 miles radii, 27 shootings when using the 0.3 miles, 23 shootings when using the 0.4 miles, and 22 shootings when using the 0.5 miles radius.

Table 4: Difference-in-differences Estimates using a Common Control Group

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
Panel A: Shots Reported					
After a Police Shooting	0.00699	-0.0192**	-0.0118*	-0.0123**	-0.0104
	(0.009)	(0.007)	(0.007)	(0.006)	(0.009)
Observations	44616	59796	71676	77220	80520
Panel B: ShotSpotter					
After a Police Shooting	0.0629**	0.0539***	0.0568***	0.0608***	0.0470**
	(0.024)	(0.018)	(0.015)	(0.020)	(0.022)
Observations	44616	59796	71676	77220	80520
β^{SR} - β^{SS}	-0.0559	-0.0731	-0.0686	-0.0730	-0.0573
	(0.0262)	(0.0196)	(0.0166)	(0.0213)	(0.0237)
P-value	0.0329	0.000195	0.0000348	0.000608	0.0157

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 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: β_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 5: Heterogeneous Effects

	Neighborhood		Subject		Shooting	
	(1) Minority	(2) White	(3) Armed	(4) Unarmed	(5) Fatal	(6) Nonfatal
Panel A: Shots Reported						
After a Police Shooting	-0.0141	-0.00829	-0.0108	-0.00649	0.0000631	-0.00713
	(0.010)	(0.006)	(0.011)	(0.009)	(0.007)	(0.006)
Observations	29700	29172	85800	68376	49896	86856
P-value of difference		0.617		0.757		0.454
Panel B: ShotSpotter						
After a Police Shooting	0.0641***	0.0145	0.0346	-0.0269	-0.0116	0.0617***
	(0.020)	(0.014)	(0.024)	(0.025)	(0.013)	(0.019)
Observations	29700	29172	85800	68376	49896	86856
P-value of difference		0.0418		0.0753		0.00172

Standard errors in parentheses.

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

Notes: This table shows the heterogeneity tests results using police-involved shootings that happened between 2010 and 2018 and 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. 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 the p-values. All regressions include block and month-year fixed effects. Standard errors are clustered at the Census tract level.

Table 6: Effect of a Police Shooting on Police Activity

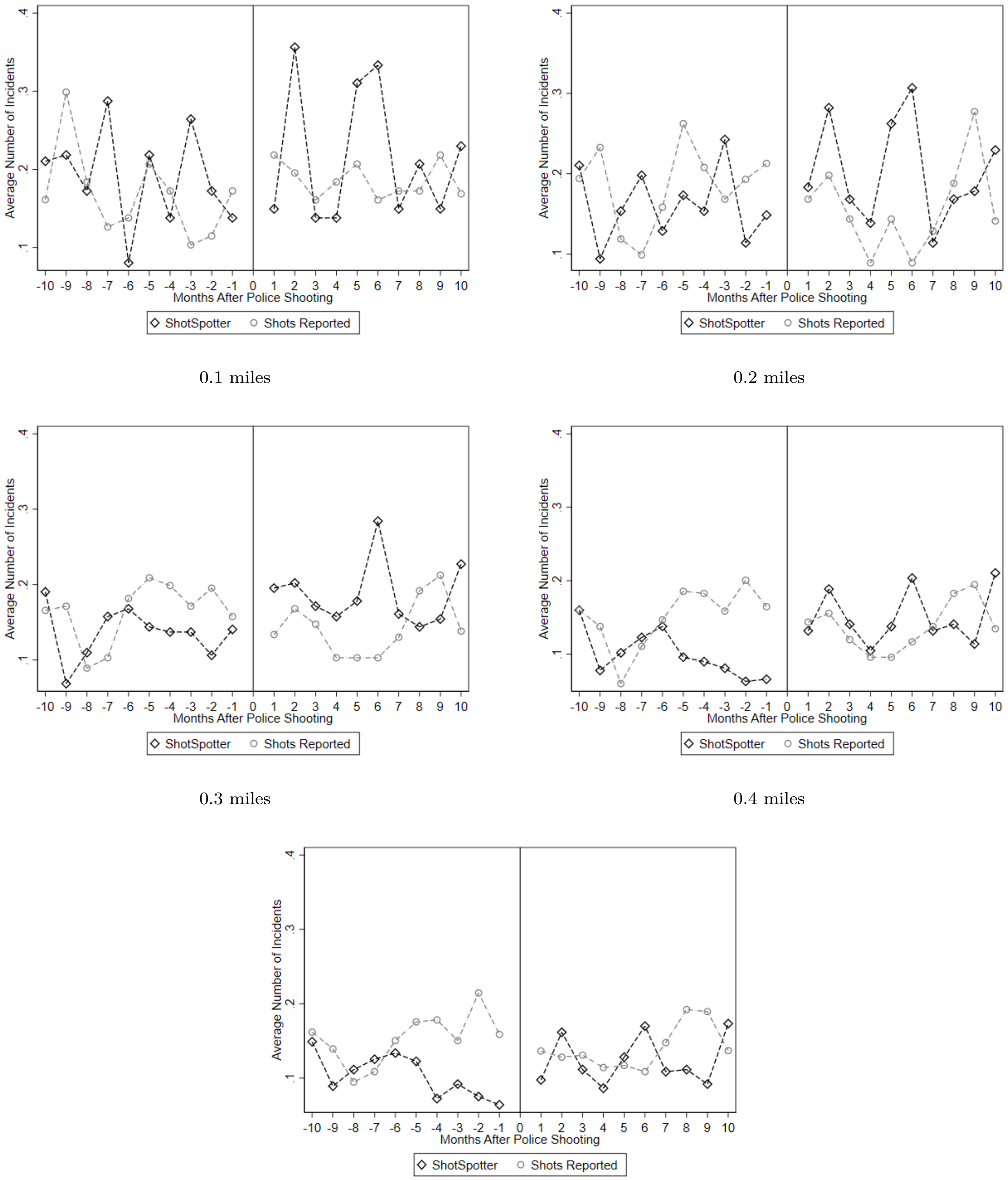
	(1) IHS Police Calls	(2) IHS Arrests
After a Police Shooting	0.0606*	0.0299
	(0.0354)	(0.0230)
Observations	90420	90420

Standard errors in parentheses

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

Notes: This table shows the difference-in-differences effect of a police shooting on 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: Raw ShotSpotter and Shots Reported



0.1 miles

0.2 miles

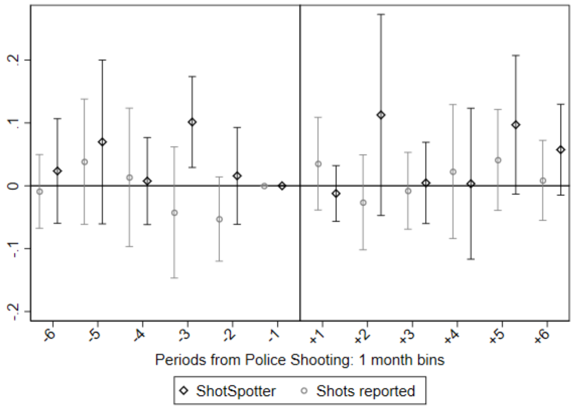
0.3 miles

0.4 miles

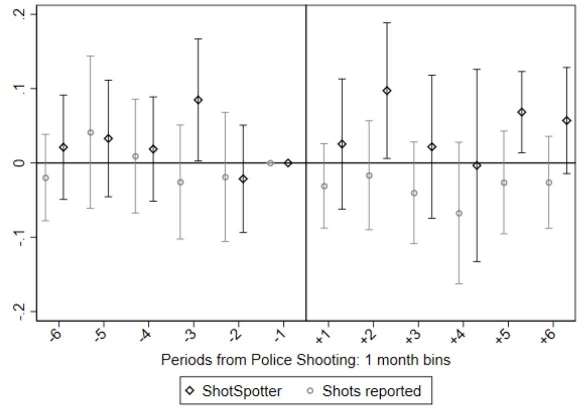
0.5 miles

Notes: Each panel represent the average number of shots reported and ShotSpotter incidents per month for treated blocks over time. Specifically, each point is the one-month average of a given outcome across all treated blocks. The x-axis represents the time since a police shooting, and each time period is one-month long. The vertical line represents the shooting date (t=0).

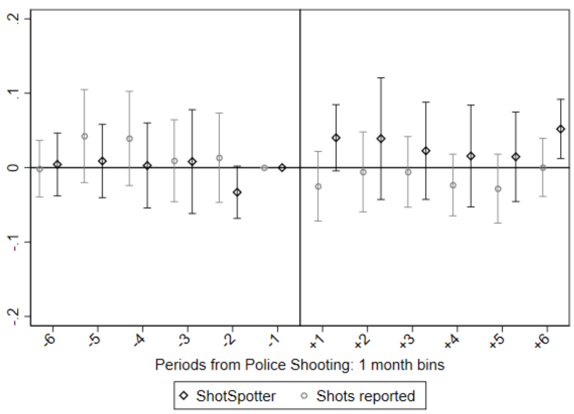
Figure 2: Event-study Analysis



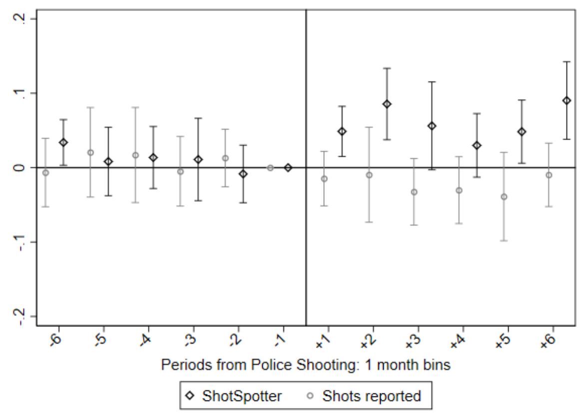
0.1 miles



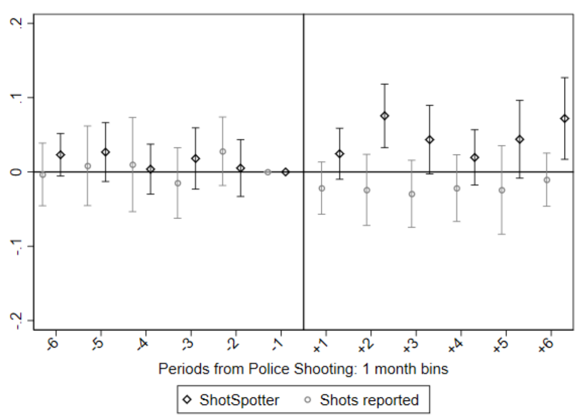
0.2 miles



0.3 miles



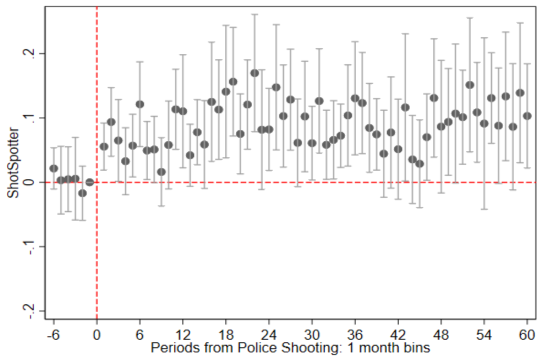
0.4 miles



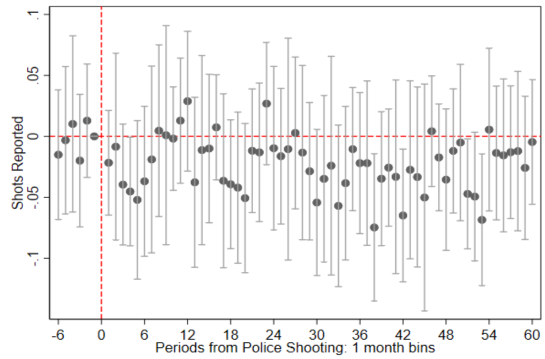
0.5 miles

Notes: These figures show the estimated coefficients and the 95 percent confidence intervals from event study regressions of Equation 2 for all five definitions of treatment, where the outcome is the inverse hyperbolic transformation of ShotSpotter incidents (red) and shots reported (blue). 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 3: Long-run Effects on Shots Reported and ShotSpotter



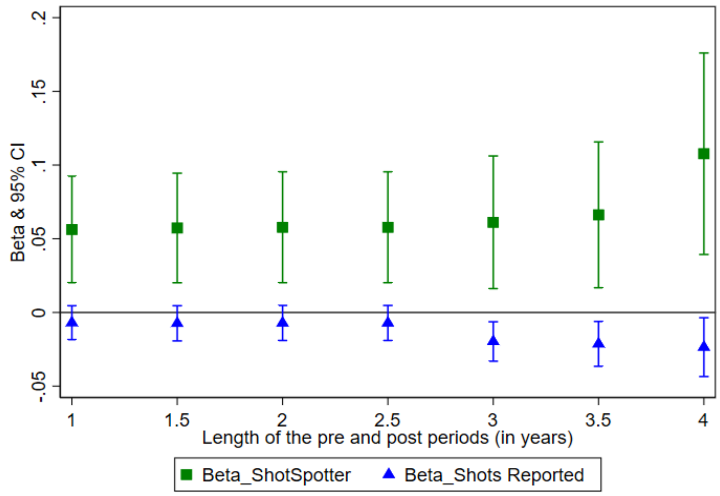
(a) ShotSpotter



(b) Shots Reported

Notes: These figures show the long-run effect of a police shooting on ShotSpotter and shots reported. 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 t=-1 is the omitted period. We control for month-by-year fixed effects and block fixed effects, and we cluster the standard errors at the tract level.

Figure 4: Difference-in-differences Estimates by the Length of Pre- and Post-periods



Notes: This figure represents the coefficients from the difference-in-differences estimates for each outcome, as a function of the length 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 reached 4 years on each side of the cutoff. We estimate the difference-in-differences separately for each length, and we report the coefficient and the 95% confidence intervals in navy for shots reported and green 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 Shootings 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 β^{SR}, β^{SS}, and α 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, β^{SR} would be estimating the effect of exposure to police violence in a given block, b, in the following way:

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

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:

$$SR_{bt} = WTR_{bt} \times SS_{bt} \tag{6}$$

Plugging equation 6 into equation 5, we further derive β^{SR} as follows[22]:

[22]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.

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

In the above equation, terms (b) minus (d) reflect β^{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.[23] In our case, we are able to estimate this portion because of the ShotSpotter data.

Finally, terms (a) minus (c) reflect α, 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:

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

[23]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: Summary Statistics II

	(1) 2009-2019	(2) 2010-2018	(3) 2009 or 2019	(4) Difference
Female	10.71	10.64	11.11	-0.47
	(31.209)	(31.17)	(33.333)	(11.46)
Black	70.91	71.74	66.67	5.07
	(45.837)	(45.52)	(50.000)	(16.85)
White	10.91	10.87	11.11	-0.24
	(31.463)	(31.47)	(33.333)	(11.58)
Hispanic	1.82	2.17	0.00	2.17
	(13.484)	(14.74)	(0.000)	(4.95)
Fatal	26.19	23.53	37.50	-13.97
	(44.500)	(43.06)	(51.755)	(17.57)
Number	57	48	9	39

Standard errors in parentheses.

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

Notes: Column (1) presents the summary statistics for all police-involved shootings that occurred between 2009-2019. Column (2) shows the summary statistics for the shootings that happened between 2010-2018, which are used for the main analysis. Column (3) presents the summary statistics for the shootings that are excluded to balance the samples, i.e. those that occurred in 2009 and 2019. Finally, column (4) calculates the difference between the statistics reported in columns (2) and (3) and reports the standard error of that difference. Sex is missing for one shooting, race is missing for two shootings, and subject weapon is missing for 15 shootings.

Table A2: Robustness Checks

	(1)	(2)	(3)	(4)
Panel A: Shots Reported				
After a Police Shooting	-0.00646	-0.0134	-0.000560	-0.0136
	(0.006)	(0.012)	(0.010)	(0.026)
Observations	90420	18837	22308	5496
Panel B: ShotSpotter				
After a Police Shooting	0.0562***	0.0361**	0.0644**	0.104**
	(0.018)	(0.015)	(0.028)	(0.042)
Observations	90420	18837	22308	5496
β^{SR} - β^{SS}	-0.0627	-0.0495	-0.0650	-0.117
	(0.0192)	(0.0196)	(0.0296)	(0.0488)
P-value	0.00107	0.0116	0.0284	0.0162
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 using the 0.4 miles radius to define treatment. It also shows the P-values of the Wald tests, where H_0: β_1^{SS} - $\beta_1^{SR} = 0$. We report results from the balanced sample using the 2010-2018 shootings. 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 2010 and 2011, 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 A3: 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
			Panel A: Shots Reported		
After a Police Shooting	0.032	-0.011	0.010	-0.012	-0.016
	(0.025)	(0.028)	(0.020)	(0.024)	(0.020)
Observations	122892	113388	103620	90420	80520
			Panel B: ShotSpotter		
After a Police Shooting	0.062*	0.087***	0.074***	0.091***	0.083***
	(0.037)	(0.025)	(0.024)	(0.028)	(0.030)
Observations	122892	113388	103620	90420	80520
β^{SR} - β^{SS}	-0.030	-0.098	-0.064	-0.103	-0.099
	(0.045)	(0.037)	(0.031)	(0.037)	(0.036)
P-value	0.505	0.008	0.041	0.005	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. In all five columns, we restrict police shootings to those that occur between 2010 and 2018 in order to observe at least 1 year of pre- and post-periods for all blocks in the sample. 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: β_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, Callaway and Sant'Anna (2021).

Table A4: Difference-in-Difference Effects using Sun and Abraham 2021

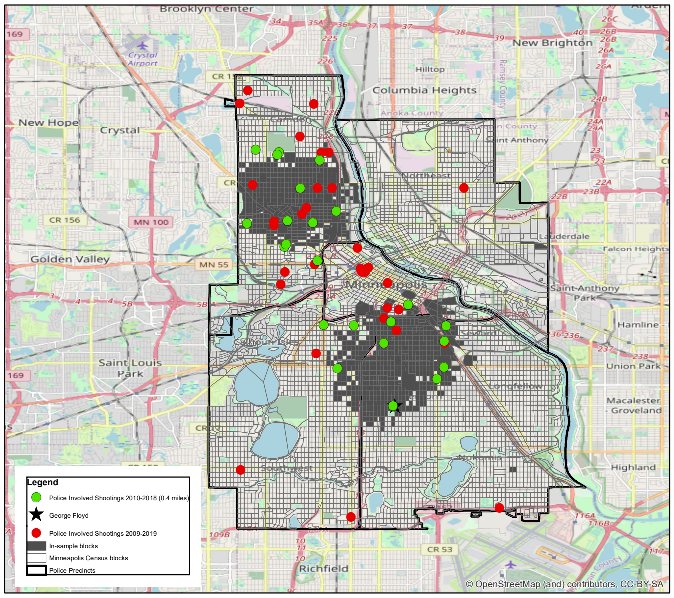
	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
			Panel A: Shots Reported		
After a Police Shooting	0.021*	-0.028*	0.002	-0.003	-0.004
	(0.012)	(0.017)	(0.013)	(0.014)	(0.012)
Observations	122892	113388	103620	90420	80520
			Panel B: ShotSpotter		
After a Police Shooting	0.044***	0.036***	0.033***	0.062***	0.047***
	(0.014)	(0.010)	(0.012)	(0.012)	(0.013)
Observations	122892	113388	103620	90420	80520
β^{SR} - β^{SS}	-0.024	-0.064	-0.031	-0.065	-0.051
	(0.018)	(0.019)	(0.017)	(0.018)	(0.018)
P-value	0.194	0.001	0.075	0.000	0.004

Standard errors in parentheses.

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

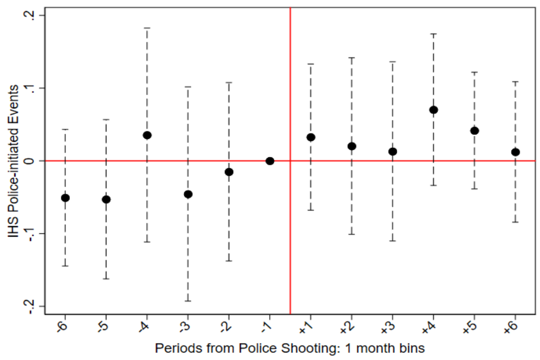
Notes: This table shows the difference-in-differences results using the Sun and Abraham 2021 procedure. We implement the interaction weighted (IW) estimator. In all five columns, we restrict police shootings to those that occur between 2010 and 2018 in order to observe at least 1 year of pre- and post-periods for all blocks in the sample. 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: β_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.

Figure A1: Police-involved Shootings in Minneapolis

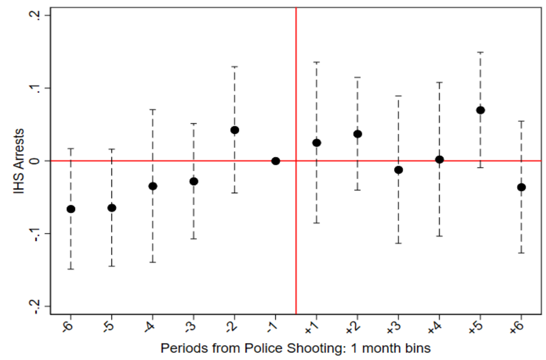


Notes: This map shows the geographical distribution of police-involved shootings in Minneapolis, in addition to the Census blocks we include in our analysis. The thick black lines represent the boundaries of the five police precincts in the city. Additionally, we highlight the blocks that we include in our analysis in grey. These are the blocks that experience at least one ShotSpotter activation incident between 2007 and 2009. The red dots represent all the police shootings that occur between 2009 and 2019, while the green ones are the shootings that we include in our analysis, using the 0.4 miles radius.

Figure A2: Effect of a Police Shooting in Police Activity



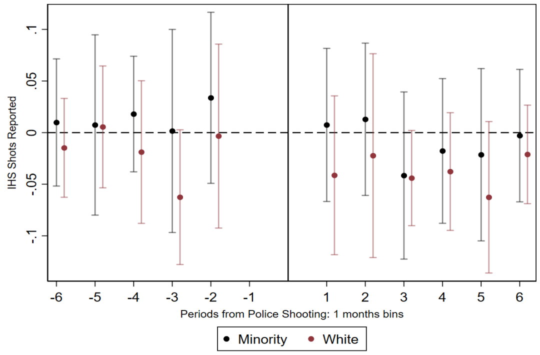
(a) IHS Police-Initiated Events



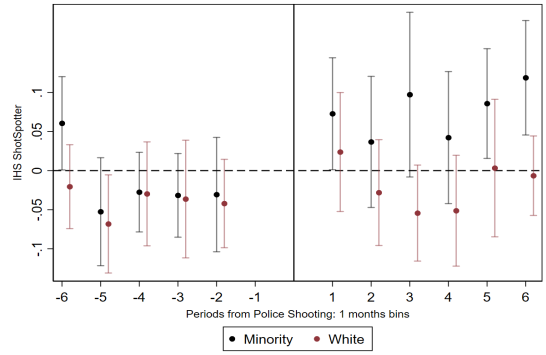
(b) IHS Arrests

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.

Figure A3: Effect of a Police Shooting by Race



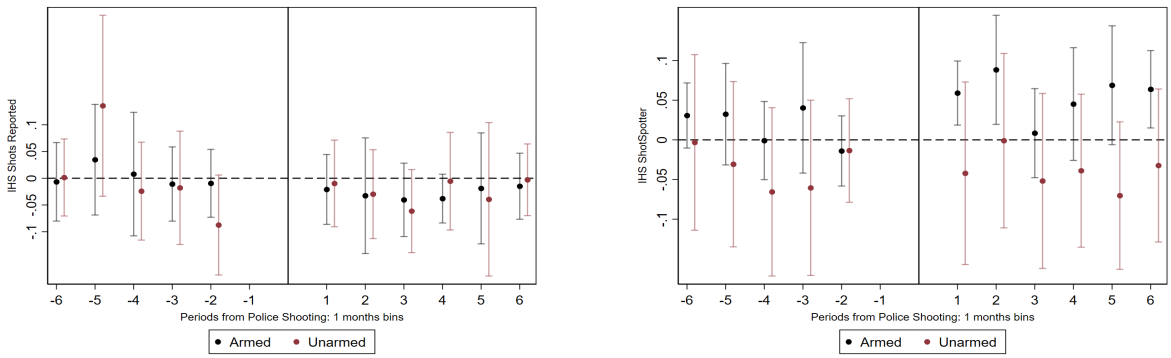
(a) Effect on IHS Shots Reported



(b) Effect on IHS ShotSpotter

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 A4: Effect of a Police Shooting by Victim's Weapon

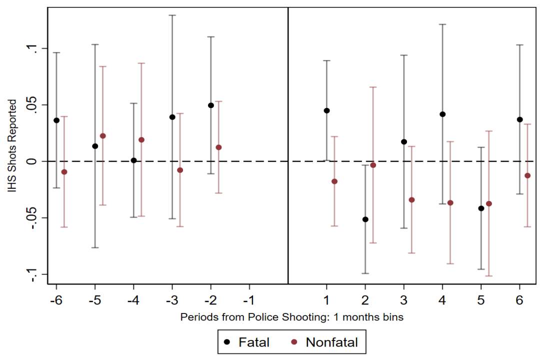


(a) Effect on IHS Shots Reported

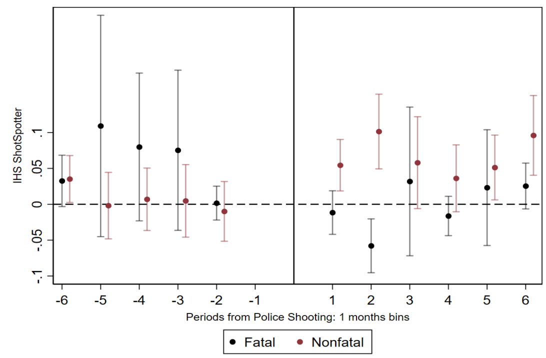
(b) Effect on IHS ShotSpotter

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 use police-involved shootings that happened between 2010 and 2018 and the 0.4 miles radius to define treatment. 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 A5: Effect of a Police Shooting by Fatality of Shooting



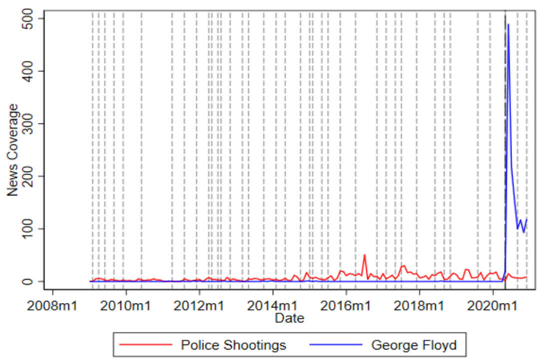
(a) Effect on IHS Shots Reported



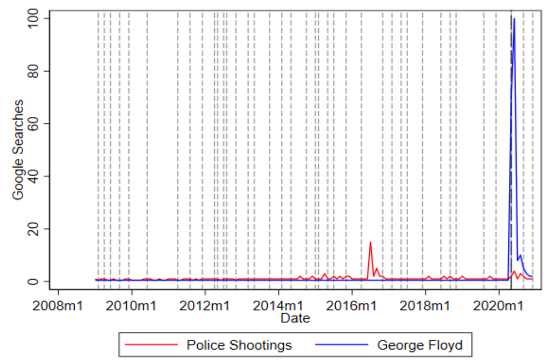
(b) Effect on IHS ShotSpotter

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. We use police-involved shootings that happened between 2010 and 2018 and the 0.4 miles radius to define treatment. 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.

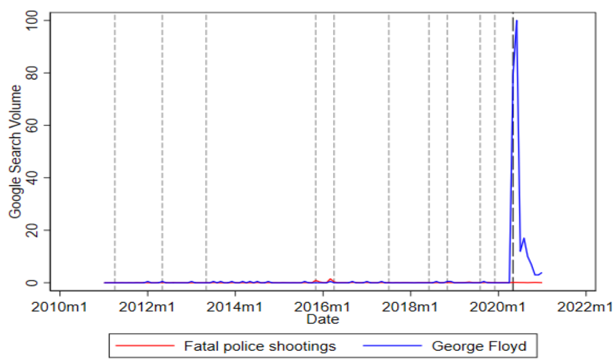
Figure A6: News Coverage and Google Trends for Police Shootings vs George Floyd



(a) LexisNexis



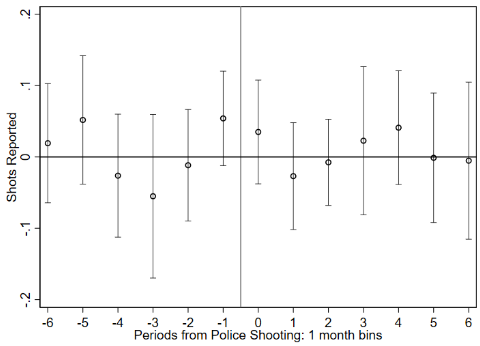
(b) Google search volume for “police shooting”



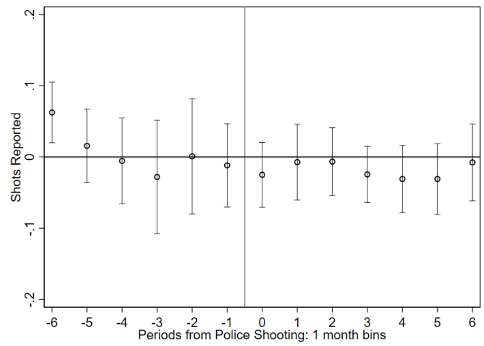
(c) Google search volume for victim names in fatal police shootings

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.

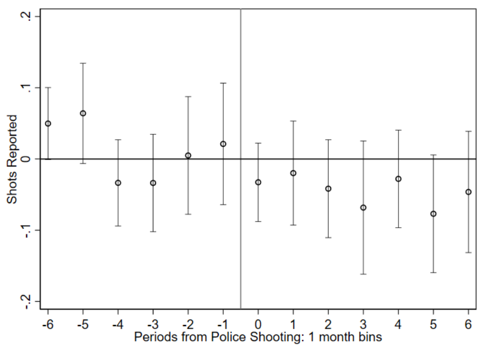
Figure A7: Event-Study Analysis of Shots Reported using Callaway and Sant'Anna (2020)



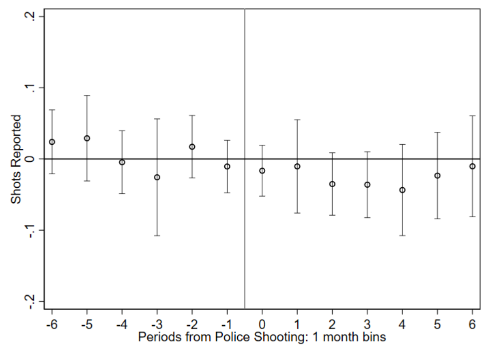
0.1 miles



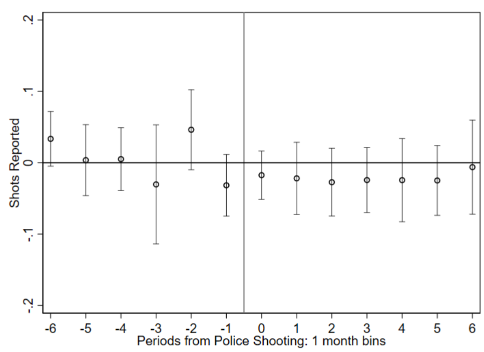
0.3 miles



0.2 miles



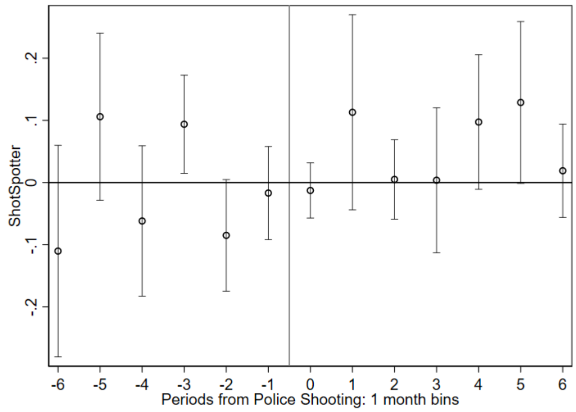
0.4 miles



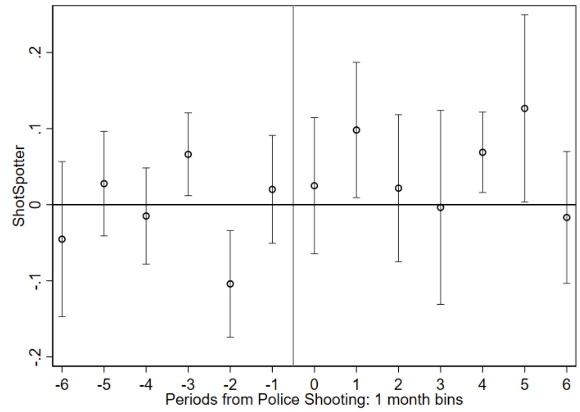
0.5 miles

Notes: These figures show the average causal estimates and 95 percent confidence intervals estimated using the Callaway and Sant'Anna procedure, where the outcome is the inverse hyperbolic transformation of shots reported. Each period is one month long.

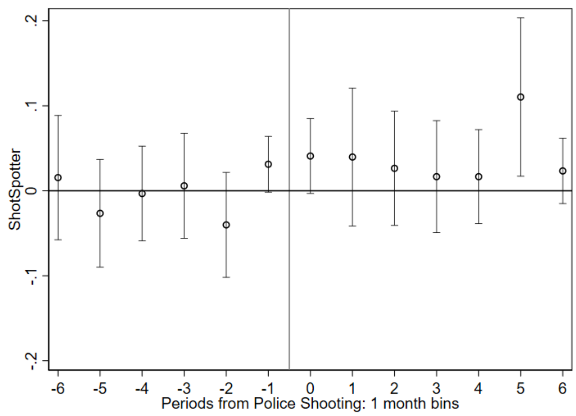
Figure A8: Event-Study Analysis of ShotSpotter using Callaway and Sant'Anna (2020)



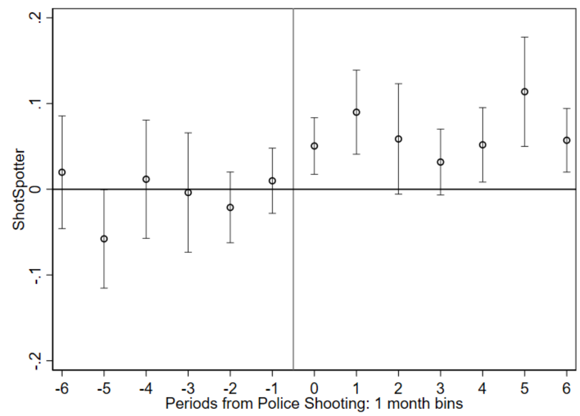
0.1 miles



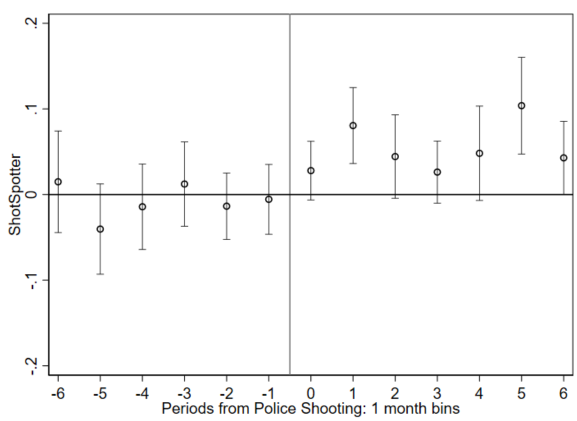
0.2 miles



0.3 miles



0.4 miles



0.5 miles

Notes: These figures show the average causal estimates and 95 percent confidence intervals estimated using the Callaway and Sant'Anna procedure, where the outcome is the inverse hyperbolic transformation of ShotSpotter incidents. Each period is one month long.