The Impact of Police Shootings on Gun Violence and Civilian Cooperation

[DRAFT]

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Abstract

This paper studies the effect of exposure to police involved shootings on gun violence and crime reporting as a measure of civilian cooperation. To distinguish between crime reporting and crime incidence, we use administrative data on 911 calls and ShotSpotter data from Minneapolis. By exploiting the variation in the timing and the distance to these incidents, we show that exposure to a police shooting increases gun-related crimes by 5-7 percent and decreases shots reported by 1-2 percent. Taken together, this implies a 6 to 9 percent decrease in the reporting rate following a police shooting.

Keywords: Crime, Crime reporting, Police use of force

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"If I called the police, I had no confidence that order would be restored... I did think it possible, maybe even probable, that one of these guys, or more, might end up shot or beaten up without cause. That they all, those fighting and those merely there waiting for the bus, might end up in handcuffs. Or the morgue."

— Salimah Perkins[1]

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. For instance, after George Floyd's murder in May 2020, confidence in the police hit a historical low of 48% (Brenan 2020). In an unprecedented move, Minneapolis voted to dismantle their current police department in November 2021. Although the proposal was rejected, it was a close race, as 44 percent of the voters voted in favor (Kaste 2021).

Similar debates and movements, thus, pose multiple questions about the impact of policing on public safety. A sizeable literature has focused on the deterring effect of police on crime (Evans and Owens 2007; Chalfin and McCrary 2017; Draca et al. 2011). However, this literature mostly disregards the repercussions of policing severity or its quality. 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.

As a byproduct of policing, police shootings can have a deterring effect and can be followed by a decline in criminal behavior, as individuals try to avoid contact with police agencies. However, in 2014, following the police shooting of Michael Brown, there was a noticeable rise in violent crime in Ferguson, Missouri. This rise was attributed to a reduction

[1] Contributor at The Baltimore Sun, Salimah Perkins, writing about a street fight she witnessed at a bus stop, and explaining why she did not report it to the police. Salimah Perkins, (Oct. 11, 2016). Law Abiding, and Afraid of the Police, Baltimore Sun. *https://www.baltimoresun.com/news/opinion/oped/bs-ed-salimah-perkins-20161011-story.html*.

in the police activity as a result of public scrutiny, a phenomenon referred to as the "Ferguson Effect" (Lind 2016).

Moreover, anecdotal evidence suggests that exposure to police use of force affects civilians' perception of the police, especially among racial and ethnic minorities. Not only can it affect crime incidence, but it can also affect the willingness to report crime and participation in civic and political engagements (Ang and Tebes 2021). From an economic perspective, the decision to report crime is subject to a cost-benefit analysis (Bowles et al. 2009). Confidence in the police's ability to resolve a situation without necessarily escalating it is a major factor in this analysis (Baumer 2002).

Nevertheless, the extent to which police use of force alters crime reporting or crime has not been extensively studied. The lack of an objective measure of crime incidence makes the distinction between the two unattainable. To make unbiased conclusions about changes in crime incidence and reporting, one has to observe the actual level of crime. Crime that is not reported nor observed by the police is excluded from official measures, depressing the number of crime incidence. 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; 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.

To overcome this issue, we focus on gun related crimes detected by ShotSpotter devices. ShotSpotter is a system of audio sensors that detect and analyze gunshot sounds and send 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 the reporting rate. In a similar paper, Ang et al. 2021 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. They find that following the incident, both the volume of 911 calls and the call-to-shot ratio significantly decrease, implying a decline in crime reporting.

Contrary to Ang et al. 2021, we avoid using highly publicized incidents. These events are extremely rare, heavily covered by media outlets and might not be representative. For instance, the viral footage of Floyd's 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). Consequently, it is unclear how to disentangle the effect of these incidents from the effect of other events happening simultaneously (protests, riots, pandemic, etc...). Instead, we focus on police involved shootings, which are more frequent events across the U.S.

To estimate the effect of exposure to police shootings, we utilize data on 911 calls, ShotSpotter and police involved shootings from Minneapolis, Minnesota. Between 2009-2019, Minneapolis experienced 57 police involved shootings, most of which were nonfatal. Using the variation in the location and the timing of these incidents, we estimate a difference-in-differences model, comparing exposed Census blocks to other blocks overtime.

Our results show a 5 to 7 percent increase in gun violence in exposed blocks relative to unexposed blocks after a police shooting. Additionally, we estimate a 1 to 2 percent decrease in shots reported by civilians. Taken together, we estimate a statistically significant 6 to 9 percent decrease in the reporting rate. These changes persist for up to three years after a police shooting, and they are more profound in minority neighborhoods.

This paper contributes to a growing literature that studies the consequences of exposure to police use of force (Ang 2021; Legewie and Fagan 2019; Gershenson and Hayes 2018). To our knowledge, it is the first to study the effect of police-involved shootings on crime incidence and reporting in one of the most populous U.S. cities. By doing so, it alludes to the social costs of policing practices that could potentially counteract their benefits in deterring crime.

In addition, our analysis is conducted at the smallest geographical level, the census block level, reflecting the hyper-local consequences of police use of force. This analysis emphasizes the local channel of information transmission, where knowledge of police incidents spreads through limited personal networks or bystanders.

Most importantly, we differentiate between crime incidence and reporting by relying on ShotSpotter data. We can also distinguish between shots reported by civilians and shots reported by the police. This assures that the effects estimated on crime reporting truly reflect a change in civilians' behavior.

The remainder of the paper proceeds as follows: Section 2 describes the data and their sources. Section 3 presents the empirical strategy. Section 4 discusses the results, and Section 5 shows their heterogeneity. Section 6 investigates the validity of the analysis. Finally, section 7 concludes.

2 Data

We obtain data from the Minneapolis Police Department on all 911 calls for service, ShotSpotter activation incidents and police-involved shootings from 2009-2019. As previously discussed, we focus on shooting crimes to be able to make the distinction 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[2]. This data documents incidents where an officer was involved in any shooting, whether fatal or not, and includes 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 shows the weapon used by the subject, if any.

In total, there were 57 unique police involved shooting incidents between 2009 and 2019, 19.3 percent of which were fatal. 66.67 percent of the civilians involved were African

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

American, while only 10.53 percent of them were White. 8.7 percent of the civilians were unarmed at the time of the shooting.[3]

We define treated blocks as those that are within a 0.1-0.5-mile radius from a police-involved shooting. We later report the results using all 5 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.

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. We balance the sample by restricting the shootings to those that happen between 2012 and 2017. This allows us to observe at least three years before and after treatment for all the treated blocks.

2.2 Outcome Variables

To measure the number of gun-related crimes, we use publicly available ShotSpotter data from the city's open data website.[4] This data includes all ShotSpotter activation incidents that occurred between 2009 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.[5] 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 a gunfire. Finally, the system sends it into an "Incident Review Center" which makes the final confirmation. This process takes almost 60

[3] Data on civilian's weapon and the fatality of the shooting are missing for 26% of the shootings.
[4] https://opendata.minneapolismn.gov
[5] https://www.shotspotter.com/technology/

seconds and provides 97 percent accuracy according to the company's webpage.[6]

According to a local news website, 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. We could only deduce the availability of a ShotSpotter device in a certain block if we observe ShotSpotter incidents in the data. Hence, we condition on observing at least one ShotSpotter activation incident per block over our sample period, and we include 1,426 Census blocks that meet this criterion.

To measure the number of shots reported, we use 911 calls for service. Our data includes more than 4.5 million events in total, where we observe the time, date, location, problem, and disposition of each call. We can also observe the source of the call in the data, whether it was citizen initiated or officer initiated. Making this distinction is pivotal to estimate changes in civilians' reporting behavior. Calls for service data usually include both civilian-initiated calls and officer-initiated calls, and a failure to distinguish between these two types can cause us to mistake a change in police activity for a change in civilians' behavior (Lehman 2021). In Minneapolis, 58 percent of calls for service are civilian initiated. We only focus on calls where citizens reported sounds of gunshots.

We collapse the data at the month and block-level and focus on two different outcome variables: ShotSpotter detected gunshots and gunshots reported through 911 calls. Our outcome variables show great variation across months and blocks, and a big portion of the 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.[7]

Summary statistics in Table 1 show that, on average, there are 0.09 ShotSpotter inci-

[6] https://www.shotspotter.com/company/

[7] 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.

dents in a given block-by-month. The number of gunshots detected in a block range from 0 to a maximum of 9 per month. Shooting crimes are more likely to occur in treated blocks (those that are within a 0.5 miles radius from a police shooting). On average, there are almost 0.1 shots reported in a block per month (Table 1). Dividing monthly shots reported by monthly ShotSpotter incidents, we conclude that on average, only 22 percent of monthly gun shots are reported.

Lastly, we use data from the American Community Survey (ACS) to examine heterogeneity across census blocks. Table Table 1 shows that on average, treated blocks have a higher percentage of Black population compared to non-treated ones (38 percent compared to 29.5 percent), and that the share of Hispanic population is relatively low across groups (6 percent Hispanic in the entire sample).

3 Empirical Strategy

Estimating the causal effect of exposure to police shootings is not straightforward, 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 the Appendix A.2, shows the geographical distribution of police shootings between 2009 and 2019 in Minneapolis. As mentioned in subsection 2.1, we define exposure by the distance from a shooting. We select Census blocks that are within a distance "r" from a police shooting as treated blocks. 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 within a 0.1-0.5 miles distance from these events to those that are not, before and after a police shooting. Particularly, we estimate the following model:

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

where Y_{bt} is the inverse hyperbolic transformation of shots reported or ShotSpotter incidents in block b, at month t. $Treat_b \times Post_t$ 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 the change in ShotSpotter and shots reported in unexposed blocks. We include month-by-year and block fixed effects, and cluster the standard errors at the census tract level.

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:

$$Y_{bt} = \alpha_0 + \sum_{t=-6}^{6} \gamma_1 Treat_b \times MonthsPost_t + Month \times Year_t + Block_b + \epsilon_{bt} \quad (2)$$

where $MonthsPost_t$ are indicator variables for months before and after a police shooting. Including block and month-by-year fixed effects, 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.

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 can be 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. As shown 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

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 five different specifications. Panels (a) and (b) of Figure 1 show the results for shots reported and ShotSpotter incidents respectively. Both graphs show that there is no evidence of pre-trends for both outcomes, which means that absent treatment, shots reported and ShotSpotter incidents would have continued to track each other across groups. This supports the validity of our research design and the parallel trends assumption, for all five different radii.

We also see a slight decrease in shots reported and an increase in ShotSpotter incidents post treatment across all specifications. Interestingly, the changes persist for more than 36 months after a police shooting.

4.2 Difference-in-differences results

Our primary results are presented in Table 2. Panel A of Table 2 shows the effect of a police shooting on shots reported, and Panel B shows the effect on ShotSpotter incidents. We report the results across all five different radii. For each radius, we calculate the difference between the effect on shots reported and ShotSpotter incidents to estimate the effect on the reporting rate.

Panel (A) shows a 1 to 2 percent decrease 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 5 to 7 percent and is statistically significant across all five specifications.

As we show in Appendix A.1, the effect on shots reported represents a lower bound of 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 2, 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. It is statistically significant 80 percent of the time. We conclude that after a police shooting, the reporting rate decreases by 6 to 9 percent in exposed blocks relative to unexposed blocks.

For example, using the 0.3 miles radius, we estimate an 8.24 percent decrease in the reporting rate, and this decrease is significant at the 1 percent level (p=0.0000658). To put this in a better context, we calculate the average reporting rate for the exposed blocks before exposure to be 0.25. This means on average, for every 100 gunshots, 25 of them were being reported by civilians. An 8.25 percent decrease in the reporting rate implies that the reporting rate becomes 0.23, which means 3 more gunshots go unreported (for every 100 gunshots, 23 are reported compared to 25).

5 Differential Effects by Race

Given that the majority of police involved shootings in Minneapolis affect the African American community (insert percentage black here), we look at the differential effects across minority (African American & Hispanic) and White neighborhoods. A census block is defined to be majority White (minority) if more than 50 percent of its population are White (minority).

Table 3 shows the effect of exposure to a police shooting across minority and White neighborhoods, using the 0.5 miles radius to define exposure. Across both types of neighborhoods, we estimate an increase in gun violence that is higher for minority neighborhoods. There is an almost 9 percent increase in ShotSpotter incidents in minority neighborhoods relative to a 3 percent increase in White neighborhoods. In minority neighborhoods, this increase is associated with a 1.7 percent decrease in shots reported, unlike in white neighborhoods, where the effect on shots reported is insignificant.

This means that the decrease in the reporting rate is higher in minority neighborhoods (10.9 percent compared to 3 percent in white majority neighborhoods). Although we do not report them in the table, these results are robust across different treatment definitions. This supports the anecdotal evidence that minority groups fear reporting to the police after being exposed to violent incidents. It is also in line with statistical figures that reflect the huge racial gap in the confidence in the police across racial groups. For example, only 19 percent of Black adults express confidence in the police, relative to 56 percent of White adults (Jones 2020).

6 Validity Threats

6.1 Two-way Fixed Effects Bias

Another possible threat to identification is bias in the two-way fixed effects estimates arising from the fact that treatment timing is staggered across blocks (Goodman-Bacon 2021; Callaway and Sant'Anna 2021; De Chaisemartin and d'Haultfoeuille 2020). Thus, we use the Callaway and Sant'Anna 2021 estimation method for robustness.

We plot the estimates and provide the figures in Appendix A.3. As before, the parallel trends assumption in the pre-treatment periods appears to be satisfied. Thus, both procedures support the validity of our empirical strategy. Although this approach does not exactly match the previous one, we still observe a persistent increase in ShotSpotter incidents following exposure to police shootings.

6.2 Change in the Nature of Shooting Crimes

A possible threat to the validity of our results is a change in the nature of shooting crimes before and after a police shooting. Suppose there are two types of gunfire incidents, ones that are always reported, such as incidents that result in an injury, and ones that are never reported. If the increase in gunfire is caused by an increase in the latter, the estimated decrease in the reporting rate can be invalid.

As a validity check, we test for any change in the observed characteristics of ShotSpotter incidents before and after a police shooting. We only observe the date, time and location of each incident. Using the main generalized difference-in-differences equation, we estimate the effect of a police shooting on the date and the time of ShotSpotter incidents. The results are presented in Table 4. Day time is a dummy variable that takes the value 1 if the incident happens between 6 am and 6 pm, and weekend is 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. Although we cannot observe any other characteristics, we can argue that the day and the time of a gun-related crime are correlated with its nature. For example, more violent crimes such as murder, assault and robbery are more likely to happen at night (Doleac and Sanders 2015).

6.3 ShotSpotter Inaccuracy

Although it provides the peculiar advantage of observing a true measure of gunshots, ShotSpotter data have 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). The accuracy in detecting the location of the gunshots, on the other hand, was 90.9 percent (Goode 2012).

In order to decrease the likelihood of observing false positives in our data, we drop dates where ShotSpotter might have detected firecrackers instead of shots. These include New Year's Eve and Fourth of July. Moreover, we have no direct proof of systemic differences in the measurement error across treatment and control blocks, which should eliminate the threat of biased results arising from the possible inaccuracy of ShotSpotter devices.

7 Conclusion

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 gunshots detected by ShotSpotter in Minneapolis, we employ a difference-in-differences methodology, exploiting the variation in the location and the time of police involved shootings. Since ShotSpotter data provides an objective measure of gunfire in Minneapolis, we can isolate the effect of police involved shootings on crime incidence from that on crime reporting, overcoming a big hurdle in the criminal justice literature.

We conclude that Census blocks exposed to police shootings face a double whammy: (1) an increase in the level of gun violence, accompanied with (2) a decrease in its reporting rate. In line with Carr and Doleac 2018, our results indicate the importance of having better measures of crime and that using 911 calls as a measure of crime reporting leads to biased results.

One major limitation of our study is that the effect on other types of crime is unknown. Yet, our results have profound implications on the literature that studies the consequences of police violence on socioeconomic outcomes. This literature often relies on traditional crime measures to rule out that the observed effects are driven by an increase in crime incidence after a negative police encounter. Our empirical evidence shows, however, that crime incidence is in fact increasing, affecting other outcomes at the same time.

Our study also shows that violent encounters with the police might counteract the positive effects of policing, by increasing gun violence and decreasing civilians' cooperation. The latter is especially critical, considering that ultimately, the police cannot deter crime without the assistance of the community.

Future research is required to understand the underlying mechanisms. For example, the increase in gun violence can not be solely attributed to a "Ferguson Effect", given that there can be other mechanisms at play (legal cynicism, decrease in the reporting rate, etc...). This is especially true because the police shootings we include in our sample are not highly publicized, so it is unknown whether police activity is changing following these events.

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Tables and Figures

Table 1: Sample Census Blocks Characteristics

	(1) Entire Sample	(2) ≤0.5 miles	(3) >0.5 miles
Block Characteristics			
Percent White	37.47	30.53	42.20
	(23.57)	(20.41)	(24.40)
Percent Black	32.98	38.03	29.54
	(22.02)	(23.30)	(20.40)
Percent Hispanic	6.076	5.951	6.161
	(8.012)	(7.851)	(8.118)
Total Population	97.68	95.27	99.33
	(73.64)	(69.48)	(76.31)
Outcomes			
ShotSpotter	0.0863	0.149	0.0434
	(0.353)	(0.466)	(0.240)
Shots Reported	0.105	0.142	0.0804
	(0.381)	(0.450)	(0.322)
Observations	90156	36564	53592

Standard deviations in parentheses.

Note: Standard deviations are in parentheses. Column (2) presents the summary statistics for Census blocks that are within a 0.5 miles distance from a police shooting. The sample is restricted to blocks that have non-negative total population.

Table 2: Difference-in-Difference Effects

	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
Panel A: Shots Reported					
After a Police Shooting	0.00265	-0.0150**	-0.0123**	-0.0187***	-0.0202***
	(0.007)	(0.006)	(0.005)	(0.006)	(0.006)
Observations	176616	159984	145728	127644	110352
Panel B: ShotSpotter					
After a Police Shooting	0.0539*	0.0549**	0.0700***	0.0754***	0.0659**
	(0.031)	(0.023)	(0.020)	(0.025)	(0.029)
Observations	176616	159984	145728	127644	110352
Difference	-0.0513	-0.0699	-0.0824	-0.0941	-0.0861
SE Difference	0.0318	0.0241	0.0206	0.0259	0.0300
P-value (H_0: β_1^{SS} - $\beta_1^{SR} = 0.$)	0.107	0.00373	0.0000658	0.000275	0.00406

Standard errors in parentheses.

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

Notes: This table shows the difference-in-differences results from Equation (1) using police-involved shootings that happened between 2012 and 2017 as treatment. It also shows the p-values of 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 3: Difference-in-Difference Effects by Race (0.5 miles)

	IHS Shots Reported	IHS ShotSpotter
Panel A: Minority Neighborhoods		
After a Police Shooting	-0.0174***	0.0917***
	(0.006)	(0.028)
Observations	39732	39732
Panel B: White Neighborhoods		
After a Police Shooting	0.00335	0.0307*
	(0.007)	(0.017)
Observations	45012	45012

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) by racial composition of neighborhoods using the 0.5 miles radius to define treatment. A census block is defined to be majority White (minority) if more than 50 percent of its population are White (minority). All regressions include block and month-year fixed effects, and standard errors are clustered at the census tract level.

Table 4: Validity Checks

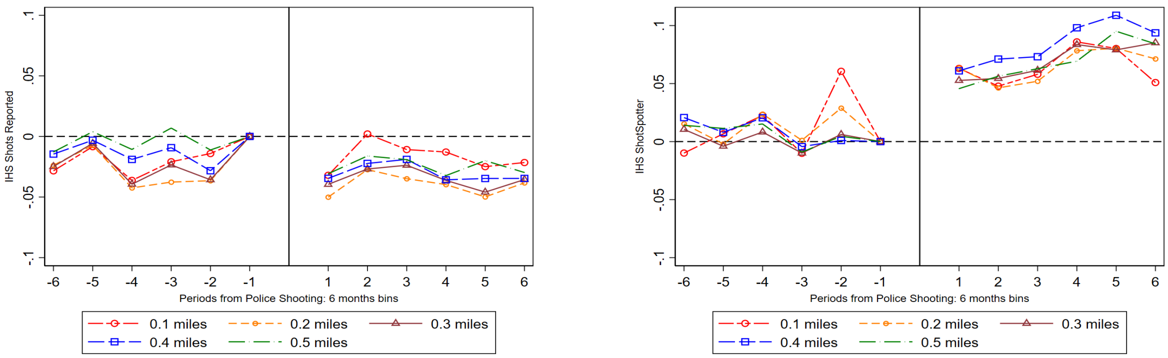
	Day Time				
	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
After a Police Shooting	0.0163	0.00393	0.0115	0.0126	0.0122
	(0.046)	(0.025)	(0.021)	(0.019)	(0.027)
Outcome Mean	0.189	0.189	0.189	0.189	0.189
	Weekend				
	0.1 miles	0.2 miles	0.3 miles	0.4 miles	0.5 miles
After a Police Shooting	-0.0176	0.00226	-0.0197	-0.0273	-0.00676
	(0.028)	(0.026)	(0.023)	(0.021)	(0.018)
Observations	21693	21693	21693	21693	21693
Outcome Mean	0.372	0.372	0.372	0.372	0.372

Standard errors in parentheses.

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

Note: This table presents the effect of a police shooting on ShotSpotter incidents' characteristics. We estimate equation (1), where $Y_b t$ 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.

Figure 1: The Effect of Police Shootings on Shots Reported and ShotSpotter



(a) Effect on IHS Shots Reported

(b) Effect on IHS ShotSpotter

Notes: These figures show the estimated results of Equation 2 for all five specifications. 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 six months long, and period -1 is excluded.

A 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[8]:

[8] 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.

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

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.[9] 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:

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

[9] 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.

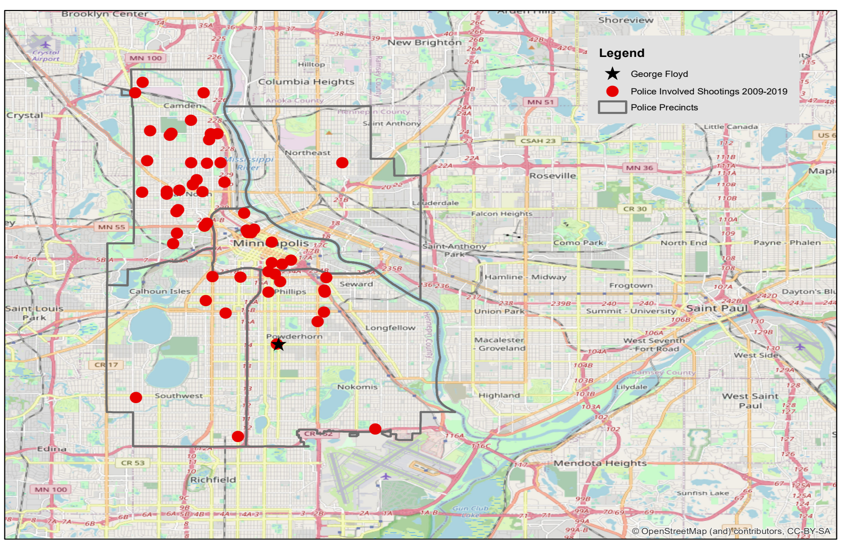
A.2 Tables and Figures

Table A1: Shootings Characteristics

	Mean	Std Deviation
Subject		
Female	10.53	(30.96)
Black	68.42	(46.90)
White	10.53	(30.96)
Hispanic	1.754	(13.25)
Observations	57	

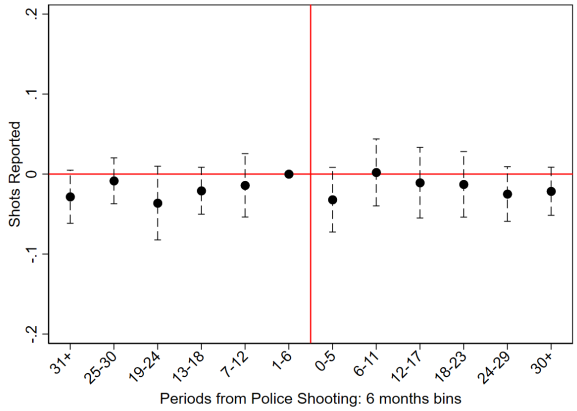
Standard deviations in parentheses.

Figure A1: Police-Involved Shootings in Minneapolis 2009-2019

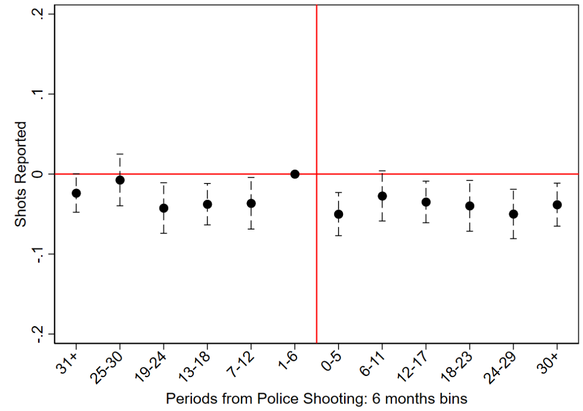


Notes: This map shows the geographical distribution of police involved shootings that occurred between 2009-2019 in Minneapolis across the five police precincts.

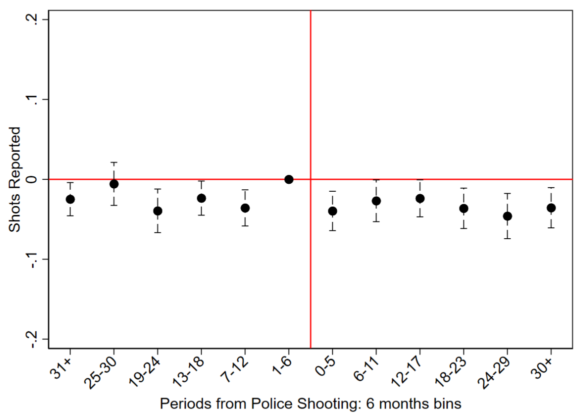
Figure A2: Event-Study Analysis of Shots Reported



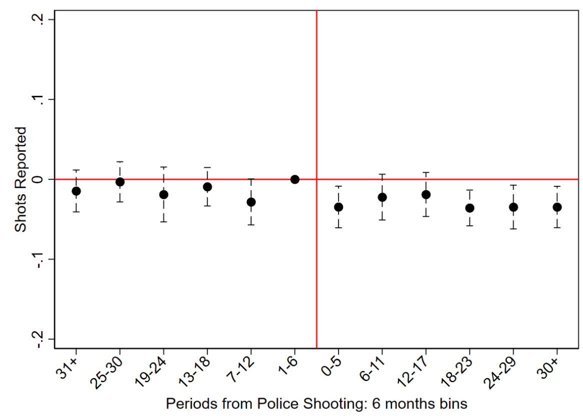
0.1 miles



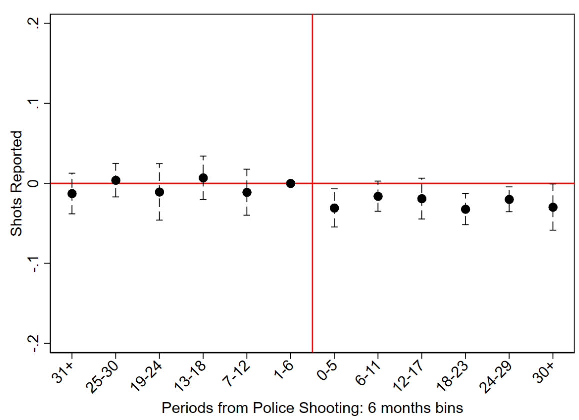
0.2 miles



0.3 miles



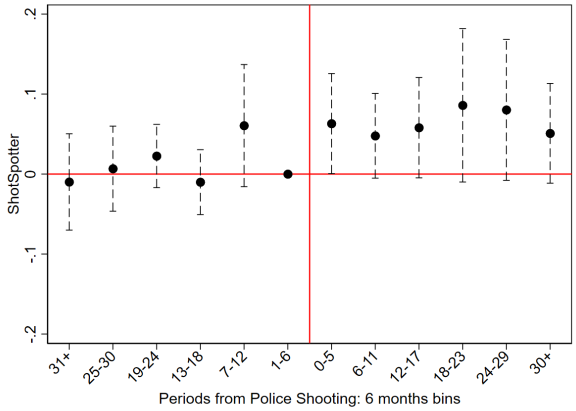
0.4 miles



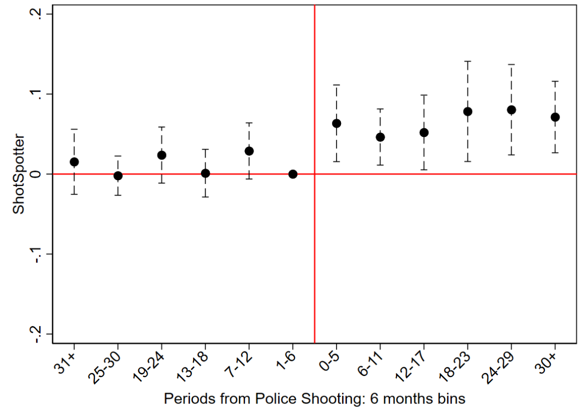
0.5 miles

Notes: These figures show the estimated coefficients and 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 shots reported. 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 six months long, and period -1 is excluded.

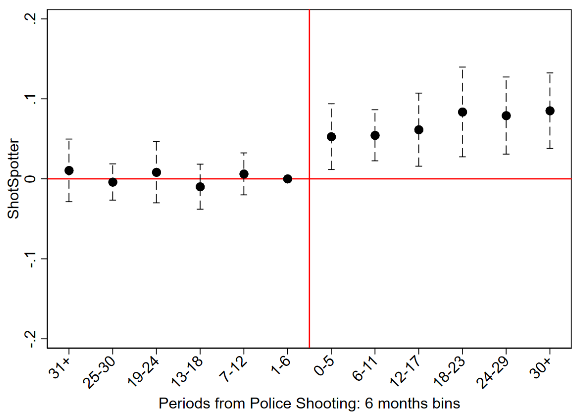
Figure A3: Event-Study Analysis of ShotSpotter



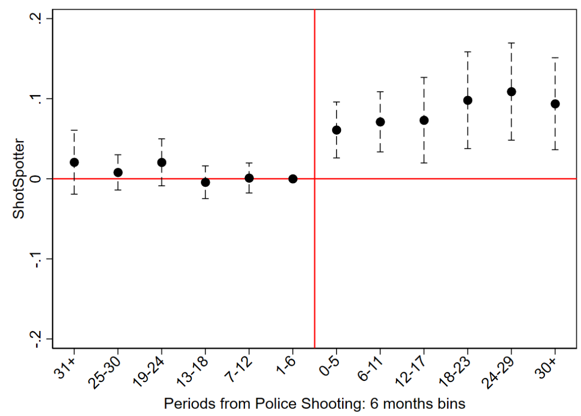
0.1 miles



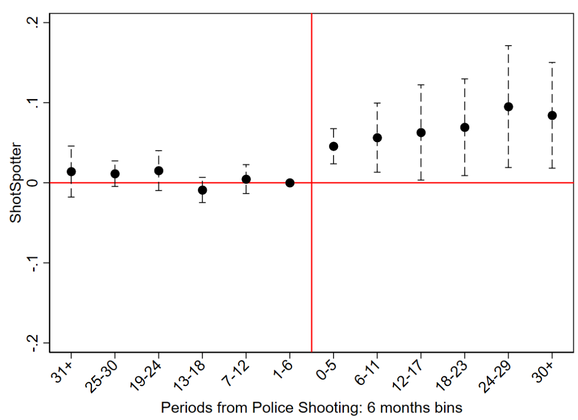
0.2 miles



0.3 miles



0.4 miles

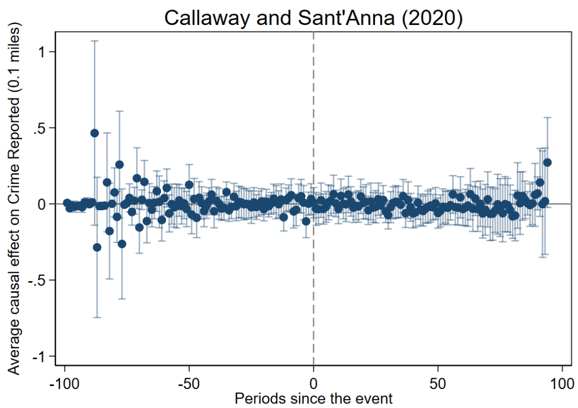


0.5 miles

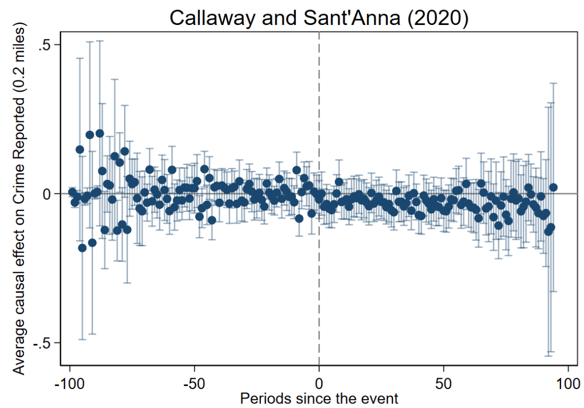
Notes: These figures show the estimated coefficients and 95 percent confidence intervals from event study regressions of Equation 2 for all five definitions of treatment, where the outcome is the inverse hyperbolic transformation of ShotSpotter incidents. Census block fixed effects as well as month-by-year fixed effects are included. Standard errors are clustered at the tract level. Each period is six months long, and period -1 is excluded.

A.3 Callaway and Sant'Anna Estimation Procedure (2020)

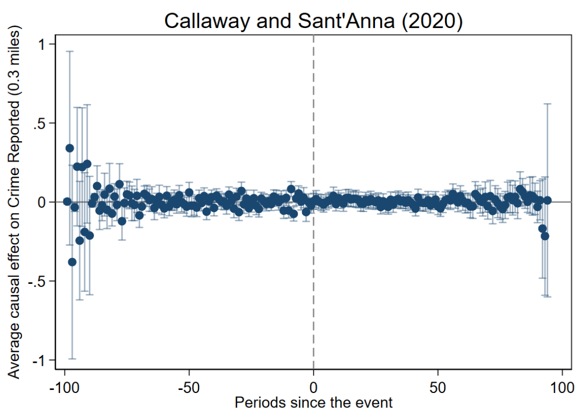
Figure A4: Event-Study Analysis of Shots Reported using Callaway and Sant'Anna (2020)



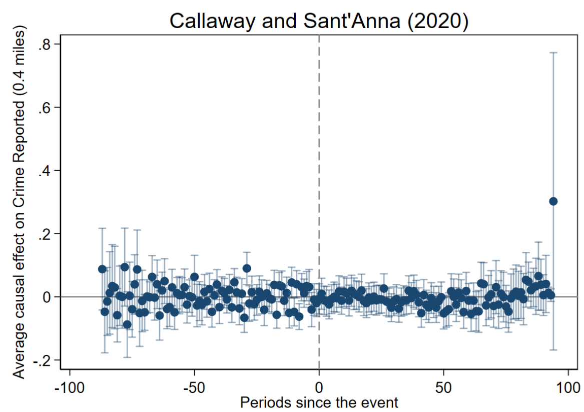
0.1 miles



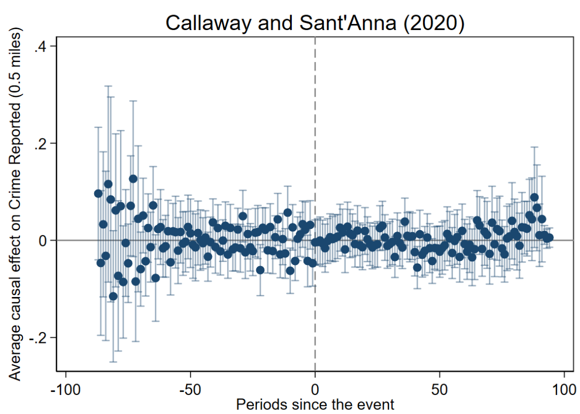
0.2 miles



0.3 miles



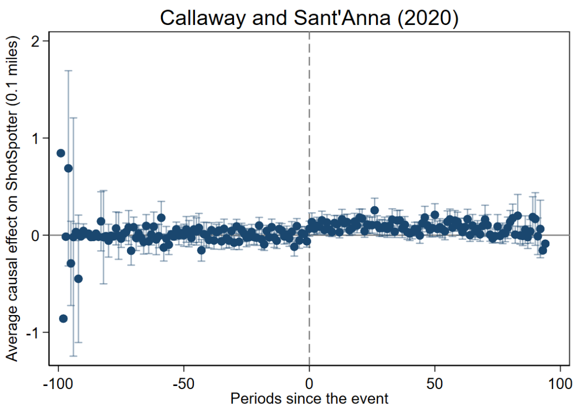
0.4 miles



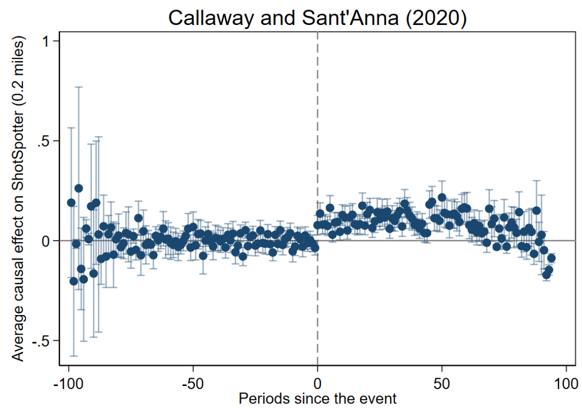
0.5 miles

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 shots reported. Each period is one month long.

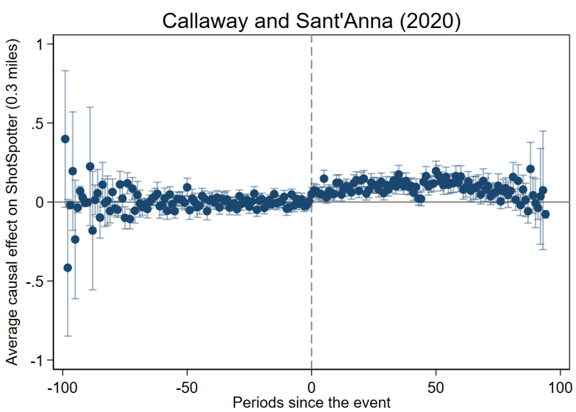
Figure A5: Event-Study Analysis of ShotSpotter using Callaway and Sant'Anna (2020)



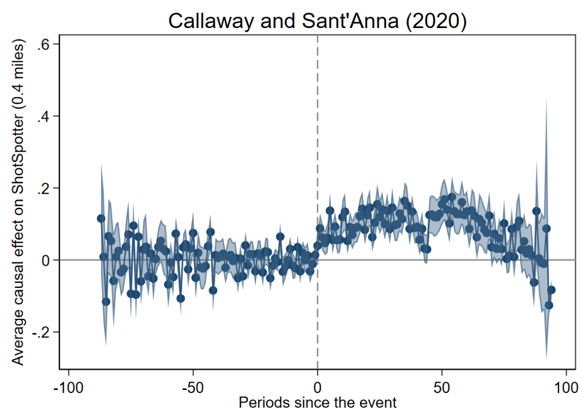
0.1 miles



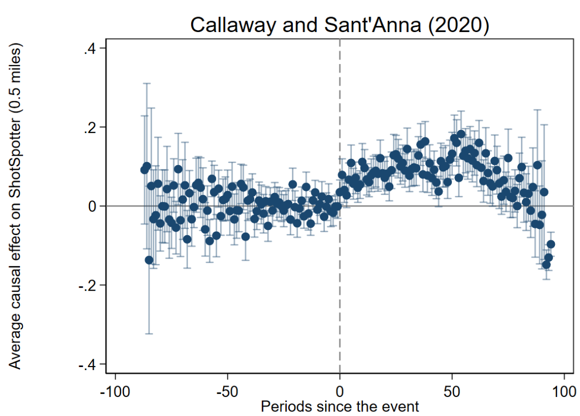
0.2 miles



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.