The Effect of ShotSpotter Technology on Police Response Times

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Abstract

ShotSpotter is an acoustic gunfire detection technology utilized by police departments in over 150 cities with the intention of rapidly dispatching police officers to violent crime scenes to catch perpetrators and reduce gun violence. Despite its prevalence, little is known about its effectiveness in reducing gun violence (intended consequence) nor its effect on 911 emergency response times (unintended consequence) given its resource-intensive operating procedures. In this paper, we utilize variation in timing from ShotSpotter roll-outs across Chicago police districts from 2016-2022 to estimate the causal effects of ShotSpotter on 911 emergency response times that are designated as Priority 1 (immediate dispatch). Using comprehensive 911 dispatch data from the Chicago Police Department, we find that ShotSpotter leads police officers to be dispatched one-minute slower (23% increase) and arrive on-scene two-minutes later (14% increase) while controlling for the police officer availability and overall 911 call quantities. Moreover, these effects are driven by resource-constrained periods, and consequently, reduce police officers' success rate in arresting perpetrators (12%) when responding to emergency calls. However, we also find that ShotSpotter increases the number of gun-related arrests, thereby indicating success in achieving its primary goal, albeit at a significant cost.

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1 Introduction

Artificial intelligence (AI) has surpassed its role in social media consumption, becoming an integral component of modern policing. As of 2023, police departments are utilizing AI policing technologies such facial recognition, traffic cameras, and predictive algorithms in effort to combat rising violent crime. These AI technologies are seen as imperative to public safety moving forward, in effort to mitigate both police officer shortages and all-time high levels of public mistrust in police.

One quickly expanding and widely adopted AI technology is ShotSpotter—an acoustic gunfire detection technology which is currently implemented in over 150 cities world-wide. Shotspotter's primary intention is to rapidly dispatch police officers to violent crime scenes to catch perpetrators and reduce gun violence. The technology utilizes an array of microphones and sensors placed on street lights and buildings that use machine learning algorithms to detect the sound of gunfire, triangulate its location, and alert police officers for rapid response. Because of its unique functionality, ShotSpotter bypasses the reliance on civilian reporting which has been facing a persistent decline since 2020 (Ang et al., 2021). In effect, previous studies have utilized this feature of ShotSpotter as a measure of underlying crime that is independent of reporting (Carr and Doleac, 2016, 2018; Ang et al., 2021).

Despite ShotSpotter's prevalence, there is little consensus on its intended effect on gun violence, or more insidiously, its unintended consequences on policing given the extensive amount of police resources it requires. In Chicago, the setting of this paper, nearly all instances of gunfire detected by ShotSpotter result in a police unit dispatching to the potential crime scene. Each of these detections are designated as Priority 1 (immediate dispatch)—the equivalent priority to a report of an active shooter. As a result, an average day in Chicago experiences approximately 60 ShotSpotter dispatches with officers spending roughly 20 minutes investigating the crime scene. This large allocation equates to 20 hours of daily officer resources and comes at the cost of another time-sensitive police responsibility—Priority 1 911 call response times.

In this paper, we utilize variation in timing from ShotSpotter rollout-outs across Chicago police districts from 2016-2022 to estimate the causal effect of ShotSpotter technology on response times from 911 calls designated as Priority 1. Using 911 dispatch data from the Chicago Police Department (CPD), we construct two measures of police response: the time from a 911 call to when a 911 dispatcher finds an available police officer for dispatch (call-to-dispatch) and the time from the 911 call to when the officer arrives on scene (call-to-on-scene). Using a staggered difference-in-differences framework, we find that both response times are significantly increased following the implementation of ShotSpotter by approximately one minute (23%) and two minutes (15%) respectively when controlling for the amount of police working and 911 call volumes. These estimates are robust to a variety of sensitivity tests and estimators.

Moreover, we find that the increases in response times are driven by resource-constrained periods. In particular, days where there are less officers on duty or more 911 call volumes each show larger effect sizes, thereby suggesting that ShotSpotter hinders police officers' capacity to complete their other duties in favor of responding to ShotSpotter alerts. Consistent with this mechanism, response times from other time sensitive calls (Priority 2) are also increased while conversely, time-insensitive calls (Priority 3) appear unaffected.

Consequently, these increased response times come at a significant cost. In Section 6.1, we analyze the relationship between police response time and the likelihood of an arrest. We find that Priority 1 calls are 8% less likely to have the perpetrator caught, consistent with previous literature that attributes faster rapid response to higher clearance rates (Blanes i Vidal and Kirchmaier, 2018) and lower likelihood of injury (DeAngelo et al., 2023). The effect is particularly apparent in calls regarding domestic battery (16%) and domestic disturbances (15%)—two situations in which may escalate without quick intervention. However, distinct these previous works, we are able to closely examine a determinant of rapid-response directly, rather than focus solely on its consequences.

Surprisingly, the intensive resources allocated to ShotSpotter appear to be relatively fruitless. In Section 6.1, we find little suggestive evidence that ShotSpotter increases the probability of arrest for 911 reports of gun violence. Furthermore, a tertiary analysis in Section 6.4 reveals that few ShotSpotter dispatches recover firearms or result in an arrest. This is consistent with previous literature that has found limited evidence that ShotSpotter reduces gun-related homicides or arrests (Doucette et al., 2021). Taken together, ShotSpotter appears to misallocate scarce police resources, resulting in a significant social cost.

Although few studies have examined the effects of ShotSpotter, we contribute to a growing literature on the effects of technology on policing, and in a wider context, the criminal justice system. However, while previous studies have found positive effects in the form of algorithmic bail decisions (Kleinberg et al., 2018), body-worn cameras (Zamoff et al., 2022; Ferrazares, 2023), electronic monitoring (Williams and Weatherburn, 2022), militarization (Harris et al., 2017; Bove and Gavrilova, 2017), predictive policing (Mastrobuoni, 2020; Jabri, 2021; Heller et al., 2022), and traffic cameras (Conover et al., 2023), we conversely find significant costs attributed to a policing technology that is expensive both fiscally and socially.[1]

More broadly, this study adds to the claim that cities are under-policed, as put forth in Chalfin and McCrary (2018). Similar studies have explored the added benefits of additional police presence through crime deterrence (Chalfin and McCrary, 2018; Weisburd, 2021; Mello, 2019). Distinct from these works, the unique setting of this paper allows us to explore shocks in the availability of officers due to the operating procedures of ShotSpotter. We find that when police resources are stretched thin, the effectiveness of a police force to respond to crimes and arrest perpetrators is diminished.

The paper proceeds as follows: Section **??** provides background information on dispatching procedures and implementation of ShotSpotter in Chicago, Section 2 discusses the data, Section 3 describes the empirical strategy, Section 4 presents the main results, Section 5 contains heterogeneity analysis, Section 6 discusses the implications, and Section 7 concludes.

[1] Chicago is currently under a 33 million dollar 3-year contract with ShotSpotter.

1.1 ShotSpotter Technology and Implementation in Chicago

ShotSpotter is an acoustic gunfire technology that employs a network of microphones and sensors on buildings and light-posts to detect gunfire sounds. These sounds are used to triangulate the location of potential gunfire, which is then relayed to nearby police officers. Over the past decade, this technology has seen significant expansion and is now operational in over 150 cities globally. Advocates promote the technology as a tool for enabling rapid police response to gunfire incidents, whereby they can catch the perpetrators, and reduce the quantity of guns. Moreover, the unique functionality of ShotSpotter allows police departments to bypass their reliance on civilian reporting. However, previous studies have found mixed evidence supporting these claims (this needs a BIG cite here).

The technology relies on machine learning algorithms to classify sounds of potential gunfire.[2] When a potential gunshot is detected, the sensors triangulate the location of the noise and data/recordings on the incident are forwarded to ShotSpotter's Incident Review Center. At this center, a human reviewer assesses the data, and flags for false-positives to avoid erroneous alerts. Once a gunshot is confirmed, information regarding the location and number of shots fired are shared with the police department where dispatchers can then send officers to scene. This entire process from gunshot noise to police dispatch is known as a *ShotSpotter dispatch*.

In Chicago, ShotSpotter technology has been implemented in 12 of the 22 police districts. The staggered roll-out begins in January 2017 and ends in May 2018.[3] Officially, there is no justification for the specific order in which certain police districts are chosen to receive ShotSpotter. Nevertheless, it is important to note that the police districts chosen have historically high rates of gun violence.[4]Appendix Figure A1 shows the locations of the 12 police

[2] According to ShotSpotter's website, from 2019 to 2021, the aggregate accuracy rate across all of their customers was 97 with a very small false-positive rate of approximately 0.5%, however this has not been independently tested.

[3] This wide-scale adoption follows previous testing of select areas between 2003 and 2007, and again in 2012. However, no district received district-wide coverage during this trial period and the extent of testing was small (https://www.cbsnews.com/chicago/news/chicago-police-testing-new-gunshot-detection-technology/), Office of Inspector General

[4] Note that difference-in-differences relies on the assumption of common trends, not random assignment

districts in Chicago that received ShotSpotter technology. As mentioned previously, the areas where this technology is implemented (the South and West Chicago areas) experience higher rates of gun crime on average.

1.2 Dispatching 911 Calls and ShotSpotter Alerts in Chicago

In Chicago, the coordination of emergency 911 calls involves two main entities: the Office of Emergency Management (OMEC) and the Chicago Police Department (CPD). The OEMC oversees 911 calls and dispatches available police officers to the crime scenes. Each 911 call is prioritized on a scale of imminent danger/threat ranging from Priority 1 (immediate dispatch) to 3 (routine dispatch).[5]

On the other hand, the coordination of ShotSpotter dispatches is a collaborative effort involving the OEMC, CPD, and the Strategic Decision Support Center (SDSC). When gunfire is detected, ShotSpotter's headquarters sends vital information such as the location, time, severity, amount of shots being fired, and direction of possible offender to the SDSC. The SDSC then synthesizes this information and notifies the OEMC to immediately dispatch a police officer to the location of the gunfire.

Importantly, each ShotSpotter alert is classified with the same distinction as a Priority 1 911 call. Priority 1 necessitates immediate dispatch due to the imminent threat to life, bodily injury, or major property damage/loss. Hence, both Priority 1 911 calls and ShotSpotter alerts share the same dispatch procedures and responding officers. Furthermore, the OEMC prioritizes dispatching all Priority 1 emergencies to rapid response units and police officers within the police district of occurrence.[6] Only in rare circumstances are police officers assigned to emergencies outside their district.[7]

of the rollout.

[5]Technically, there are 6 priorities ranging from priority 0-5. However, Priority 0, 4, and 5 are reserved for special cases such as police officers calling for emergency assistance, administrative meetings, or alternate responses that do not need a field unit respectively.

[6]Specifically, dispatchers prioritize dispatching police officers within the beat they are assigned to. Police beats are subsections within police districts.

[7]In particular, the dispatching order is in the following order of priority: rapid response unit or beat unit from the beat of occurrence, tactical unit, rapid response sergeant, sector sergeant, tactical sergeant, other

Despite the similarities in ShotSpotter dispatches and Priority 1 911 calls, police officers must follow an additional operating procedure when arriving to the location of a ShotSpotter alert. In particular, officers are instructed to canvass a 25-meter radius of the precise location identified via the ShotSpotter system for victims, evidence, and witnesses. Moreover, officers are also expected to notify the SDSC if they are aware of any deficiencies in ShotSpotter data or alerts, and if completing a case report, to document if the case incident is ShotSpotter-related. According to the data on ShotSpotter-related dispatches, each ShotSpotter dispatch takes an officer an average of 20 minutes to complete the investigation once they have arrived on-scene.

2 Data

2.1 Data Sources

The main sample is a daily-level panel from 2016 to 2022 that is constructed using several administrative data sets obtained through Freedom of Information Act requests to the Chicago Police Department (CPD). This data contains 911 call dispatches, officer shifts of sworn police officers, and district-level ShotSpotter activation dates. Additionally, these data are supplemented with reported incidents of crime, arrests, and ShotSpotter dispatches downloaded from Chicago's Open Data Portal.

The CPD 911 call dispatch data encompasses all 911 calls that led to the dispatch of a CPD officer. This administrative data is rich, containing information on the time of the 911 call, the time an officer is dispatched to the scene of the crime, and the time the officer arrives on-scene, each recorded at the seconds level. Additionally, the data details details the priority-level of the call, a brief description, a location, and an indication of if an arrest is made.

Based on this information, we construct the two main outcome variables: the time from

field supervisor, and closest available unit.

a 911 call to an officer dispatch (call-to-dispatch) and the the time from a 911 call to an officer's arrival (call-to-on-scene). Notably, while call-to-dispatch contains no missing data, approximately 52% of the call-to-on-scene information is missing due to officers failing to report when they arrive at the scene. However, we address this potential limitation in Appendix Table BLANK where we find little evidence of significant changes in the frequency of officers failing to report their on-scene time due to ShotSpotter's rollout.

These two measures of time accomplish different measures of police availability. If an officer is busy, they will be delayed or unable to be dispatched. This increase in time would be seen as a higher call-to-dispatch time and is a function of the coordination of a OEMC dispatcher and an individual police officer. On the other hand, call-to-on-scene, which captures both the dispatch time and the time an officer takes to arrive on scene may increase independently of call-to-dispatch time if, for example, an officer drives at a slower speed or is located farther away from their dispatch location.

The police shift data contains information on every shift start time, end time, and district/beat assignment worked by CPD staff in the sample period. We restrict the shift data to include only police officers that are present for duty, excluding administrative positions and higher level managerial roles such as police lieutenants and police chiefs. To assess officer availability, we construct the number of officer hours within a police district-day. By using on the number of officer hours rather than the number of shifts, we account for the possibility of overtime or early-leave.

The ShotSpotter activation dates indicate when each police district is equipped with ShotSpotter technology. However, since the exact day is missing, we rely on ShotSpotter alert data, which provides minute-level alerts. This allows us to determine the specific activation day for each police district. Nonetheless, we observe several discrepancies in the activation dates when comparing to the number of alerts in districts 6, 9, 10, and 15. In particular, these districts have no ShotSpotter alerts until several months after their official activation date. Therefore, we adjust these four dates of activation to align with the onset of ShotSpotter alerts. This adjustment ensures that the effects observed are accurately attributed to police officers responding to ShotSpotter alerts. However, as a robustness check, we estimate the results using the official dates in Appendix Figure A2 and find that the results remain consistent.

Figure 1 shows the monthly trend of ShotSpotter dispatches in addition to the activation dates as displayed by the dashed line. Each police district exhibits an increase in ShotSpotter dispatches as time progresses. This is possibly due to ShotSpotter's machine learning algorithms refining with time.

2.2 Sample Restrictions

We restrict the sample to only 911 call dispatches of Priority 1 (immediate dispatch), Priority 2 (rapid dispatch), and Priority 3 (routine dispatch).[8] Priorities 4 and 5 are omitted as these are reserved for special cases and administrative designations such as a police beat-meeting. These exclusions account for approximately ~0.04% of the total number of 911 dispatches. By including only Priority 1-3 the analysis focuses only on the call types that are most commonplace or require the most time-sensitive responses.

Three further restrictions are implemented to reduce sensitivity of the estimates. First, all observations that exhibit a negative call-to-dispatch or call-to-on-scene time are removed, accounting for approximately 0.03% of the data. Second, for each priority level, call-to-dispatch and call-to-on-scene outliers that exceed three standard deviations from the mean are omitted. This is done to mitigate the impact of outliers on the ordinary least squares estimator which is sensitive to extreme values. We relax this restriction in Appendix Figure BLANK to verify the consistency of the results. Last, specific dates including January 1, July 4, and December 31 are excluded from the analysis. These dates coincide with celebratory gunfire and fireworks that may generate many false-positive ShotSpotter alerts.

[8] We also include Priority 0 (Emergency Assistance), since these are time-sensitive responses. However, these are extremely rare and occur in less than 0.1% of the sample.

2.3 Descriptive Statistics

Each data source is aggregated to the police district-day and matched by police district and date. Importantly, both call-to-dispatch and call-to-on-scene times are averaged, and hence, these outcomes are interpreted as average daily response times within a police district-day. Conversely, officer hours, ShotSpotter dispatches, crimes and arrests are aggregated as counts and therefore represent the number of occurrences within a police district-day.

Table 1 shows summary statistics of the main outcome variables and corresponding control variables, all presented at the police district-day level. Panel A shows that the time to dispatch a police officer to the crime scene for Priority 1 911 calls (Call-to-Dispatch) is approximately four minutes, while it takes police officers an additional six minutes on average to arrive on-scene. We additionally plot the distribution of both of these outcome variables in Figure 2 which shows that the average daily call-to-dispatch and call-to-on-scene times are centered around their mean, although their right tails are rather large. Priority 1 calls are the most frequent as shown in Panel B, and police officers are dispatched approximately 150 times a day within a district. Moreover, for every 100 police dispatches, there are approximately 14 arrests made (Arrest Rate). Considering the high level of crime in the South and West locations of Chicago, the presence of officers varies considerably across districts, ranging from as little as 200 officer-hours or as high as 3431 officer-hours. We later analyze this heterogeneity in officer hours in Section 4.1 where we find longer response times when there are fewer officers.

3 Empirical Strategy

3.1 Baseline Specification

To estimate the causal effect of ShotSpotter technology on police response times, we estimate the following staggered difference-in-differences using ordinary least squares (OLS):

$$ResponseTime_{dt} = \beta ShotSpotter_{dt} + \delta_d + \gamma_t + \lambda \mathbb{X}_{dt} + \varepsilon_{dt} \tag{1}$$

where $ResponseTime_{dt}$ is the average call-to-dispatch or call-to-on-scene in police district d at time t. The treatment variable is $ShotSpotter_{dt}$, which is an indicator variable equal to one when a police district is equipped with ShotSpotter. Moreover, δ_d and γ_t are police district and day-by-month-by-year fixed effects, respectively. Finally, $\mathbb{X}_{dt}$ is a vector of time varying controls that differ across police districts, and ϵ_{dt} is the error term. The standard errors are clustered by police district (N = 22) to allow for serial correlation within districts, although we also report wild cluster bootstrapped standard errors in our main results as recommended by Cameron et al. (2008) since the number of clusters is below 30. Intuitively, Equation 1 is comparing average response times on days with ShotSpotter activated to days without ShotSpotter activated, while accounting for the expected differences in police districts and different times of the year.

Police district fixed effects, δ_d, are included to account for the systematic, time-invariant differences between police districts. Given that Chicago's police districts have distinct baseline characteristics such as levels of wealth, crime, and potential policing tactics, adding police district fixed effects accounts for these fixed differences. Additionally, the day-by-month-by-year fixed effects, γ_t, are included to control for time-varying fluctuations that occur over particular days of each year.

Within $\mathbb{X}_{dt}$, we control for two important factors that vary between districts and over time: officer hours and the number of 911 dispatches. Each of these controls are included to ensure that the estimates are not confounded by days in which there are more police officers or a higher amount of reported crimes to respond to. As mentioned in Section 2.1, officer hours is the number of working hours by police officers within a district-day. Officer hours are preferred over number of shifts in order to account for the possibility of overtime.

3.2 Identification

The coefficient of interest is β, which measures the average change in the daily average response times between days with and without ShotSpotter technology. To identify β as a causal effect, there are several assumptions that must be satisfied: response times would have continued on a similar trend to non-Shotspotter districts in the absence of ShotSpotter (common trends), there is no change in 911 dispatching procedures post-ShotSpotter implementation, and there are no other policies that coincide with the timing of ShotSpotter that may affect response times.

The first key identification assumption is that police districts that adopt ShotSpotter would have continued to have similar response times non-ShotSpotter districts in the absence of adoption (i.e., *common trends*). Specifically, ShotSpotter adoption must not be correlated with a systematic rise or fall in response times. To address this concern, we estimate an event study framework given by the following model:

$$ResponseTime_{dt} = \sum_{\substack{i=-12,\\ i\neq -1}}^{12} \beta^i \text{ShotSpotter}^i_{dt} + \gamma_t + \delta_d + \lambda \mathbb{X}_{dt} + \varepsilon_{dt} \tag{2}$$

where $ShotSpotter^i_{dt}$ is a set of indicators that are set to 1 if ShotSpotter is adopted i months from day t in district d. Each period is relative to the month before ShotSpotter adoption. Twelve periods before and after are estimated before and after ShotSpotter implementation where the first and final periods are binned endpoints as described in Schmidheiny and Siegloch (2023). % , although only eleven periods are reported, as the last periods are binned endpoints as described in BLANK. We opt to use monthly periods instead of day periods in order to increase statistical power of each coefficient estimate and thereby reduce potential noise that arises from using small sets of data. This also allows us to explore dynamic treatment effects over a substantially longer time period.

Figures 3 and 4 show the event study estimations for call-to-dispatch and call-to-on-scene response times and display little visual evidence of an upward or downward trend prior to the implementation of ShotSpotter. The error-bars represent 95% confidence intervals while the coefficient estimates are reported in seconds. We report two sets of estimates in this visualization: the two-stage difference-in-difference imputation estimator (Gardner, 2022) and the OLS estimator. The two-stage difference-in-difference estimator is robust to the negative weights which arise in OLS estimates when there are heterogeneous treatment effects across groups and over time in staggered designs (de Chaisemartin and D'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Athey and Imbens, 2022). In each set of estimations, there appears to be little evidence of a trend prior to ShotSpotter implementation. We later enhance this visual test in Section 4.2 with a sensitivity test as described in Rambachan and Roth (2023) where we allow for relaxations of the common trends assumption.

Given that the standard operating procedures for 911 calls do not change and there are no other police department policies that directly coincide with ShotSpotter implementation, it is likely that the second and third main assumptions hold. Although ShotSpotter changes a police officer's time allocation through responding to ShotSpotter alerts, the rapid response units and beat cops responding to 911 calls remain consistent. Moreover, the only other department-wide policy that was implemented at a similar time as ShotSpotter are body worn cameras.[9] However, the district implementation of BWC, although similar, do not align with the timing of ShotSpotter. Furthermore, while body worn cameras have been found to affect complaints (Kim, 2019; Braga et al., 2022; Zamoff et al., 2022; Ferrazares, 2023), arrests, and stops (Braga et al., 2022; Zamoff et al., 2022), it is unlikely that body worn cameras affect an officers response time or overall workload.

4 Results

In this section, we present the main estimates on the effect of ShotSpotter on Priority 1 response times using Equation 1. We show that the results are robust across various

[9]Body worn cameras were implemented beginning in June 2016 and ended in December 2017.

specifications, estimators, sample selections, and sensitivity tests. Moreover, we present the first set of evidence that ShotSpotter affects response times by constraining officer resources. Later, we analyze dynamic effects and the intensive margin.

Figure 5 serves as an intuitive preview of the main results plotting only the raw data. We plot the average call-to-dispatch and call-to-on-scene times within each police district before/after ShotSpotter implementation. Consistent with the main results, districts that received ShotSpotter show a substantial increase in the average call-to-dispatch and call-to-on-scene times in nearly every police district. Notably, there does not appear to be significant visual evidence that average response times are different in districts that receive ShotSpotter in comparison to those that did not.

4.1 Main Results - Response Time Changes

Table 2 reports estimates from Equation 1 for call-to-dispatch (Panel A) and call-to-on-scene (Panel B) response times where each coefficient estimate is reported in seconds. Recall that call-to-dispatch and call-to-on-scene are the length of time from when a 911 call is received to when a police is dispatched and subsequently arrives on the scene respectively. In Column 1 of Table 2, we estimate Equation 1, first, without the addition of control variables. We find an increase in call-to-dispatch and call-to-on-scene times of 63 seconds and 110 seconds respectively. Notably, call-to-on-scene time shows that travel time is increasing by approximately one minute in addition to the delays in finding responding officers to dispatch. This suggests that ShotSpotter is not placing officers in areas closer to other crimes whereby travel time may be reduced.

Column 2 of Panels A and B, show that call-to-dispatch and call-to-on-scene times increase significantly by approximately one minute and two minutes respectively when controlling for officer hours and number of 911 dispatches. Each of these controls are included to ensure that the results are attributed to the implementation of ShotSpotter rather than changes in the number of police officers or overall 911 response workload.

Given the staggered difference-in-difference research design, Column 3 reports estimates that are robust to treatment heterogeneity across groups and over time using the two-stage difference-in-difference imputation estimator (Gardner, 2022). This estimator equally weights each district-date estimate making it less susceptible to the bias from negative weighting in the presence of treatment effect heterogeneity (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Athey and Imbens, 2022). The estimates, albeit slightly larger, remain consistent with the OLS estimates.

We consider spillover effects in Column 4 by including an indicator variable equal to one (Border Activated) for any police district that is adjacent to a ShotSpotter-activated district. In effect, the coefficient on Border Activated shows ShotSpotter's effect on adjacent police districts that may not have implemented the technology. As reported in both Panel A and Panel B, there does not appear to be evidence of spillover effects on response times. This result aligns with the standard dispatching procedures as discussed in Section 1.2—officers are only to be dispatched outside their beat/district of patrol in rare circumstances.

Finally, in Columns 5 and 6, we separate district-days that have above and below the median officer hours to further test that ShotSpotter is affecting response times by exhausting scarce police resources. Column 5 reports that when there are more officers on duty, ShotSpotter's resource-constraining effects are less apparent, exhibiting a 23 second increase in call-to-dispatch time and a 60 second increase in call-to-dispatch time respectively. On the other hand, Column 6 shows the opposite: on district-days with less police officers, the effects of ShotSpotter are more than twice as large. In particular, a district-day below the median of officer hours exhibits a 1.5 minute increase in call-to-dispatch time and a 2.5 minute increase in call-to-on-scene time. Given these large disparities, we find further evidence that these effects are driven by resource-constrained times in Section 5.1 where we analyze differences in shift times and days with higher 911 call volumes.

Importantly, the main results are robust to a variety of sample selections and sensitivity tests. First, Appendix Figure A2 shows Equation 1 for five different sample selections estimated with both OLS and the Gardner 2021 robust estimator: omitting 2020 (Covid-19 pandemic), omitting 911 calls for gun shots fired in the event that dispatchers begin to merge reports of gunfire and ShotSpotter alerts, including all outliers that are removed in the main sample, using the official activation dates from the Freedom of Information Act request rather than the observed beginning of ShotSpotter alerts, and omitting the never-treated police districts. In nearly all of these samples, the results for both response time outcomes remain consistent with the main results. The one exception is when the never-treated districts are removed. However, we attribute this inconsistency to the a loss in precision from removing approximately half the sample, and in addition, note that the point estimates still remain positive. Second, we perform a leave-one-out analysis in Appendix Figure A3 where Equation 1 is estimated 22 times with each iteration excluding a distinct police district. Given that the results remain consistent with the main findings in each iteration, we rule out the possibility that these effects are driven by only one police district.

4.2 Dynamic Effects

Next, to analyze the effect of ShotSpotter over time, we estimate an event study using Equation 2. We estimate this model using both OLS and the Gardner (2022) robust estimator to account for potential treatment heterogeneity across groups and time periods.

Figures 3 and 4 show that the effect of ShotSpotter implementation takes several months post-implementation to significantly alter call-to-dispatch and call-to-on-scene times respectively. In each figure, the red error bars represent the 95% confidence intervals using OLS while the blue error bars are estimates using the Gardner (2022) estimator. We attribute the delayed effect in response times to ShotSpotter's functionality. Specifically, ShotSpotter relies on a machine learning algorithm to detect gunfire which improves with the volume of data it receives. Therefore, the initial months of implementation may not exhibit significant effects on response times due to lower quantities of ShotSpotter alerts. As shown in previously Figure **??**, the number of ShotSpotter dispatches appears to be trending up over time across each district.

Additionally, we conduct analysis following Rambachan and Roth (2023) to illustrate the sensitivity of the estimates to possible violations of parallel trends. Specifically, we evaluate the degree of nonlinearity we can impose on a linear extrapolation of the pre-treatment trend. We adopt the notation used in Rambachan and Roth (2023) and define M as the maximum amount that the pre-treatment trend can change across consecutive periods. As an example, $M = 0$ implies no change in the post-treatment trends—the counterfactual difference in trends is exactly linear. Conversely, as M increases ($M > 0$), we allow for more nonlinearity in the pre-treatment trend and therefore greater uncertainty in the treatment effect estimates.

Since we are most interested in the average effect of ShotSpotter post-implementation, rather than one particular post-period, we perform the sensitivity analysis on the average of all post-implementation estimates obtained from Equation 2. Appendix Figures A6 and A7 report two important features: the confidence interval of the average of all post-period estimates (Original) and the corresponding robust fixed-length confidence intervals (FLCI) which show the average post-period effect under the assumption that the difference in pre-period trends can differ by up to M across consecutive periods. For both outcomes, the average of all post-implementation periods maintain their statistical significance under both a linear extrapolation of the pre-period ($M = 0$) and increasing amounts of non-linearity ($M > 0$) for both the call-to-dispatch and call-to-on-scene time.

4.3 Intensive Margin

In this subsection, we exploit an alternative source of variation to test whether ShotSpotter allocates resources away from 911 calls: the number of daily ShotSpotter dispatches. To do so, Equation 1 is modified to the following:

$$ResponseTime_{dt} = \zeta ShotSpotterDispatches_{dt} + \delta_d + \gamma_t + \lambda \mathbb{X}_{dt} + \varepsilon_{dt} \tag{3}$$

where $ShotSpotterDispatches_{dt}$ is the number of dispatches attributed to ShotSpotter alerts in district d at time t.

Consequently, this alternative specification more precisely tests the hypothesis that ShotSpotter affects response times through officer resource constraints. If this mechanism is valid, then days without ShotSpotter dispatches should see no significant change in response times since the installation of the technology does not affect other day-to-day police operations. On the other hand, a day with more ShotSpotter dispatches may allocate less time for police officers to respond to 911 calls and therefore increase response times. In effect, the coefficient of interest ζ_I measures the marginal effect of an additional ShotSpotter dispatch.

Column 1 of Table 3 shows that one additional ShotSpotter dispatch is associated with an increase in Call-to-Dispatch time of 8 seconds and an increase in Call-to-On-Scene time of 12 seconds. These results are statistically significant at the 1% level. In Column 2, the preferred specification with controls for officer hours and number of dispatches, the results remain consistent. Moreover, similar to Table 2, we split the sample by the median officer hours within district in Columns 3 and 4 and report similar findings: district-days with more police officers mitigate the delays in rapid response caused by ShotSpotter.

5 Heterogeneity

5.1 Is ShotSpotter most hindering in resource-constrained times?

In this subsection, we further test the notion that instances of limited officer resources are more prone to experiencing the higher workloads resulting from ShotSpotter implementation. As alluded to in Columns 5 and 6 of Table 2, ShotSpotter has a larger effect on response times when there are fewer officers. Remarkably, days with fewer officers working exhibit treatment effects up to three times as more than those with more officers on duty. We supplement this prior analysis by considering days and times where there are higher officer workloads. To do this, we split the sample by the median number of 911 dispatches in a district-day and by different officer shift schedules. In doing so, we isolate times where officers have higher workloads, and are therefore more likely to be delayed by ShotSpotter dispatches.

Figure 6 shows the results from these two analyses. In the left column, days are separated by the number of 911 dispatches per day. Days with a large number of dispatches represent days with higher crime levels. These high crime days show evidence of a larger increase in both Call-to-Dispatch time and Call-to-On-Scene time. Next, in the right column, dispatches are split based on the time of day that they occur, using the 3 Watches that officers work. Similarly, Watch 3 that operates during the evening, and which faces the highest levels of crime, see the largest treatment effect of each of the three watches.

NOTE TO HEATHER/KEVIN: we feel like this is kind of weak evidence, but it fits in with our story.

5.2 How does ShotSpotter affect other priority response times?

If ShotSpotter is affecting response times by depleting police resources, then 911 calls with less urgency may be relatively less affected. Recall from Section 2 that there are three main 911 call priorities: Priority 1 (immediate dispatch), Priority 2 (rapid dispatch), and Priority 3 (routine dispatch). Priorities 1 and 2 are both time-sensitive call types in which timely police response may affect the outcome of the incident. On the other hand, Priority 3 does not require rapid response.

In Figures 7 and 8 Equation 1 is estimated on response times by priority. Next, each priority is separated by the five most frequent call types. For each call type, the percent change from the mean in addition to the 95% confidence interval is plotted while the number of observations (N) and corresponding point estimate (Estimate) are shown on the y-axis. As such, each separate regression where the outcome is either Call-to-Dispatch or Call-to-On-Scene time.

Nearly all Priority 1 and Priority 2 calls exhibit increases in both Call-to-Dispatch and Call-to-On-Scene times while Priority 3 calls show no statistically significant effect. These results align with the notion that ShotSpotter affects time-sensitive calls while having little discernible impact on calls of lesser time-sensitivity, as seen in Priority 3. However, it is worth noting that Priority 3 calls are investigated when police officers have available time. Interestingly, positive point estimates are reported across the top 5 Priority 3 calls, thereby suggesting that officers may have less availability when ShotSpotter is implemented.

6 Discussion

6.1 Does ShotSpotter reduce the likelihood of catching a criminal?

Although the findings report that ShotSpotter affects police officer response times, we acknowledge that this may not be a detrimental consequence if officers' likelihood of catching perpetrators remains unchanged. In response, we conduct an analysis similar to Blanes i Vidal and Kirchmaier (2018) who find that increases in response times lowers the likelihood of a crime being cleared. Similar to this study, we provide evidence that the increased response times attributed to ShotSpotter result in a lower likelihood of perpetrators being arrested when responding to 911 calls.

To begin, we merge the 911 dispatch data to arrest data using the incident report number.[10] Importantly, not every arrest includes an incident report number. Based on conversations with the Chicago Police Department, officers may not always fill out an incident report number when making an arrest. For instance, many arrests that are made on an arrest warrant do not contain an incident report number. Therefore, we consider the following results to be lower bounds on the true effect of ShotSpotter on arrest rates.

Table 4 shows the results from estimation of Equation 1 using the arrest rate for Priority 1 dispatches as the outcome variable. Column 1 reports that arrest rates decrease by 12%

[10] We use two sets of arrest data. Arrests from the arrest database, and also case reports that end in arrests.

relative to the mean. This finding is statistically significant at the 1% level, and consequently, exhibits that the rapid response delays are costly for civilians.

To isolate which calls demonstrate the largest declines in likelihood of arrest, Columns 2 through 4 report the arrest rate for the three most frequent Priority 1 call types that end in arrests: domestic battery, domestic disturbance, and battery. Column 2 shows that the lower arrest rates are driven by calls regarding domestic battery (16% increase).

In Column 5, we test whether ShotSpotter increases the likelihood of an arrest for gun-related crimes. Although ShotSpotter decreases the likelihood of arrest in several non-gun-related crimes, it may be the reverse for gun-related crimes. For instance, officers may respond faster to reports of gun incidents if ShotSpotter has already placed them closer to the crime scene. To test this, we combine three call descriptions to create the Gun Crime outcome: person with a gun, gunshots fired, and person shot. As a result, this indirectly measures whether ShotSpotter is achieving its primary goal in inhibiting gun violence.

Column 5 reports some suggestive evidence that officers may be arresting gun-wielding perpetrators with higher success. Although imprecise, the point estimates are positive. This imprecision is likely due to lack of statistical power as the confidence intervals contain effects between a decrease of 3% and an increase in 19%.

6.2 Are injuries more likely?

We want to replicate the DeAngelo et al. (2023) study in which they look at the probability of an injury due to longer response times. We're currently waiting on the FOIA to come back from the Chicago Police. Chicago is very mad at Michael right now after abusing their FOIA laws.

6.3 Does ShotSpotter reduce Gun Crime?

6.4 Does Shotspotter perform as intended?

[Section work in progress]

In this section, we want to do something that sheds ShotSpotter is a possible better light. There are only a couple things we can do. First, we have results on arrests not-related to 911 calls. In these results, we're finding significant decreases in arrests everywhere except gun-violations (Not yet shown here). We think the story here could be that police officers are spending so much time on ShotSpotter, that they're not arresting other people (as shown in our previous section). However, because they're pouring resources into ShotSpotter, they are consequently coming up with more gun-related arrests. Initially, we were a little worried about presenting these results because of the declines we see in other arrests. However, in light of the previous discussion section, we think this might be a nice complement.

Second, we can also give raw summary statistics on ShotSpotter dispatches themselves. This won't be causal, but we can shed light on the proportion of ShotSpotter alerts that end in firearm retrievals, or something of this sort.

To explore the effectiveness of individual Shotspotter alerts we rely on data from the Chicago Office of Inspector General, which matches ShotSpotter Alerts to enforcement actions of the associated dispatch. This reports covers a 18-month period beginning January 2020 and is supplemented with our own FOIA data. Using this additional data source is necessary, since ShotSpotter alerts do not match to final enforcement actions of dispatches in the publicly available data.

During this 18-month period 50,176 Shotspotter alerts were created, of which 1,065 were linked to a police stop of an individual (2.1% of alerts). These 1,065 stops accounted for 342 civilian searches, 244 arrests, and 152 total firearms recovered. This equates to a rate of 0.005 arrests per alerts and 0.003 firearms per alert. While the rate of finding firearms is low, 152 firearms represents 25.1% of the 606 firearms recovered over this time period from all police searches.

These 1,065 stops account for 2.2% of all stops during this time period. Searches that arise from a ShotSpotter Alert are more likely to result in an arrest compared to non-ShotSpotter searches (23.1% versus 10.8%). This is somewhat expected, as a search that arises from a ShotSpotter dispatch is likely more evidence-based than an average street stop.

6.5 Cost-Benefit Analysis

This section is only conditional on whether we get some good evidence in support of ShotSpotter.

7 Conclusion

Importantly, we do not rule out that ShotSpotter may have its merits in other settings. Recall that the results are driven by the resource-constraints of the Chicago Police Department such as officer availability and overall workload. Therefore, ShotSpotter may be an effective technology to reduce gun violence provided police departments have the necessary accommodations.

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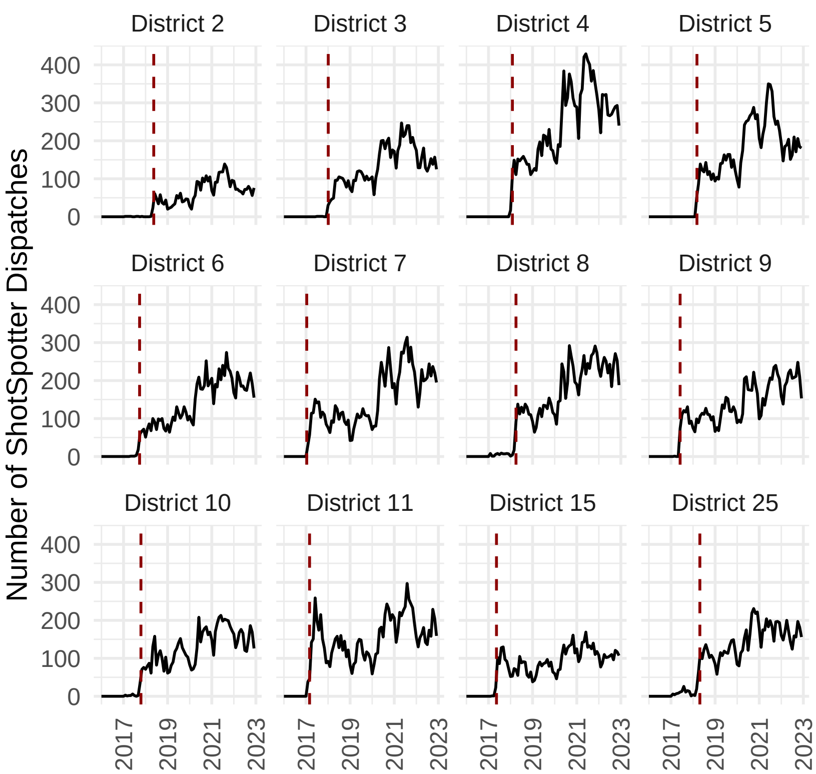
8 Figures



District	Enactment
2	2018−05−16
3	2018−01−04
4	2018−02−01
5	2018−03−07
6	2017−09−24
7	2017−01−13
8	2018−04−01
9	2017−06−01
10	2017−10−16
11	2017−03−01
15	2017−05−13
25	2018−04−24

Figure 1: ShotSpotter Alert Trends and Enactment Dates

Note: This figure depicts police districts that are imiplemented with ShotSpotter technology. Months are on the x-axis, while the y-axis is the number of ShotSpotter dispatches aggregated to the monthly level. The table on the right shows the corresponding implementation date for ShotSpotter technology. In Chicago, 12 of the 22 police districts have ShotSpotter technology. The dashed red line shows the implementation dates used in the main results. In some cases, the implementation date we use differs from the date given from the Chicago Police Department since the ShotSpotter dispatches data does not align. Analysis using public records date is shown in Appendix Figure BLANK.

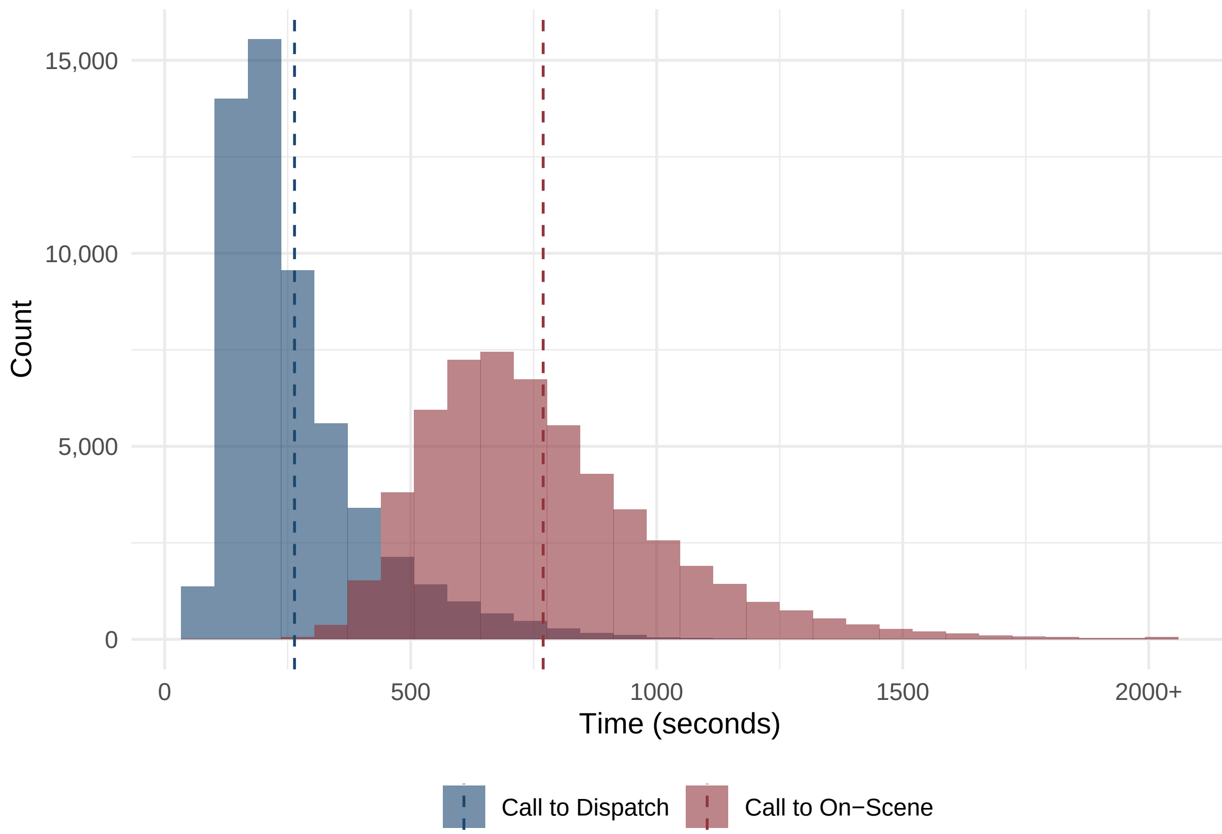


Figure 2: Distribution of Outcome Variables
Note: The two plotted variables are Call-to-Dispatch and Call-to-On-Scene. Call-to-Dispatch is time it takes for a police officer to be dispatched to the scene of the reported crime from the time of the 911 call. Call-to-On-Scene is the time from a 911 call to the time a police officer arrives at the scene of the reported crime. This sample excludes outliers that are greater than three standard deviations from the mean for each outcome. However, the main results remain consistent when including these outliers as shown in Appendix Figure BLANK.

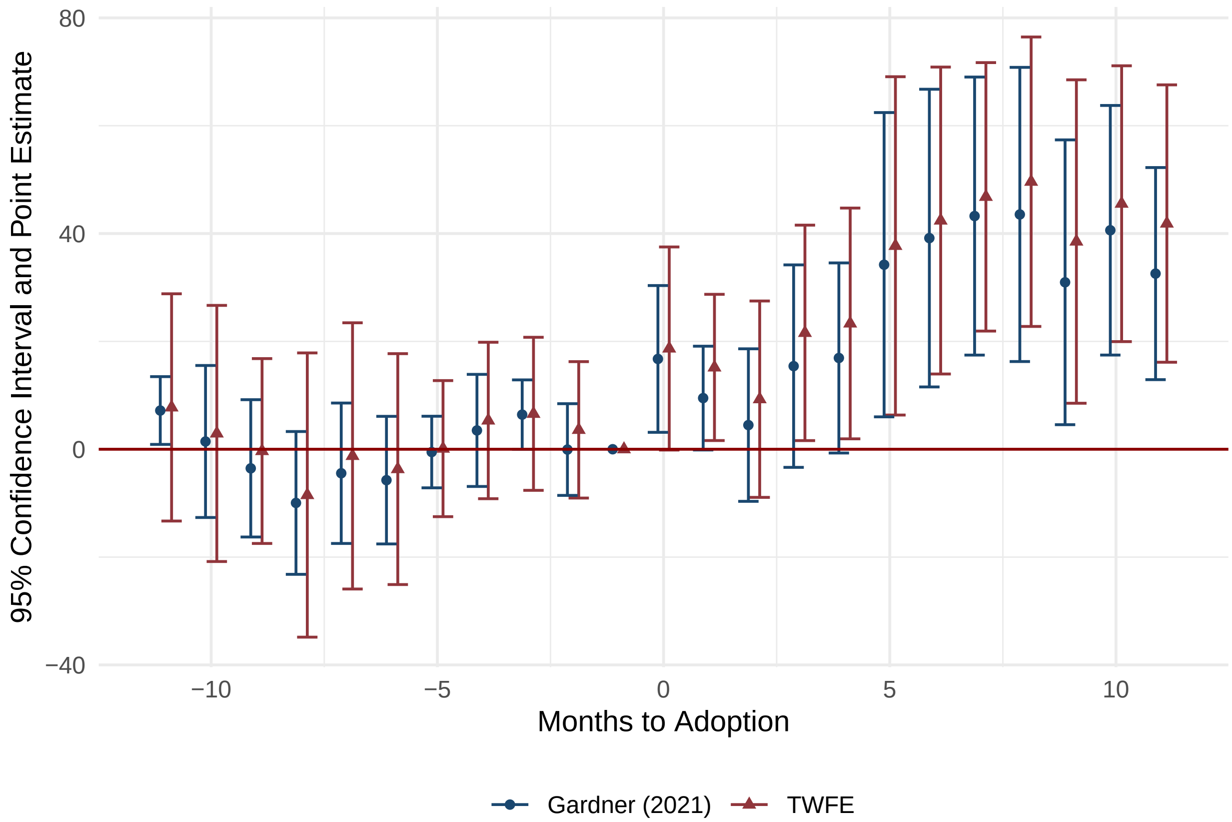


Figure 3: Event Study (Call-to-Dispatch)

Note: This figure shows the event study as specified in Equation 2 for Call-to-Dispatch times. Call-to-Dispatch is the amount of time from a 911 call to a police officer being dispatched to the crime scene. The x-axis denotes the number of months pre/post adoption of ShotSpotter technology. The y-axis denotes the 95% confidence intervals and point estimates (in seconds). The red errorbars/points represent confidence intervals/point estimates from OLS estimation while the blue are from Gardner (2022) two-stage difference-in-difference estimators which are robust to heterogeneous treatment effects in staggered adoptions. All pre/post periods are normalized by the month before ShotSpotter adoption. Twelve periods are estimated, but only 11 pre-periods and 11 post-periods are reported as the -12 and +12 are binned endpoints. Controls are synonymous with the preferred specification. Standard errors are clustered at the district level.

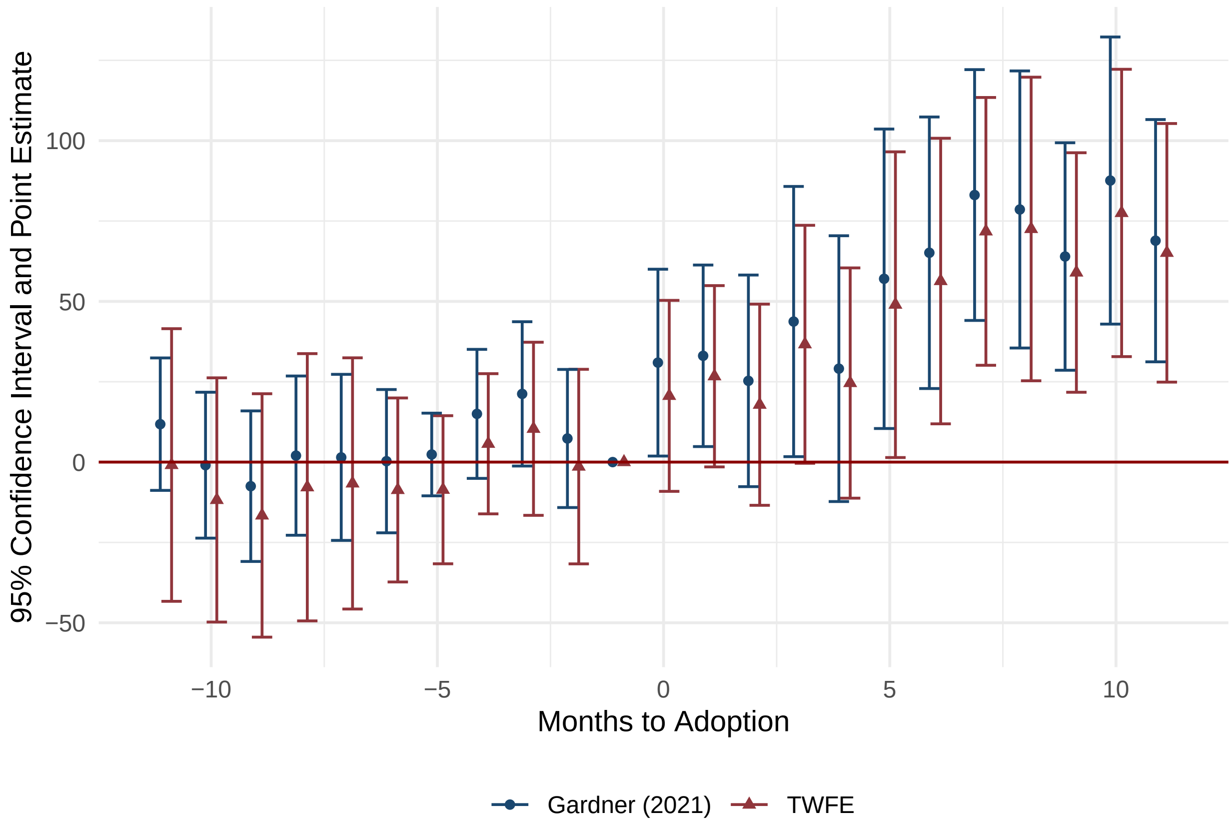


Figure 4: Event Study (Call-to-On-Scene)

Note: This figure shows the event study as specified in Equation 2 for Call-to-On-Scene times. Call-to-On-Scene is the amount of time from a 911 call to a police officer arriving to the crime scene. The x-axis denotes the number of months pre/post adoption of ShotSpotter technology. The y-axis denotes the 95% confidence intervals and point estimates (in seconds). The red errorbars/points represent confidence intervals/point estimates from OLS estimation while the blue are from Gardner (2022) two-stage difference-in-difference estimators which are robust to heterogeneous treatment effects in staggered adoptions. All pre/post periods are normalized by the month before ShotSpotter adoption. Twelve periods are estimated, but only 11 pre-periods and 11 post-periods are reported as the -12 and +12 are binned endpoints. Controls are synonymous with the preferred specification. Standard errors are clustered at the district level.

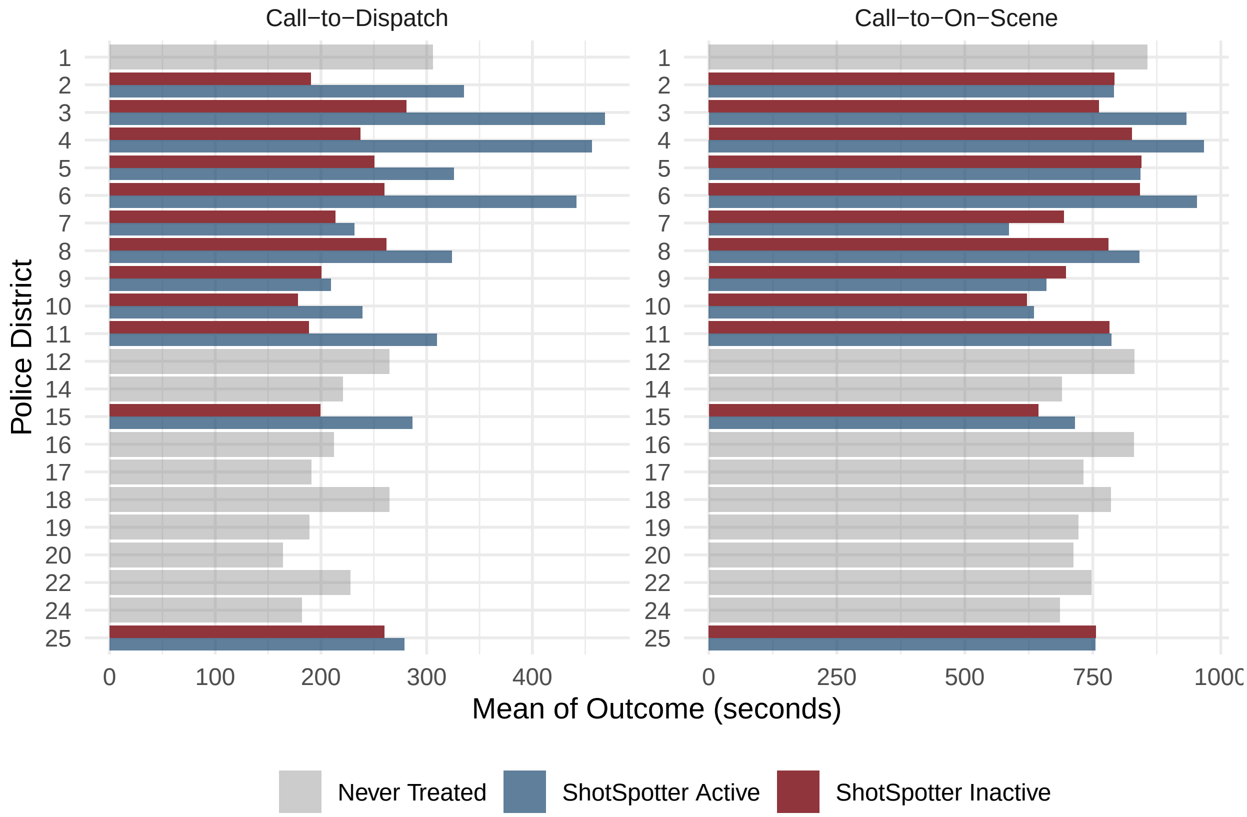


Figure 5: Average Outcomes in Police Districts

Note: Each police district is plotted on the y-axis and the average of each Priority 1 Call-to-Dispatch and Call-to-On-Scene (seconds) is on the x-axis. There are three groupings: Never Treated, ShotSpotter Active, and ShotSpotter Inactive. Never Treated refers to police districts that never received ShotSpotter technology and are plotted in light grey. All ShotSpotter-implemented districts have two distinctions: ShotSpotter Active and ShotSpotter Inactive. The red bars show prior to ShotSpotter implementation, and the blue bars show post-implementation. There are 12 of 22 police districts in Chicago that receive ShotSpotter technology.

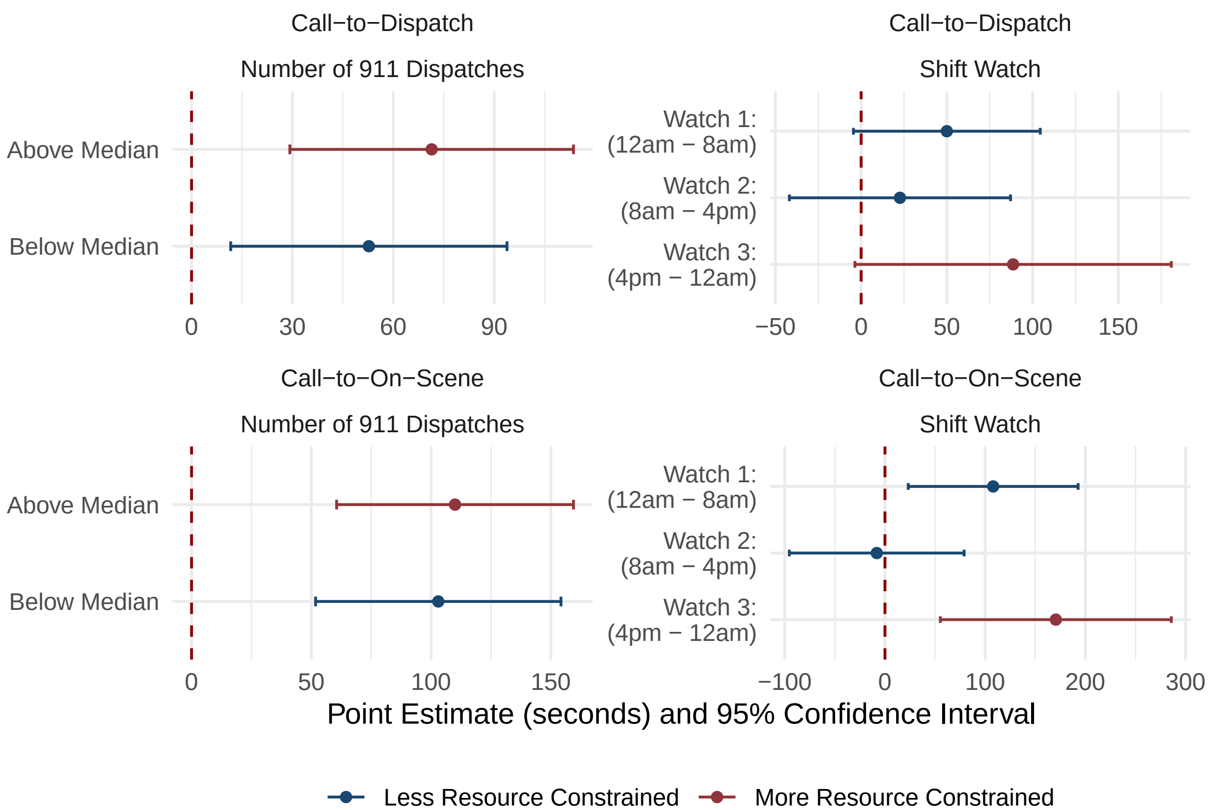


Figure 6: Effect of ShotSpotter by Watch and 911 Dispatches

Note: Two outcomes are pictured here: Call-to-Dispatch and Call-to-On-Scene. Moreover, there are two separate analyses for each outcome: shift watches and median splits of 911 dispatches. Each of these tests show that ShotSpotter delays are more apparent in times where police officers are more time-constrained. For instance, more crimes tend to occur in Watch 3 (4:00pm - 12:00am) than in Watch 1 (12:00am-8:00am) or Watch 2 (8:00am-4:00pm). OLS estimates are shown along with 95% confidence intervals using the specification in Equation 1.

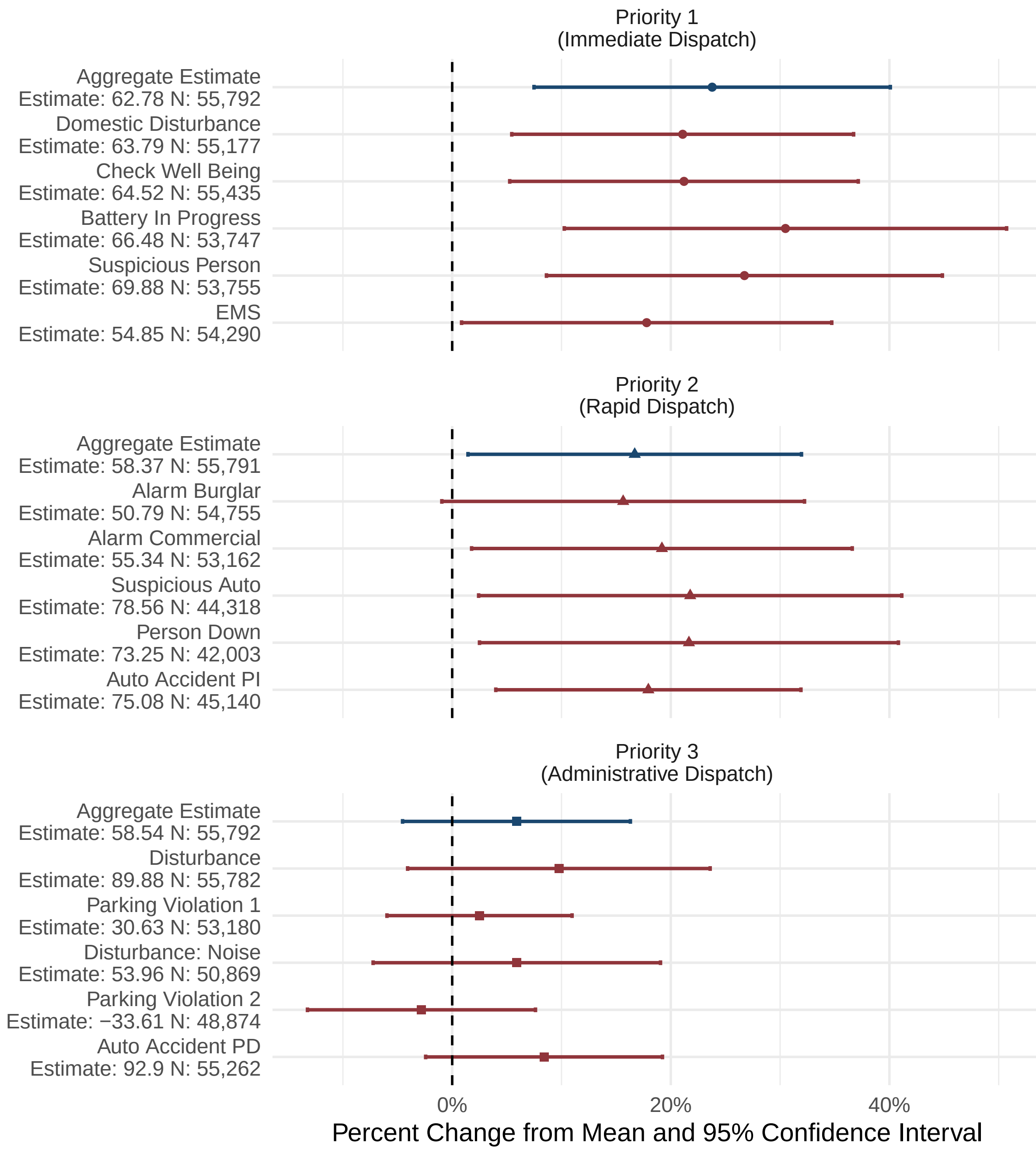


Figure 7: Effect of ShotSpotter by Priority (Call-to-Dispatch)

Note: This figure plots the effects of ShotSpotter on Call-to-Dispatch times by priority. At the top of each graph, the estimate combining all respective dispatch call types are displayed (Aggregate Estimate). The other five estimates are for the top five call types corresponding to each priority. For instance, Domestic Disturbance is the most frequenty Priority 1 dispatch. The x-axis shows the percent change from the mean (i.e., the point estimate divded by the mean of the outcome), as well as the corresponding 95% confidence interval using the specification from Equation 1. Note that the data is at the district-day level. Because of this, call-types have missing data when there are no dispatches for a particular type of call in a district-day. The number of observations are shown in the y-axis (N) while the estimated point estimate (in seconds) is also reported (Estimate).

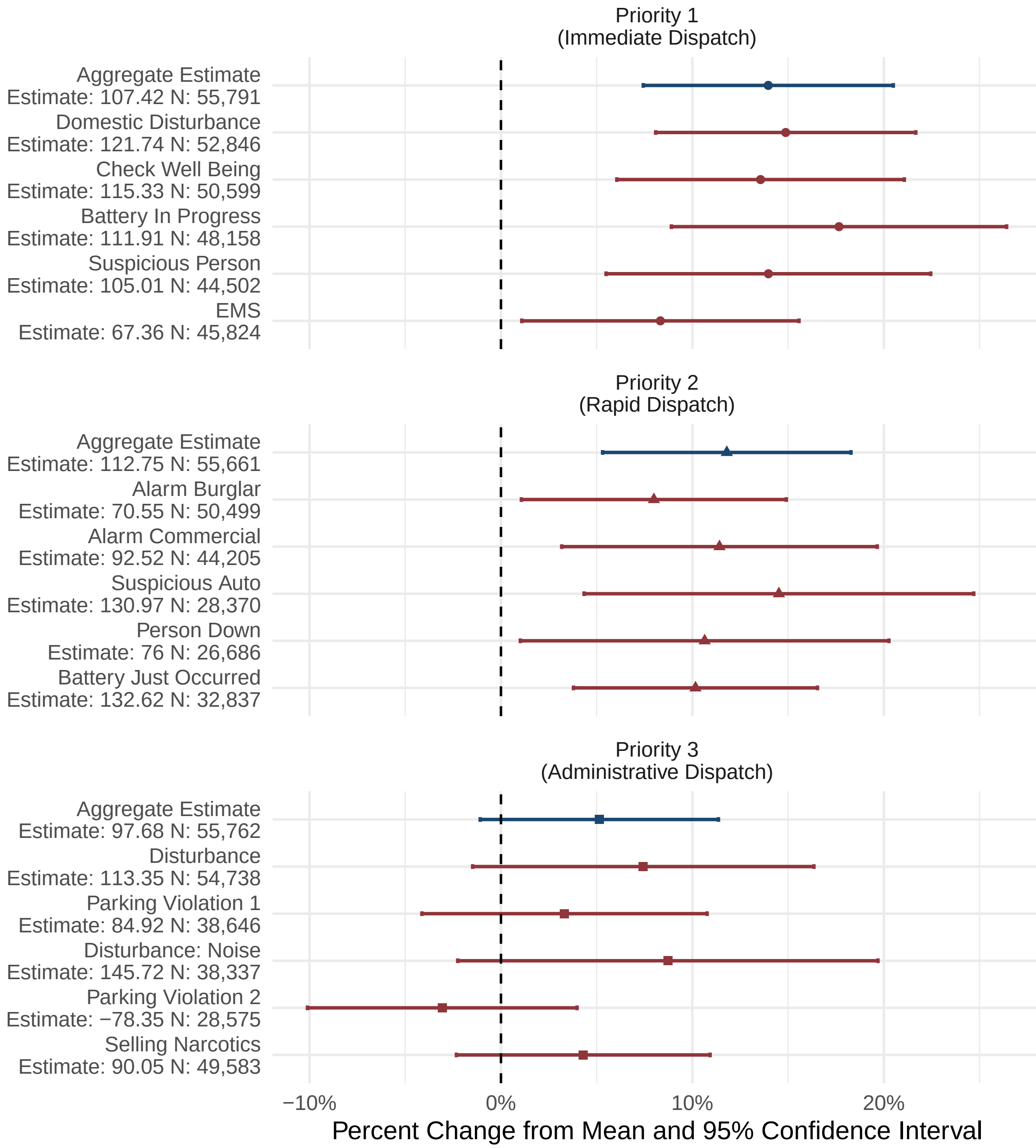


Figure 8: Effect of ShotSpotter by Priority (Call-to-On-Scene)

Note: This figure plots the effects of ShotSpotter on Call-to-On-Scene times by priority. At the top of each graph, the estimate combining all respective dispatch call types are displayed (Aggregate Estimate). The other five estimates are for the top five call types corresponding to each priority. For instance, Domestic Disturbance is the most frequenty Priority 1 dispatch. The x-axis shows the percent change from the mean (i.e., the point estimate divded by the mean of the outcome), as well as the corresponding 95% confidence interval using the specification from Equation 1. Note that the data is at the district-day level. Because of this, call-types have missing data when there are no dispatches for a particular type of call in a district-day. The number of observations are shown in the y-axis (N) while the estimated point estimate (in seconds) is also reported (Estimate).

9 Tables

Table 1: Summary Statistics of Response Times (seconds)

	Mean	Std. Dev.	Median	Min	Max
Main Outcomes:					
Call-to-Dispatch (Priority 1)	263.94	147.97	220.50	40.82	1,298.17
	(4.40 mins)	(2.47 mins)	(3.67 mins)	(0.68 mins)	(21.64 mins)
Call-to-On-Scene (Priority 1)	769.28	248.77	723.62	103.00	5,577.00
	(12.82 mins)	(4.15 mins)	(12.06 mins)	(1.72 mins)	(92.95 mins)
Controls/Secondary Outcomes:					
Number Dispatches	151.84	48.97	145.00	34.00	449.00
Priority 1	64.21	23.77	61.00	8.00	223.00
Priority 2	28.76	11.04	28.00	0.00	126.00
Priority 3	58.86	23.86	55.00	8.00	278.00
Number Arrests	2.43	1.92	2.00	0.00	14.00
Arrest Rate	0.02	0.01	0.01	0.00	0.12
Number SST Dispatches	2.02	3.37	0.00	0.00	55.00
Officer Hours	1,205.34	316.58	1,196.00	200.50	3,431.50
Number Gun Victimizations	0.36	0.70	0.00	0.00	8.00

Note:
Units are in seconds unless otherwise noted. Data is at the district-by-day level. Call-to-Dispatch represents the amount of time from the 911 call to an officer dispatching to the scene. Call-to-On-Scene is the time from a 911 call to when an officer arrives on scene. Priority 1 refers to an immediate dispatch, Priority 2 a rapid dispatch, and Priority 3 a routine dispatch. Officer Hours are the number of working hours sworn police officers work. Number of SST Dispatches is the number of dispatches due to ShotSpotter alerts. Importantly, Number of SST Dispatches is also at the district-by-day level and includes days in which ShotSpotter is not implemented. The average number of ShotSpotter dispatches across Chicago once all 12 districts have implemented ShotSpotter is approximately 60. Note that New Years Eve/New Years Day/Fourth of July are excluded from the sample as ShotSpotter alerts can be as high as 392 on these days.

Table 2: Effect of ShotSpotter on Response Times (OLS)

					Officer Hours	
					> Median	<= Median
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Call to Dispatch						
ShotSpotter Activated	63.795***	62.726***	71.918***	60.780***	26.468**	90.669***
	(21.442)	(20.670)	(21.602)	(20.073)	(11.582)	(29.448)
Border District Activated				13.414		
				(15.304)		
Mean of Dependent Variable	263.941	263.941	263.941	263.941	215.487	312.299
Observations	55,792	55,792	55,792	55,792	27,868	27,924
Wild Bootstrap P-Value	0.008	0.003		0.006	0.062	0.001
Panel B: Call to On-Scene						
ShotSpotter Activated	110.979***	107.360***	124.753***	105.088***	60.523***	142.086***
	(25.104)	(24.145)	(25.360)	(23.653)	(19.898)	(32.288)
Border District Activated				15.667		
				(18.081)		
Mean of Dependent Variable	769.284	769.284	769.284	769.284	710.531	827.917
Observations	55,791	55,791	55,791	55,791	27,867	27,924
Wild Bootstrap P-Value						
FE: Day-by-Month-by-Year	X	X	X	X	X	X
FE: District	X	X	X	X	X	X
Control Variables		X	X	X	X	X
Gardner (2022) Robust			X			

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. Shotspotter is activated in 12 of the 22 police districts in Chicago. Panel A shows results for Call-to-Dispatch while Panel B shows results for Call-to-On-Scene. Column 1 reports no controls, and only fixed effects. Controls in all other columns include officer hours and number of 911 dispatches. Column 2 reports the preferred specification from Equation 1. Column 3 reports estimates using the Gardner (2022) estimator which is robust to heterogeneous treatment effects across groups and time periods in staggered designs. Column 4 includes Border District Activated which is an indicator for when a police district is adjacent to a ShotSpotter implemented district. Wild cluster bootstrap p-values are also reported as the number of clusters (22) is below the threshold of 30 put forth in Cameron et al. (2008). Columns 5 and 6 split the sample by district median levels of officer hours. Observations for Call-to-On-Scene do not exactly match Call-to-Dispatch since there is one district-day that is missing information for Call-to-On-Scene.

Table 3: Effect of Number of ShotSpotter Alerts on Response Times (OLS)

			Officer Hours	
			> Median	<= Median
	(1)	(2)	(3)	(4)
Panel A: Call-to-Dispatch				
Number SST Dispatches	7.743***	7.212**	3.888*	6.851***
	(2.489)	(2.569)	(2.072)	(1.695)
Mean of Dependent Variable	263.941	263.941	240.245	287.635
Observations	55,792	55,792	27,895	27,897
Panel B: Call-to-On-Scene				
Number SST Dispatches	11.810***	10.857***	7.493***	8.787***
	(2.706)	(2.759)	(2.542)	(1.923)
Mean of Dependent Variable	769.284	769.284	723.041	815.525
Observations	55,791	55,791	27,895	27,896
FE: Day-by-Month-by-Year	X	X	X	X
FE: District	X	X	X	X
Control Variables		X	X	X

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. Number SST Dispatches refers to the number of ShotSpotter dispatches that occur within a district-day. All coefficient estimates are in seconds. Panel A reports results for Call-to-Dispatch while Panel B reports results for Call-to-On-Scene. Call-to-Dispatch is the amount of time from a 911 call to when a police officer is dispatched to the scene of the crime. Call-to-On-Scene is the time from a 911 call to the time a police officer arrives on-scene. In Column 1, the controls of officer hours and number of 911 dispatches are not included. Column 2 shows the preferred specification, while Columns 3 and 4 split the sample by median number of officer hours within districts to show that response times are driven by resource-constrained time periods. Observations for Call-to-On-Scene do not exactly match Call-to-Dispatch since there is one district-day that is missing information for Call-to-On-Scene.

Table 4: Effect of ShotSpotter Enactment on Arrest Rates (OLS)

		Arrest Rate by Most Frequent Arrest Calls			
	Arrest Rate	Domestic Battery	Domestic Disturbance	Battery	Gun Crimes
	(1)	(2)	(3)	(4)	(5)
ShotSpotter Activated	-0.002***	-0.010***	-0.003**	-0.004*	0.003
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)
Mean of Dependent Variable	0.025	0.064	0.020	0.044	0.036
Observations	55,792	49,999	55,177	53,747	52,128
FE: Day-by-Month-by-Year	X	X	X	X	X
FE: District	X	X	X	X	X

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. Arrest Rate is defined as the number of arrests made divided by the number of dispatches. Columns 2-4 report the top 3 most frequent calls that end in arrests: Domestic Battery, Domestic Disturbance, and Battery. Column 5 reports arrest rates for Gun Crimes which is any call corresponding to a person with a gun, shots fired, or a person shot. Observations are not consistent across each call type since not every type of call occurs on every district-day. Controls of officer hours and number of dispatches are included in all specifications. As mentioned in Section BLANK, not every arrest is included in the data, and therefore, these estimates represent a lower bound.

A Appendix Figures

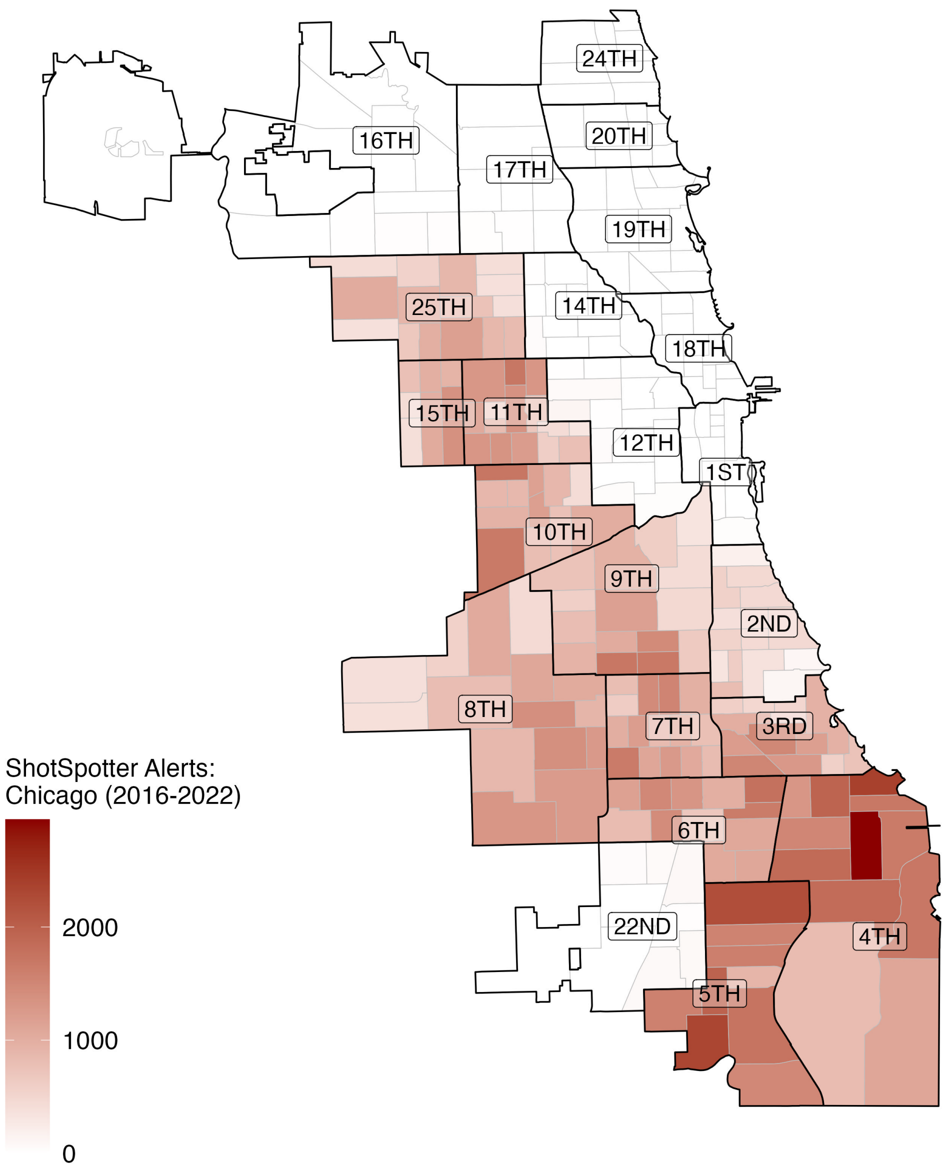


Figure A1: Map of ShotSpotter Districts in Chicago
Note: There are 22 police districts in Chicago, and 12 are equipt with ShotSpotter technology. Each district contains beat which are designated by the boxes within the district lines. ShotSpotter implmentation began in January 2017 and ended in May 2018.

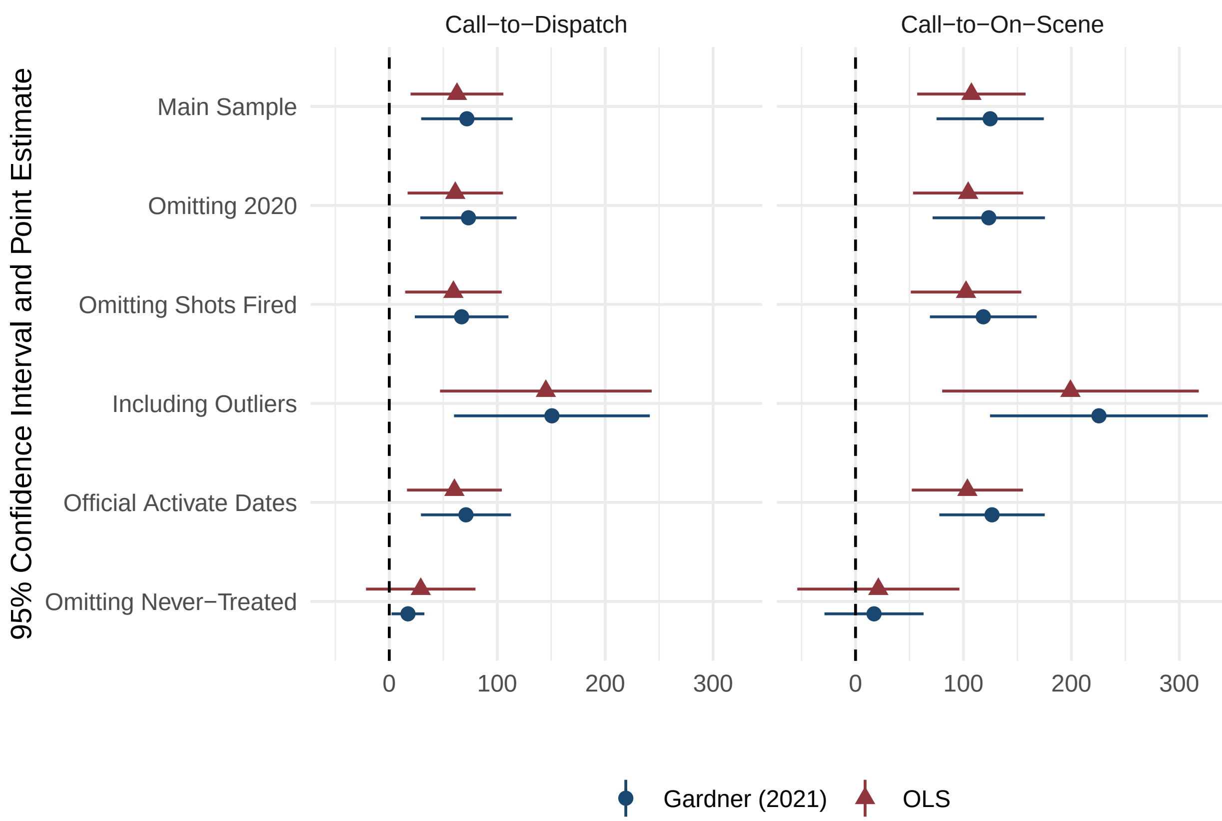


Figure A2: Robustness of Main Results

Note: This figure shows the results from estimation of Equation 1 with six different samples for both Call-to-Dispatch and Call-to-On-Scene. Main Sample refers to the main sample used in the paper. Omitting 2020 uses the main specification in the paper, but omits the year 2020 due to Covid-19. Omitting Shots Fired omits any 911 call dispatches related to the description of Shots Fired in case dispatchers begin combining reports of gun fire with ShotSpotter alerts. Including Outliers includes all outliers that are removed from the main analysis (+3 standard deviations from the mean). Official Activate Dates uses the official ShotSpotter activation dates as received from a Freedom of Information Request from the Chicago Police Department. These dates are similar, but not exact to the dates we use due to what we observe in the data. Last, Omitting Never-Treated uses the full sample, but omits any police districts that did not receive ShotSpotter technology.

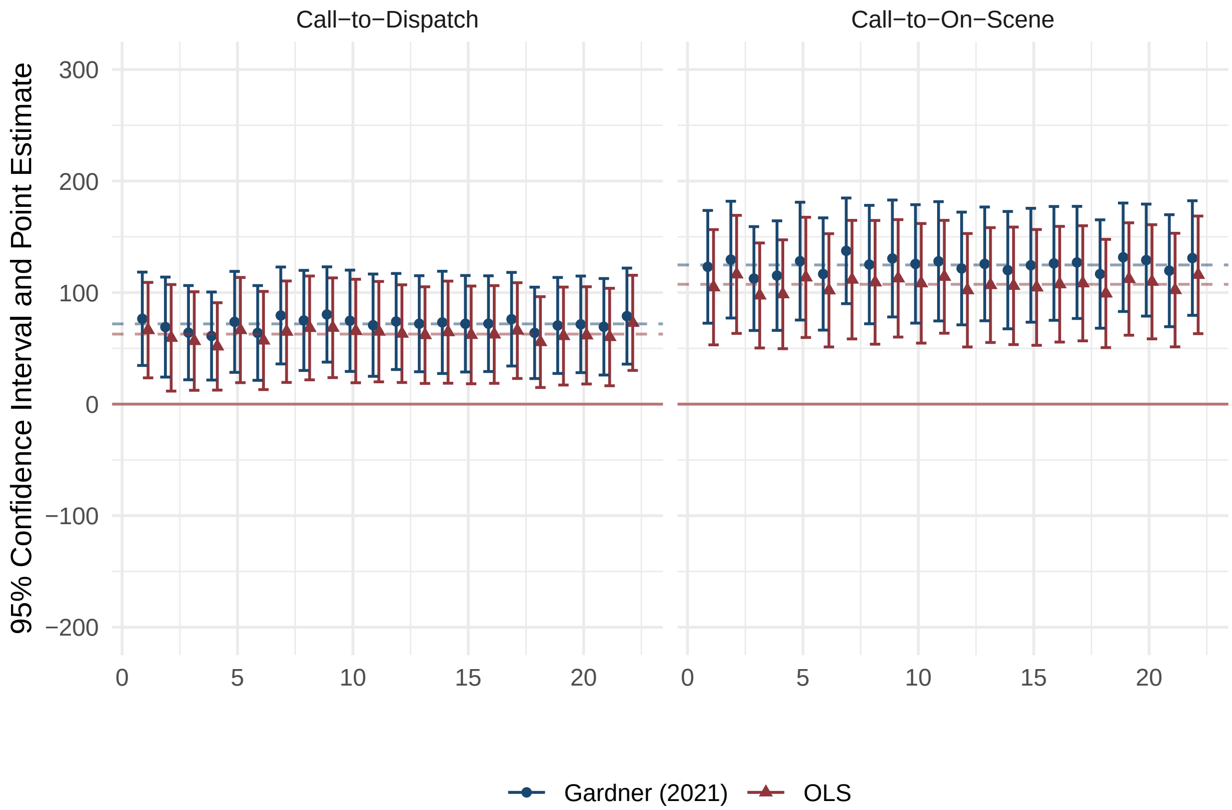


Figure A3: Leave-One-Out Analysis

Note: This figure shows the results from 22 distinct OLS and Gardner (2022) regressions using Equation 1. Both outcomes of Call-to-Dispatch and Call-to-On-Scene are pictured. In each iteration, one police district is removed from estimation to ensure that the effects of ShotSpotter are not driven by one district. The blue points and errorbars represent Garnder (2022) point estimates and 95% confidence intervals, which are robust to hetereogeneous treatment effects in staggered designs. The red points and lines denote point estimates and 95% confidence intervals from OLS estimates. Standard errors are clustered at the district level. The dashed blue lines represent the average estimate under the Gardner (2022) estimate while the dashed red lines indicate the average estimate using OLS.

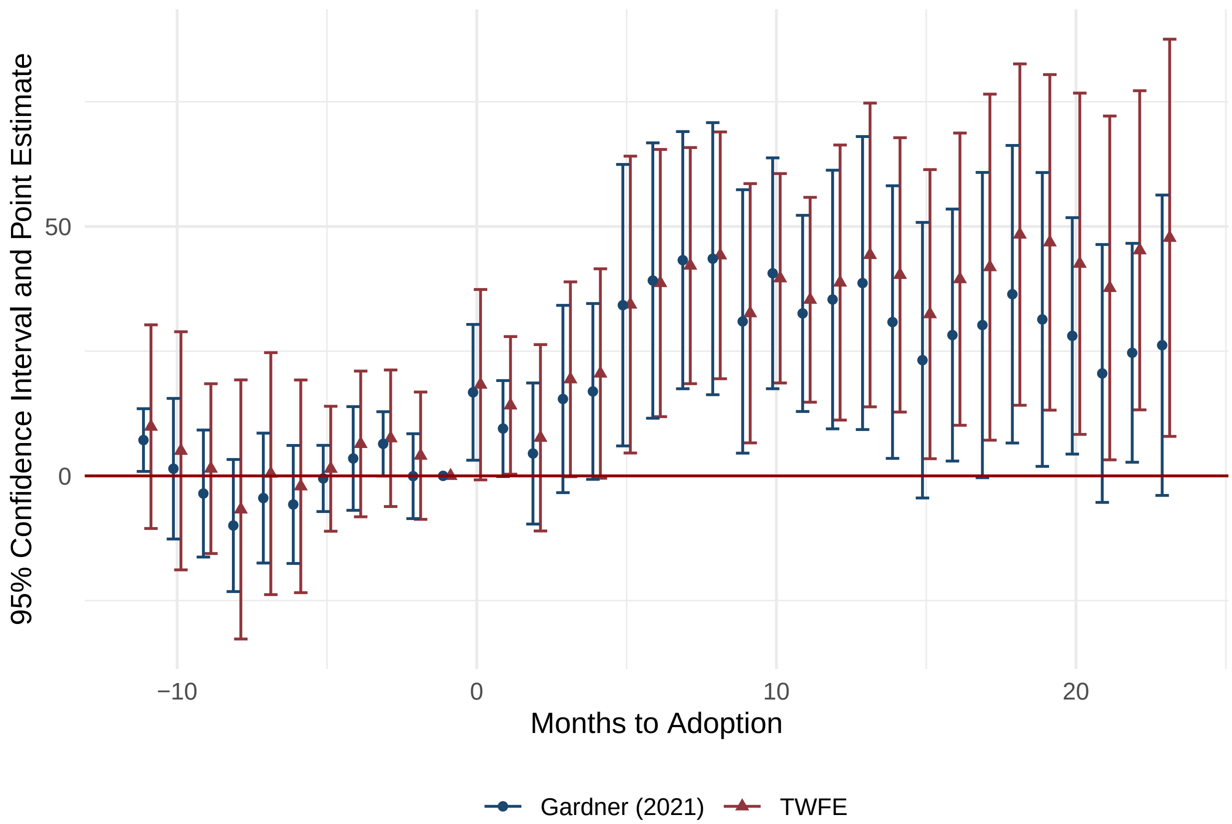


Figure A4: Event Study Longer Time Horizon (Call-to-Dispatch)
Note: This figure shows the event study as specified in Equation 2 for Call-to-Dispatch times. Call-to-Dispatch is the amount of time from a 911 call to a police officer being dispatched to the crime scene. The x-axis denotes the number of months pre/post adoption of ShotSpotter technology. The y-axis denotes the 95% confidence intervals and point estimates (in seconds). The red errorbars/points represent confidence intervals/point estimates from OLS estimation while the blue are from Gardner (2022) two-stage difference-in-difference estimators which are robust to heterogeneous treatment effects in staggered adoptions. All pre/post periods are normalized by the month before ShotSpotter adoption. Thirty six periods are estimated, but only 11 pre-periods and 23 post periods are reported as the -12 and +24 are binned endpoints. Controls are synonymous with the preferred specification. Standard errors are clustered at the district level.

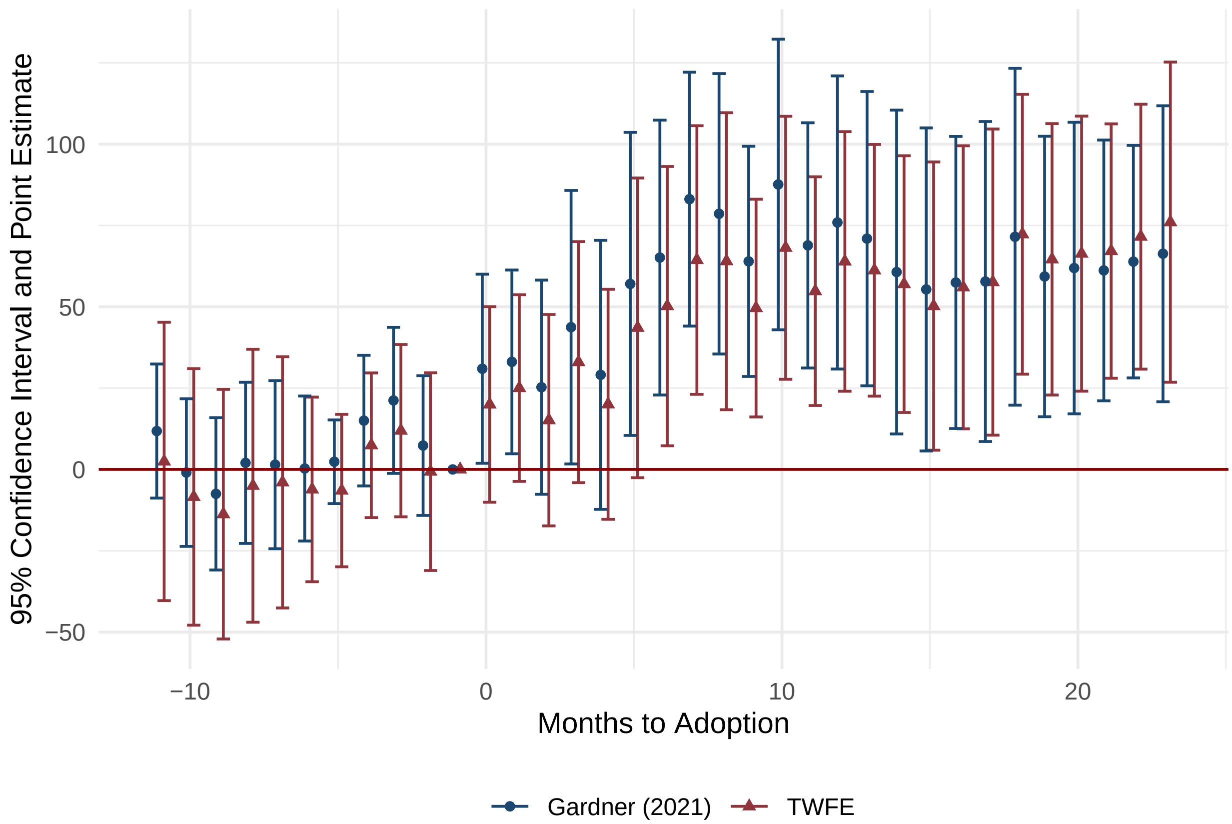


Figure A5: Event Study Longer Time Horizon (Call-to-On-Scene)

Note: This figure shows the event study as specified in Equation 2 for Call-to-On-Scene times. Call-to-On-Scene is the amount of time from a 911 call to a police officer arriving to the crime scene. The x-axis denotes the number of months pre/post adoption of ShotSpotter technology. The y-axis denotes the 95% confidence intervals and point estimates (in seconds). The red errorbars/points represent confidence intervals/point estimates from OLS estimation while the blue are from Gardner (2022) two-stage difference-in-difference estimators which are robust to heterogeneous treatment effects in staggered adoptions. All pre/post periods are normalized by the month before ShotSpotter adoption. Thirty six periods are estimated, but only 11 pre-periods and 23 post periods are reported as the -12 and +24 are binned endpoints. Controls are synonymous with the preferred specification. Standard errors are clustered at the district level.

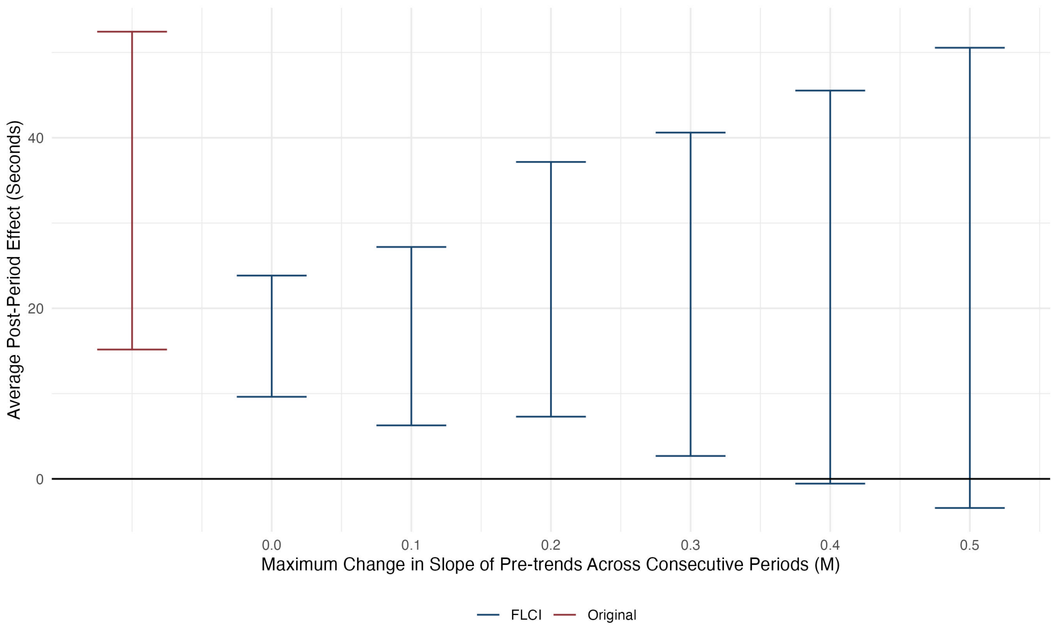


Figure A6: Sensitivity Analysis of Pre-Trends

Note: This figure shows sensitivity analysis of the event study plot in Figure BLANK. The x-axis shows the maximum change in slope of pre-trends across consecutive periods (M). We gradually increase M where M = 0 corresponds to allowing a linear trend and M > 0 allows for increasingly more varied nonlinear trends. In red, the average of the post-implementation periods are plotted. In blue, alternative Fixed-Length Confidence Intervals (FLCI), averaged over all post-implementation periods, that are proposed by Rambachan and Roth (2022) are plotted which relaxes the parallel trends assumption and requires only that differential trends evolve smoothly over time. Note that here, the breakdown value is 0.4 which means the significant effects observed in the post-implementation periods are only valid if we allow for the change in slope of the pre-period to change by no more than 0.4.

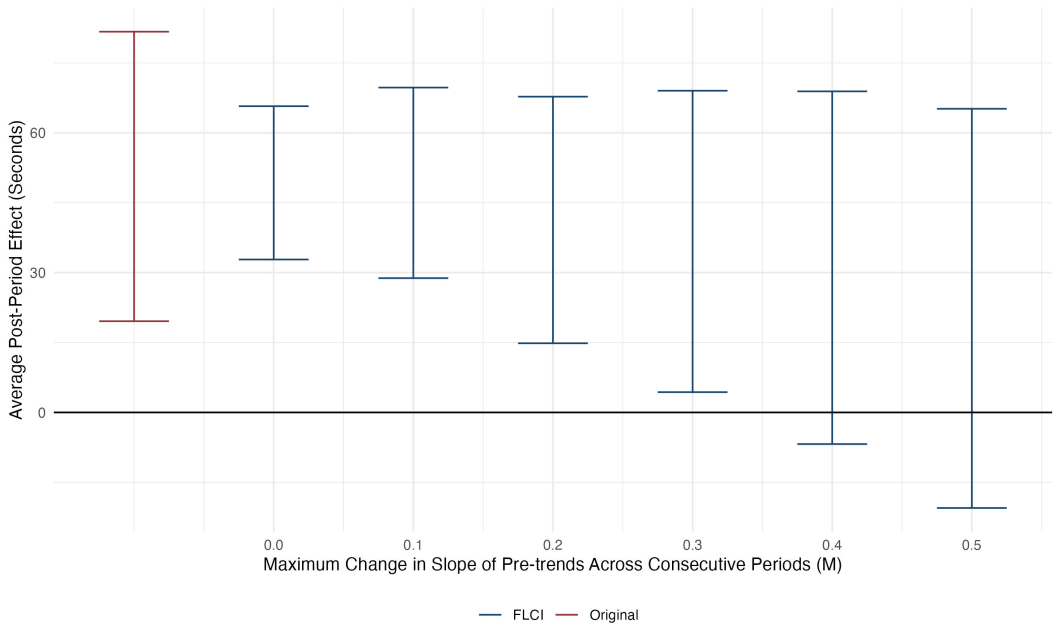


Figure A7: Sensitivity Analysis of Pre-Trends (Call-to-On-Scene)

Note: This figure shows sensitivity analysis of the event study plot in Figure BLANK. The x-axis shows the maximum change in slope of pre-trends across consecutive periods (M). We gradually increase M where M = 0 corresponds to allowing a linear trend and M > 0 allows for increasingly more varied nonlinear trends. In red, the average of the post-implementation periods are plotted. In blue, alternative Fixed-Length Confidence Intervals (FLCI), averaged over all post-implementation periods, that are proposed by Rambachan and Roth (2022) are plotted which relaxes the parallel trends assumption and requires only that differential trends evolve smoothly over time. Note that here, the breakdown value is 0.4 which means the significant effects observed in the post-implementation periods are only valid if we allow for the change in slope of the pre-period to change by no more than 0.4.

B Appendix Tables

Table A1: Proportion of Missing Call-to-On-Scene Data (OLS)

	(1)	(2)
Panel A: Missing Call-to-On-Scene		
ShotSpotter Activated	0.031	0.033
	(0.020)	(0.023)
Mean of Dependent Variable	0.526	0.526
Observations	56,254	56,254
Panel B: Number Dispatches		
ShotSpotter Activated	-3.381	-3.523
	(2.207)	(2.517)
Mean of Dependent Variable	151.839	151.839
Observations	55,792	55,792
Panel C: Officer Hours		
ShotSpotter Activated	-37.064	-57.698*
	(22.748)	(25.158)
Mean of Dependent Variable	1205.342	1205.342
Observations	55,792	55,792
FE: Day-by-Month-by-Year	X	X
FE: District	X	X
Gardner (2022) Robust		X

Note:
* p < 0.1, ** p < 0.05, *** p < 0.01
Standard errors are clustered by district. Each panel refers to a distinct outcome variable. Missing Call-to-On-Scene is the proportion of 911 call dispatches that have missing on-scene times. Number Dispatches is the number of 911 dispatches. Officer Hours is the number of police officer hours. ShotSpotter Activated refers to the timing in which each district receives ShotSpotter technology. The Gardner (2022) estimator is robust to the heterogeneous treatment effects in staggered two-way-fixed-effects designs.