The Effect of ShotSpotter Technology on Police Response Times

Michael Topper and Toshio Ferrazares*

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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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*Department of Economics, University of California Santa Barbara, 2017 North Hall, Santa Barbara, CA (michaeltopper@ucsb.edu).

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 7.1, 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 7.1, we find little suggestive evidence that ShotSpotter increases the probability of arrest for 911 reports of gun violence. Furthermore, a tertiary analysis in Section 7.4 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 2 provides background information on dispatching procedures and implementation of ShotSpotter in Chicago, Section 3 discusses the data, Section 4 describes the empirical strategy, Section 5 presents the main results, Section 6 contains heterogeneity analysis, Section 7 discusses the implications, and Section 8 concludes.

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

2 Background

2.1 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

[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

ShotSpotter. Nevertheless, it is important to note that the police districts chosen have historically high rates of gun violence.[4]Appendix Figure D1 shows the locations of the 12 police 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.

2.2 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

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

assigned to emergencies outside their district.[7]

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.

3 Data

3.1 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

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 field supervisor, and closest available unit.

is made.

Based on this information, we construct the two main outcome variables: the time from 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 D2 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.

3.2 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

[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.

gunfire and fireworks that may generate many false-positive ShotSpotter alerts.

3.3 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 5.1 where we find longer response times when there are fewer officers.

4 Empirical Strategy

4.1 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):

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

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 3.1, 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.

4.2 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 5.2 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.

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

5 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 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.

5.1 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 2.2—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 6.1 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 D2 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 D3 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.

5.2 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 C1 and C2 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.

5.3 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 **??** 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.

6 Heterogeneity

6.1 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 8 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.

6.2 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 3 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 6 and 7 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.

7 Discussion

7.1 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 **??** 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% 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%.

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

7.2 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.

7.3 Does ShotSpotter reduce Gun Crime?

7.4 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.

7.5 Cost-Benefit Analysis

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

8 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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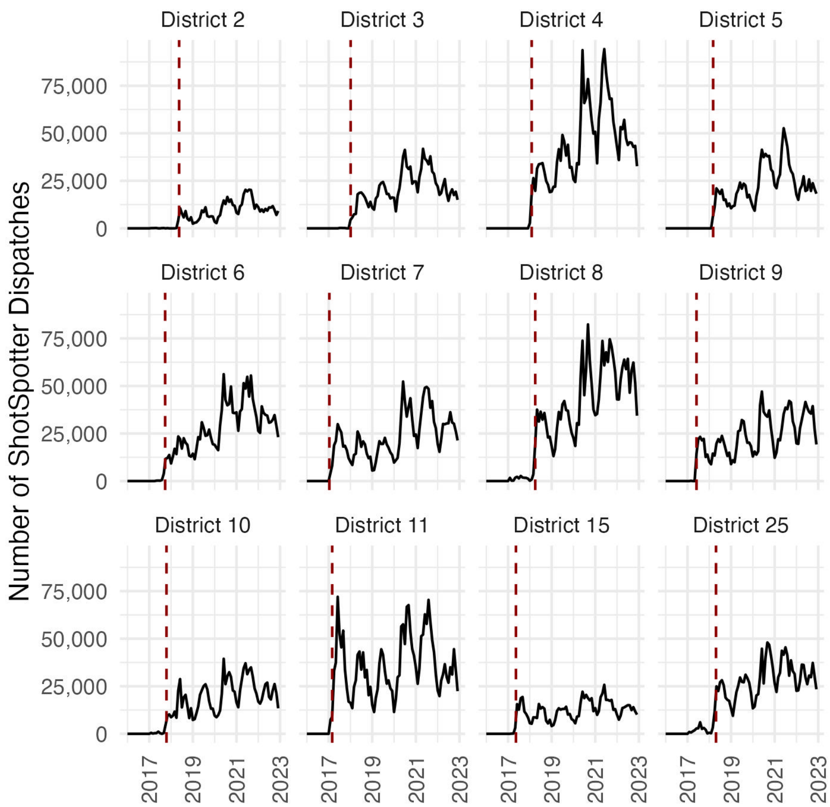
9 Figures



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 implemented 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.

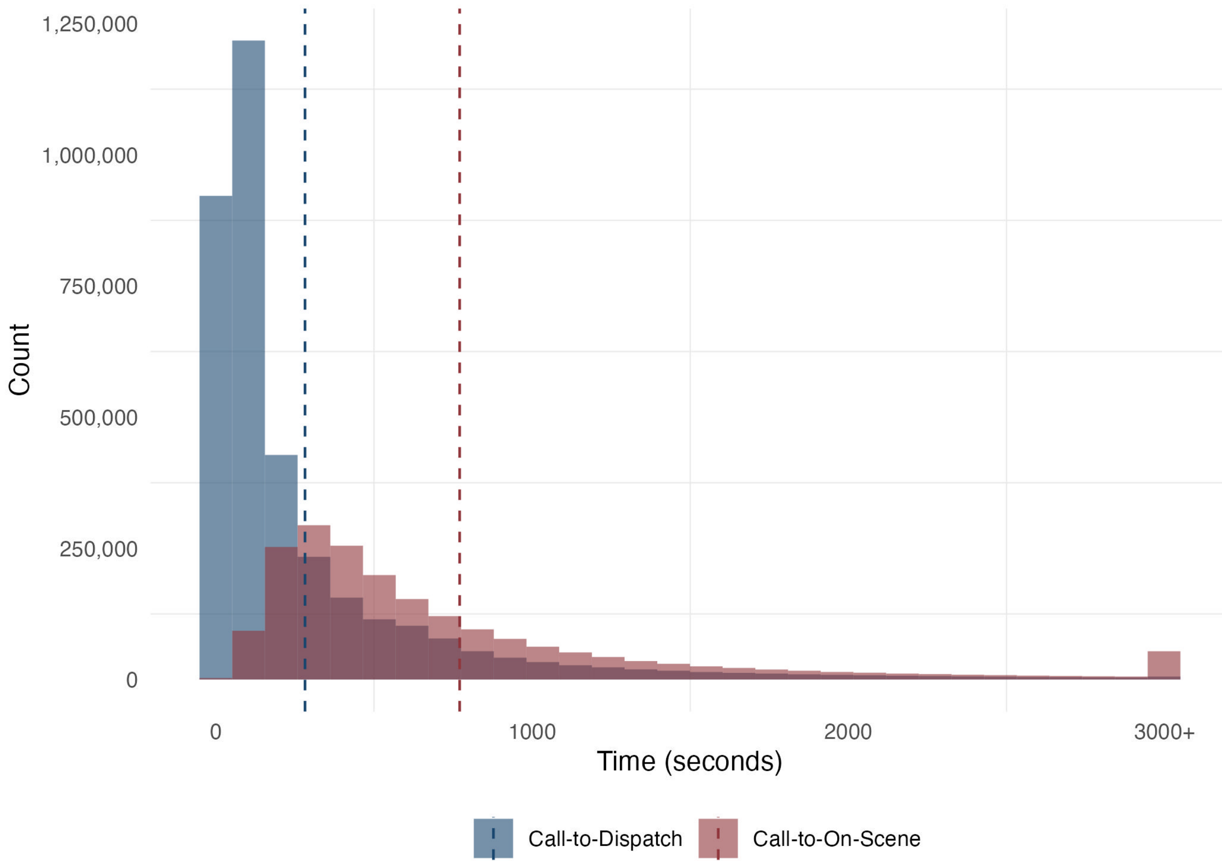


Figure 2: Distribution of Outcome Variables
Note: The two plotted variables are Call-to-Dispatch and Call-to-On-Scene. Call-to-Dispatch is time it takes for a police officer to be dispatched to the scene of the reported crime from the time of the 911 call. Call-to-On-Scene is the time from a 911 call 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.

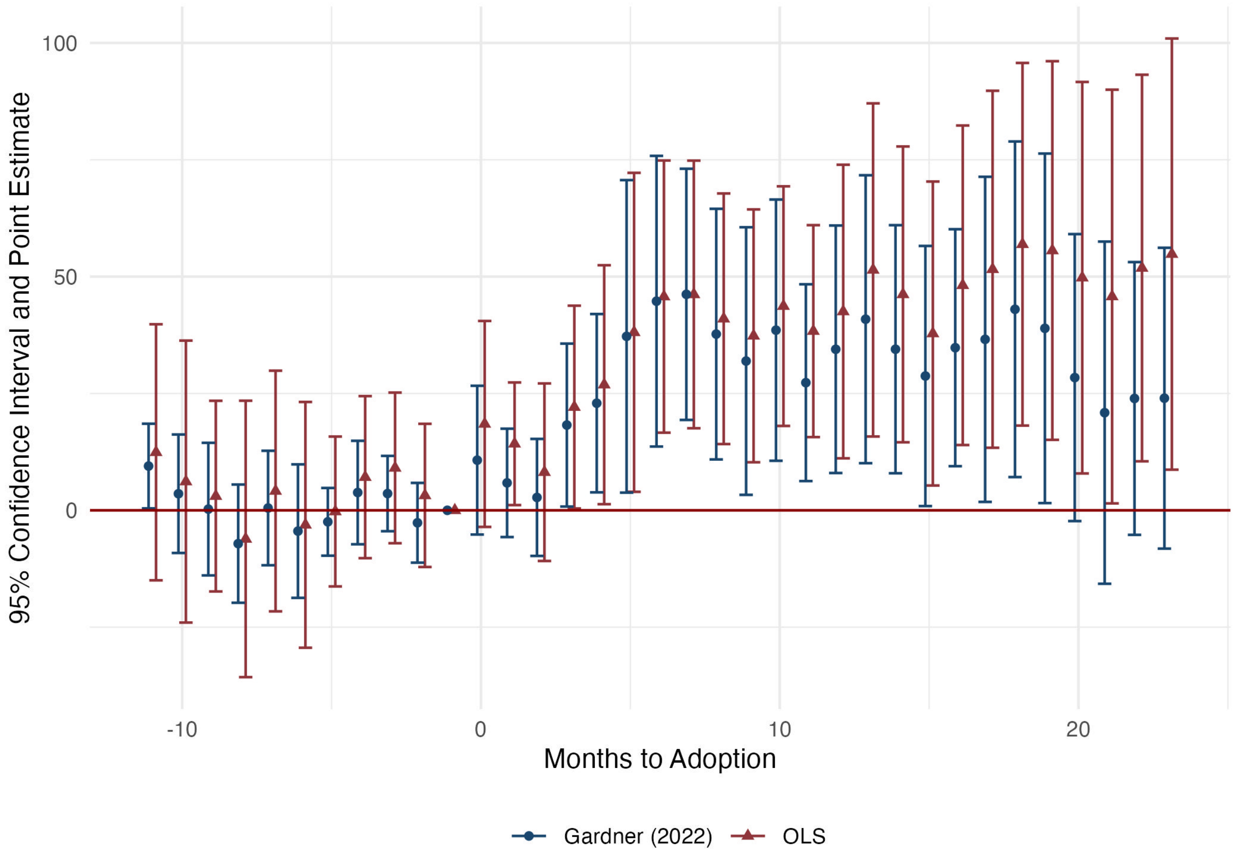


Figure 3: Event Study (Call-to-Dispatch)

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 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.

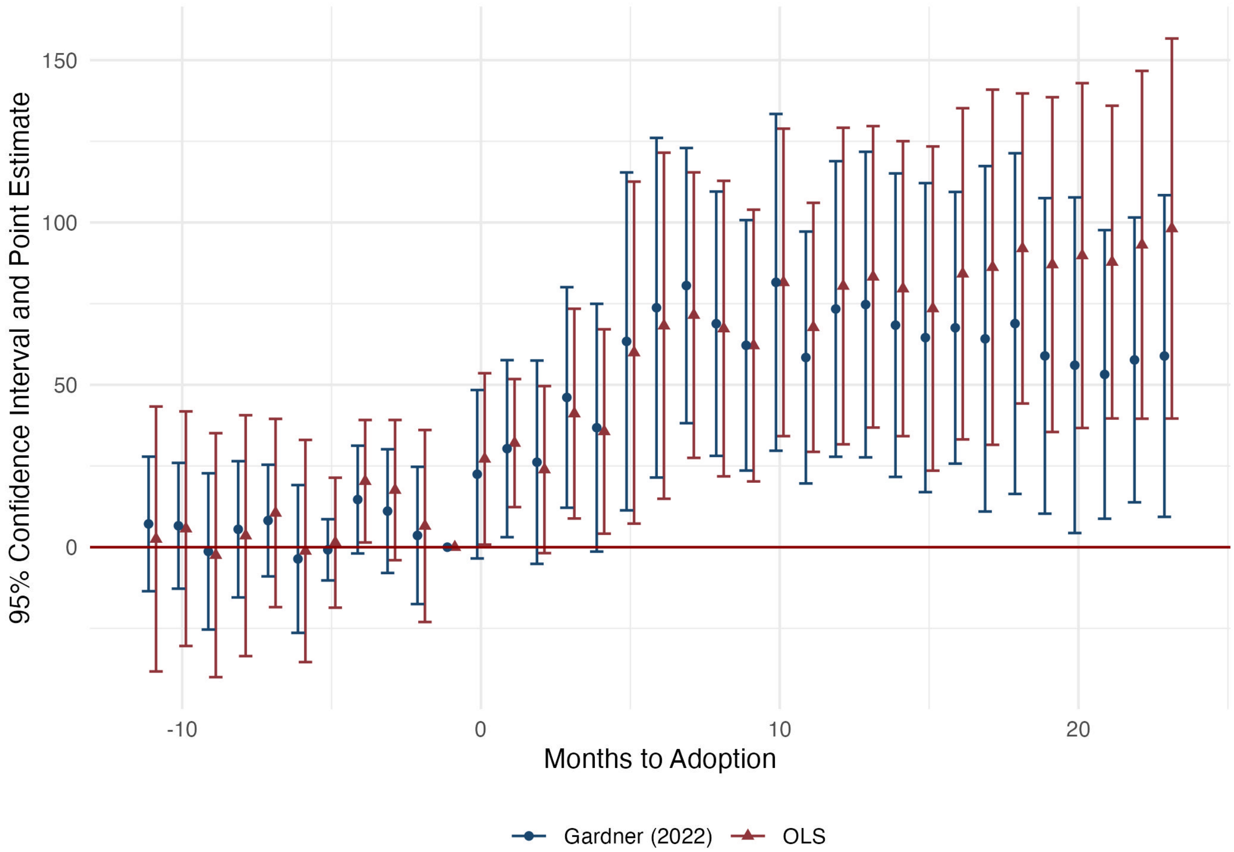


Figure 4: Event Study (Call-to-On-Scene)

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 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.

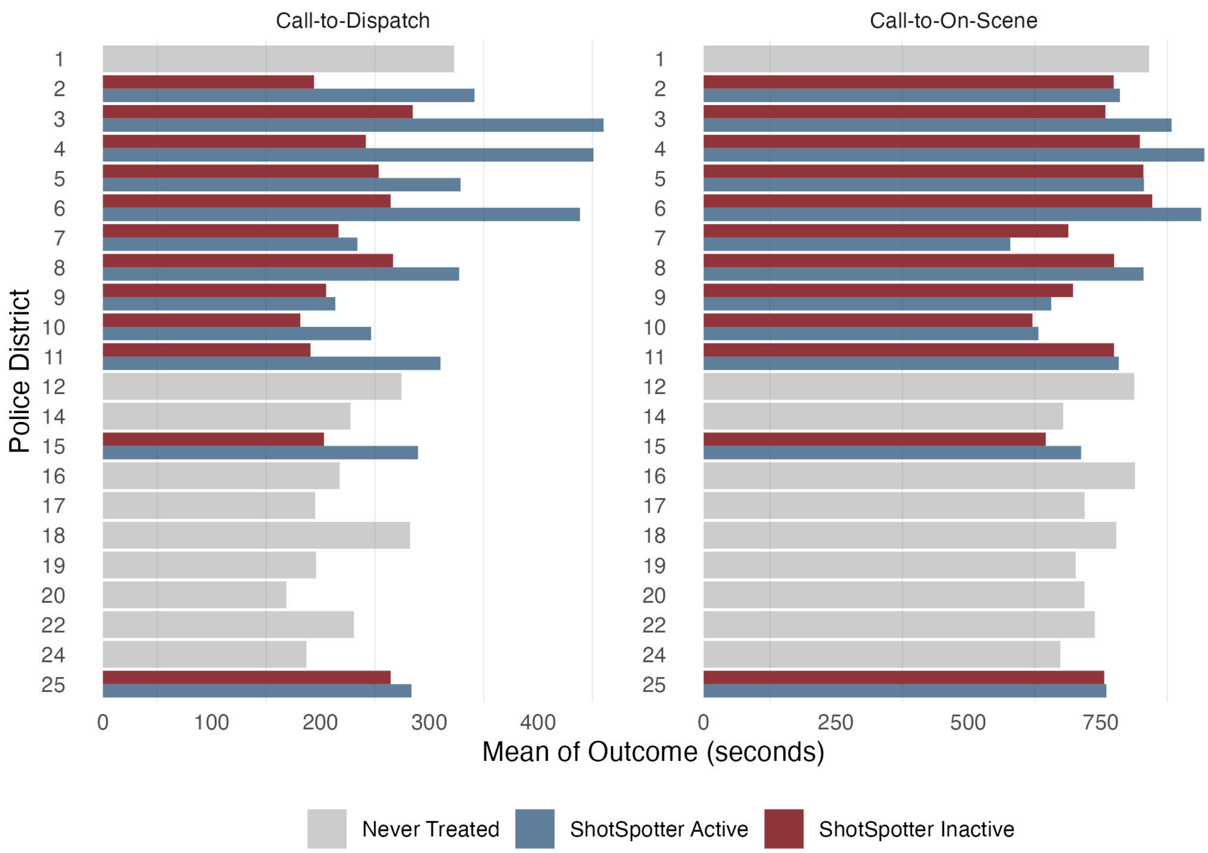


Figure 5: Average Outcomes in Police Districts

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.

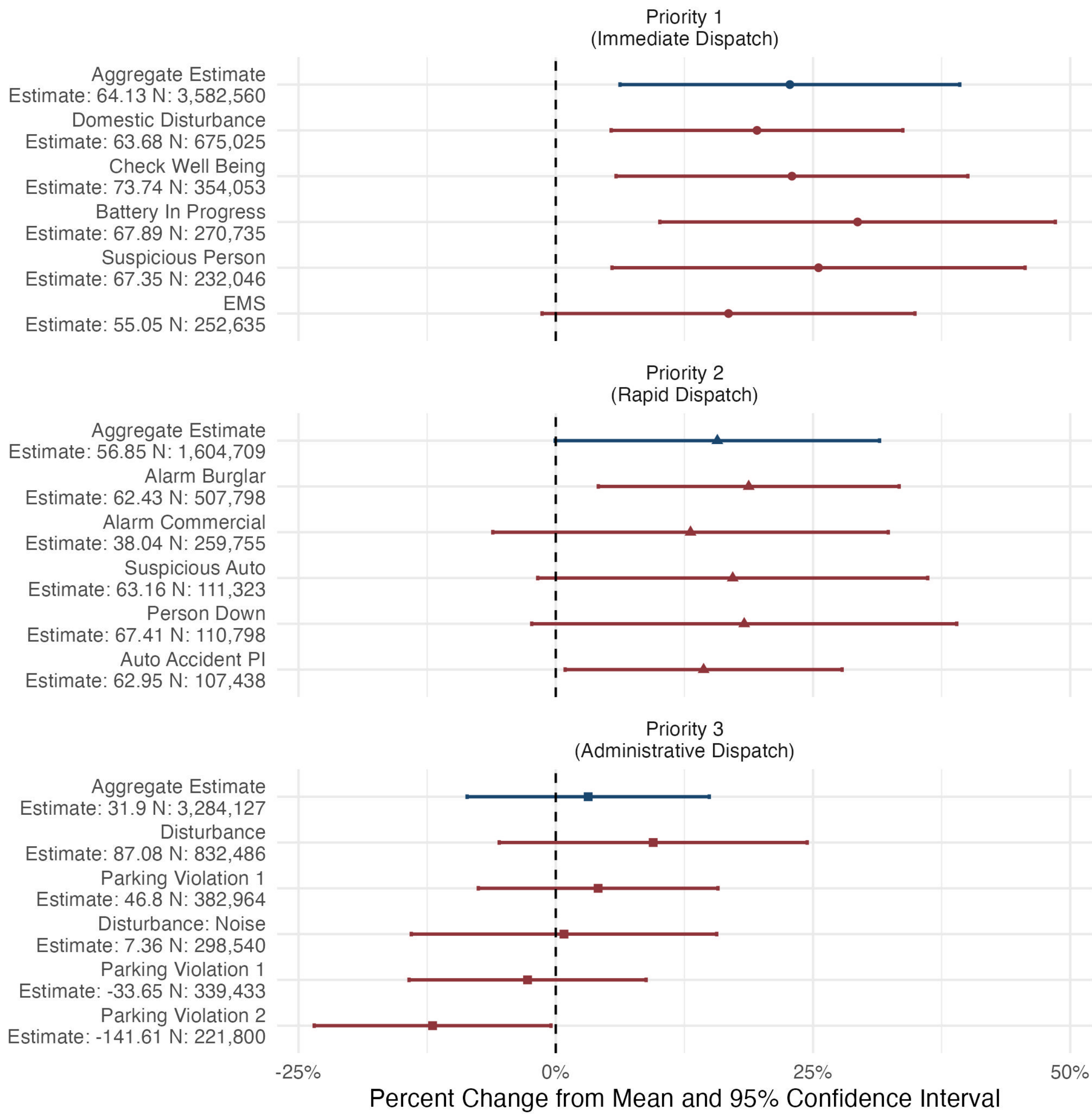


Figure 6: Effect of ShotSpotter by Priority (Call-to-Dispatch)

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).

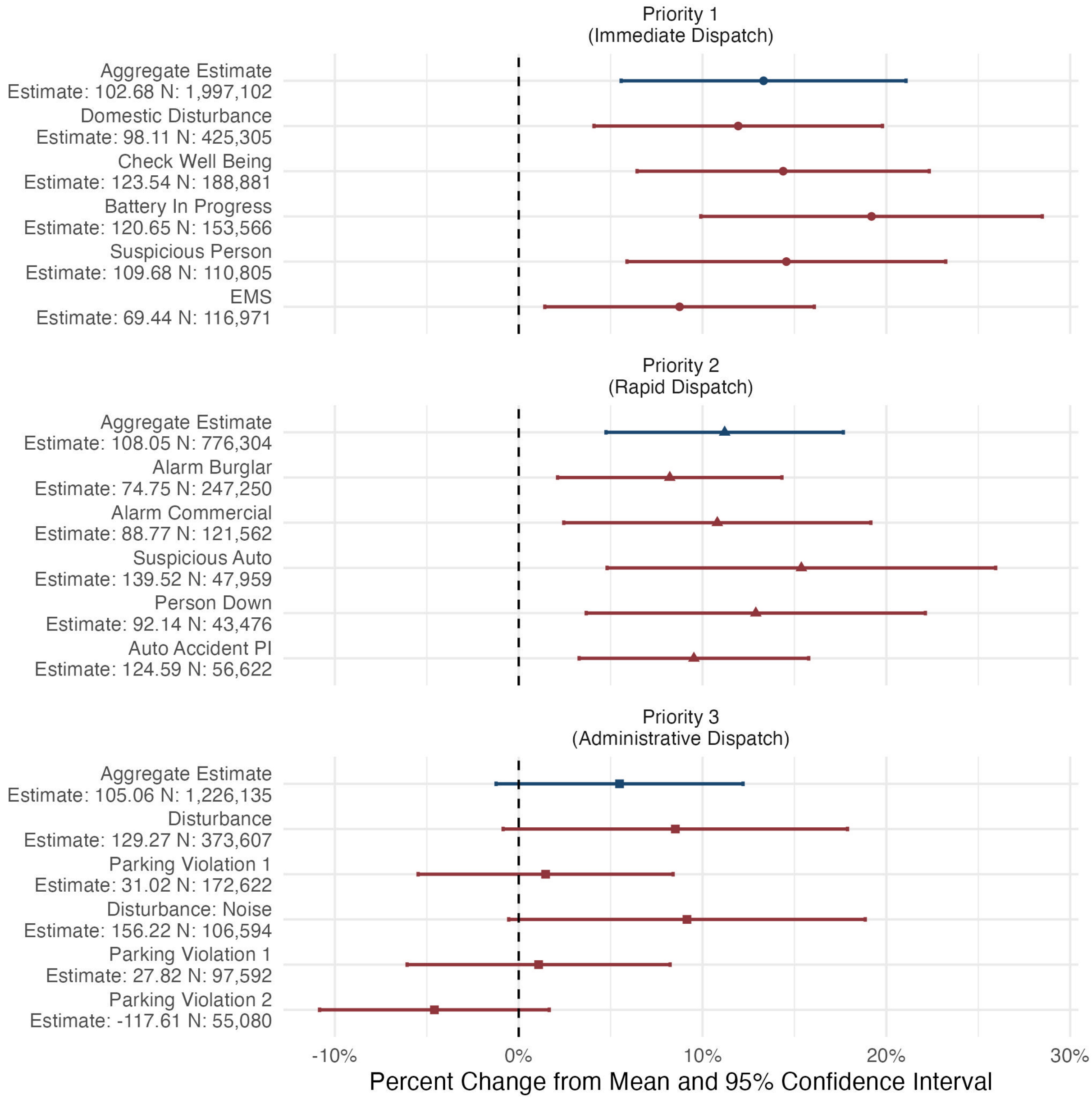


Figure 7: Effect of ShotSpotter by Priority (Call-to-On-Scene)

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).

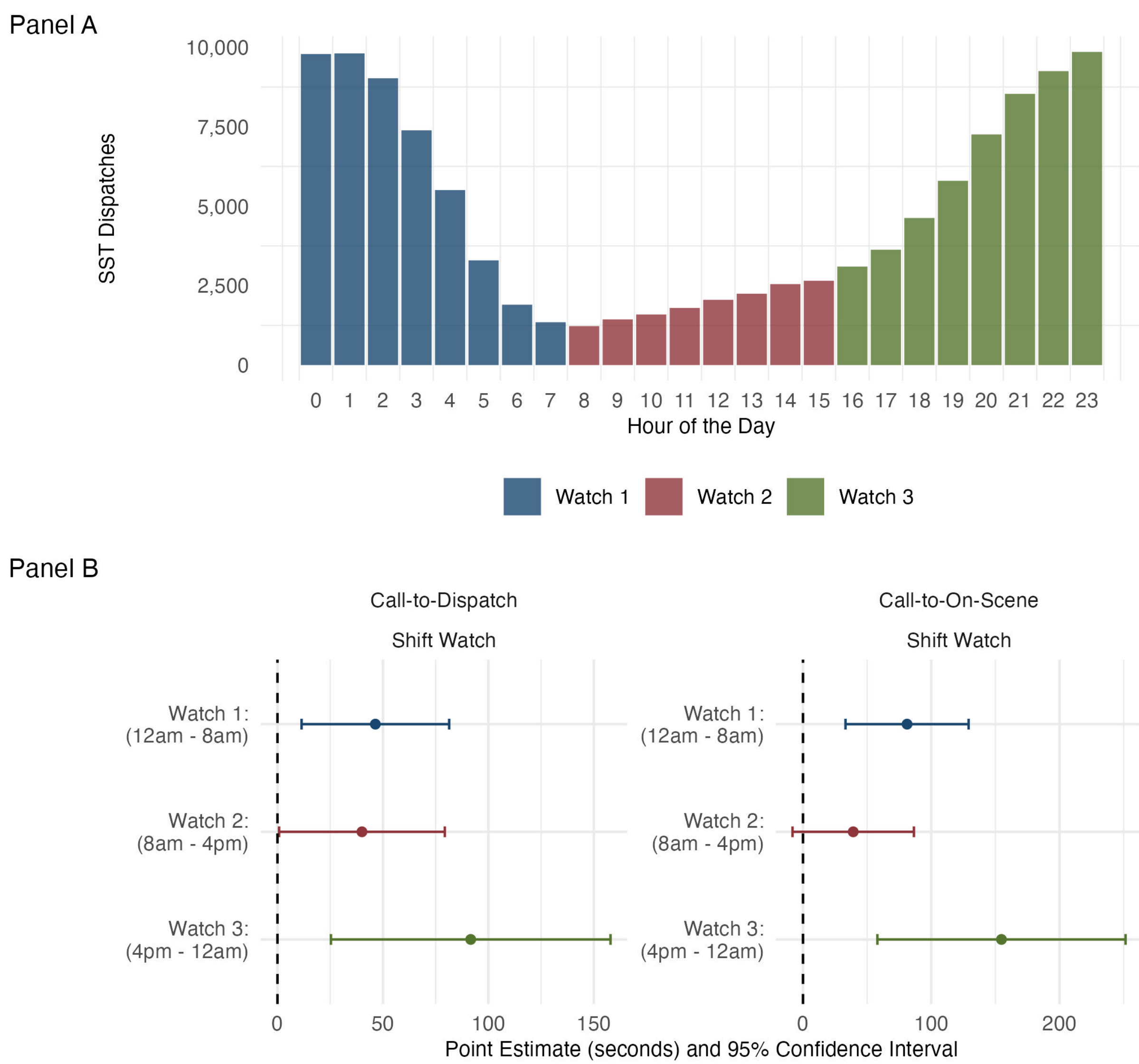


Figure 8: Effect of ShotSpotter by Watch

Note: This figure shows that in times when officers are responding to more ShotSpotter (SST) alerts, their response times are slower. In Panel A, the number of ShotSpotter alerts are plotted by the hour of occurrence. The y-axis is the number of ShotSpotter alerts while the x-axis the hour of the day. In Panel B, Call-to-Dispatch and Call-to-On-Scene estimates using the specification in Equation 1 are shown along with the 95% confidence intervals, split by officer watch. There are three main watches in Chicago: Watch 1 (12:00am - 8:00-am), Watch 2 (8:00am - 4:00pm), and Watch 3 (4:00pm - 12:00am).

10 Tables

Table 1: Summary Statistics

	Mean	Std. Dev.	Min	Max	N
Panel A: Priority 1 Outcomes:					
Call-to-Dispatch	281.89	436.53	2.00	3,111.00	3,582,560
	(4.70 mins)	(7.28 mins)	(0.03 mins)	(51.85 mins)	
Call-to-On-Scene	770.86	784.69	11.00	7,671.00	1,997,102
	(12.85 mins)	(13.08 mins)	(0.18 mins)	(127.85 mins)	
Arrest Made	0.02	0.15	0.00	1.00	3,582,560
Victim Injury (Time-Sensitive)	0.01	0.12	0.00	1.00	2,434,526
Panel B: Secondary Outcomes:					
Call-to-Dispatch (Priority 2)	362.04	524.78	2.00	3,577.00	1,604,709
	(6.03 mins)	(8.75 mins)	(0.03 mins)	(59.62 mins)	
Call-to-On-Scene (Priority 2)	964.45	901.10	14.00	6,615.00	776,304
	(16.07 mins)	(15.02 mins)	(0.23 mins)	(110.25 mins)	
Call-to-Dispatch (Priority 3)	1,012.99	1,258.17	2.00	6,550.00	3,284,127
	(16.88 mins)	(20.97 mins)	(0.03 mins)	(109.17 mins)	
Call-to-On-Scene (Priority 3)	1,915.35	1,820.17	10.00	11,702.00	1,226,135
	(31.92 mins)	(30.34 mins)	(0.17 mins)	(195.03 mins)	
Panel C: Other Variables:					
Number Dispatches	73.01	24.63	8.00	223.00	3,582,560
Number SST Dispatches	2.56	3.72	0.00	55.00	3,582,560
Officer Hours	1,259.50	316.36	200.50	3,431.50	3,582,560

Note:
Units are in seconds unless otherwise noted. Data is at the call-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. Call-to-On-Scene is missing approximately 45 percent of on-scene times. This is discussed further in Appendix A. Arrest Probability is the probability of an arrest occuring during a dispatch. Victim Injury Probability is the probability of a victim being injured during a time-sensitive dispatch call. A time-sensitive dispatch call is one in which the injury outcome has not yet been realized. 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 at the district-day level. Number of Dispatches is the number of Priority 1 dispatches at the district-day level. 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 daily 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)

	(1)	(2)	(3)	(4)	(5)
Panel A: Call-to-Dispatch					
ShotSpotter Activated	64.142***	64.058***	63.954***	71.929***	61.373***
	(21.541)	(22.394)	(22.235)	(22.405)	(21.641)
Border District Activated					21.406
					(16.503)
Mean of Dependent Variable	281.890	281.890	281.890	281.890	281.890
Observations	3,582,560	3,582,560	3,582,560	3,582,528	3,582,560
Wild Bootstrap P-Value	0.015	0.012	0.015		0.017
Panel B: Call-to-On-Scene					
ShotSpotter Activated	101.813***	103.107***	103.566***	120.721***	101.392***
	(26.205)	(28.801)	(28.182)	(27.992)	(28.167)
Border District Activated					24.407
					(17.882)
Mean of Dependent Variable	770.863	770.863	770.863	770.863	770.863
Observations	1,997,102	1,997,102	1,997,102	1,997,075	1,997,102
Wild Bootstrap P-Value	0.005	0.001	0.002		0.001
FE: Day-by-Month-by-Year	X	X	X	X	X
FE: District	X	X	X	X	X
FE: Call-Type		X	X	X	X
FE: Hour-of-Day		X	X	X	X
Officer Hours			X		
Number 911 Dispatches			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 ShotSpotter on Response Times Mechanisms (OLS)

	ShotSpotter Rollout			ShotSpotter Dispatches		
		Officer Hours			Officer Hours	
	Pooled	> Median	<= Median	Pooled	> Median	<= Median
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Call-to-Dispatch						
ShotSpotter Activated	64.131***	27.222**	93.794***			
	(22.379)	(12.382)	(31.497)			
Number SST Dispatches				5.272***	3.344***	4.237***
				(1.490)	(0.945)	(0.879)
Mean of Dependent Variable	281.890	229.785	333.871	291.300	232.886	349.536
Observations	3,582,560	1,789,157	1,793,403	2,958,754	1,477,121	1,481,633
Panel B: Call-to-On-Scene						
ShotSpotter Activated	102.682***	55.508**	141.492***			
	(28.724)	(21.030)	(38.611)			
Number SST Dispatches				7.053***	4.857***	5.152***
				(1.885)	(1.158)	(1.133)
Mean of Dependent Variable	770.863	700.283	837.941	771.964	690.147	853.515
Observations	1,997,102	973,138	1,023,964	1,732,479	864,836	867,643
FE: Day-by-Month-by-Year	X	X	X	X	X	X
FE: District	X	X	X	X	X	X
FE: Call-Type	X	X	X	X	X	X
FE: Hour-of-Day	X	X	X	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 Probability (OLS)

		Gun-Relation		Most Frequent Arrest Types		
	All	Gun	Non-Gun	Domestic Disturbance	Domestic Battery	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
ShotSpotter Activated	-0.002***	-0.002	-0.002***	-0.008***	-0.003**	-0.003
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Mean of Dependent Variable	0.024	0.034	0.024	0.061	0.020	0.042
Observations	3,582,560	317,937	3,264,623	224,022	675,025	270,735
FE: Day-by-Month-by-Year	X	X	X	X	X	X
FE: District	X	X	X	X	X	X
FE: Call-Type	X	X	X	X	X	X
FE: Hour-of-Day	X	X	X	X	X	X

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. Panel A shows Arrest Rate defined as the number of arrests made divided by the number of dispatches, while Panel B shows Injury defined as the number of injury-related dispatches divided by the number of dispatches that are time-sensitive (see Appendix Figure BLANK). Columns 2 and 3 subset Column 1 by gun-related and non-gun-related arrest rates and injury rates. Gun-related crimes for Arrest Rate are those corresponding to a person with a gun, shots fired, or a person shot. Gun-related crimes to Injury Rate corresponds to person with gun or shots fired. Columns 3-5 report the top 3 most frequent calls that end in arrests: Domestic Battery, Domestic Disturbance, and Battery. 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.

Table 5: Effect of ShotSpotter Implementation on Victim Injury (OLS)

	Probability of Victim Injury		
	Pooled	Gun Dispatch	Non-Gun Dispatch
	(1)	(2)	(3)
ShotSpotter Activated	-0.001*	-0.003	0.000
	(0.000)	(0.002)	(0.000)
Mean of Dependent Variable	0.014	0.024	0.012
Observations	2,434,526	304,544	2,129,982
FE: Day-by-Month-by-Year	X	X	X
FE: District	X	X	X
FE: Call-Type	X	X	X
FE: Hour-of-Day	X	X	X

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. The main outcome variable is the probability of a victim being injured. The sample here is restricted to only Priority 1 dispatches that are time-sensitive and have the possibility of an injury. For instance, a dispatch for a person shot is not time sensitive since the injury has already been realized. On the other hand, a dispatch for a person with a knife is considered time-sensitive as an injury has not yet occurred, but may occur if an officer arrives slower. The Pooled column refers to using the entire sample of time-sensitive Priority 1 dispatches. Gun Dispatch is restricted to only time-sensitive gun dispatches including 'Person with a Gun' and 'Shots Fired'. Non-Gun Dispatch are all other time-sensitive dispatches. In all columns the preferred specification is estimated using OLS. .

A Appendix A: Missing Call-to-On-Scene Data

In this appendix, we conduct analyses regarding the notable amount of data missing for one of the key outcome variables, Call-to-On-Scene (~45%). Recall that Call-to-On-Scene denotes the time interval between a 911 call and an officer's arrival at the scene of the incident. While we find suggestive evidence that missing Call-to-On-Scene times are correlated with ShotSpotter implementation, this section outlines several reasons to maintain confidence in the main results despite this limitation.

A.1 Reasons for Missing Data

First, we note that the underlying reason behind a missing Call-to-On-Scene entry is an officer's failure to report to the dispatcher that they have arrived on-scene. This could be due to an officer forgetting to report, or more likely, an officer being immediately engaged on-scene. Importantly, we provide suggestive evidence that the latter is happening more frequently post-implementation of ShotSpotter due to officers being more time-constrained.

In Panel A of Appendix Table A1, we estimate the preferred specification from Equation 1 on an indicator for a missing Call-to-On-Scene time and find suggestive evidence of a correlation. Column 1 of Panel A reports a 3.8% increase in the likelihood of missing Call-to-On-Scene when ShotSpotter is implemented, which is statistically significant at the 10% level. However, Columns 2 and 3 show that this effect is driven by times in which there are fewer officers on duty, implying that ShotSpotter may be straining officers time allotment. For instance, if an officer feels they have fallen behind, they may disregard explaining to the dispatcher that they have arrived to the scene. If this is the case, then the missing on-scene times may be larger than the non-missing times, thereby suggesting that the main results are a lower bound.

A.2 Impact on Call-to-Dispatch Times

Second, we examine the impact of missing data on Call-to-Dispatch times—the time from a 911 call to when an officer is dispatched to the crime scene. Notably, Call-to-Dispatch times, a mechanism underlying Call-to-On-Scene times as discussed in Section 5, are 100% reported.

To begin, we supplement Equation 1 with an interaction between ShotSpotter implementation (ShotSpotter Activate) and an indicator for missing Call-to-On-Scene times (Missing On-Scene).[11] In doing so, we test whether there are differences in the effect of ShotSpotter on Call-to-Dispatch times between cases with missing and no missing data. Panel B of Appendix Table A1 reports no significant change in Call-to-Dispatch times when there is missing Call-to-On-Scene data. As shown across Columns 1-3, there is little evidence that Call-to-Dispatch times differ in a missing data case. Specifically, the coefficient on the interaction term is small and statistically insignificant.

A.3 Consistent Trends

Last, given that Call-to-Dispatch times are fully reported and there is no change when Call-to-On-Scene times are missing, we plot the event study coefficients from Figures 3 and 4 in Appendix Figure A1 which shows that there is a consistent time trend for each outcome variable. The convergence in trends reinforces the notion that even when Call-to-On-Scene data is absent, officers may still experience delays in reaching the scene due to slower dispatching procedures. This consistent pattern underscores the reliability of the Call-to-On-Scene findings.

[11] The fixed effects are also interacted with Missing On-Scene.

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

		Officer Hours	
	Pooled	> Median	<= Median
	(1)	(2)	(3)
Panel A: Missing Call-to-On-Scene			
ShotSpotter Activated	0.038*	0.032	0.042*
	(0.019)	(0.019)	(0.022)
Mean of Dependent Variable	0.443	0.456	0.429
Observations	3,582,560	1,789,157	1,793,403
Panel B: Call-to-Dispatch			
ShotSpotter Activated	66.408***	29.280**	97.359***
	(23.059)	(12.846)	(32.122)
ShotSpotter Activated x Missing	-0.249	-1.435	-2.469
	(32.877)	(18.407)	(44.942)
Mean of Dependent Variable	281.890	229.785	333.871
Observations	3,582,560	1,789,157	1,793,403

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. In Panel A, the table shows regressions on a binary variable equal to one if Call-to-On-Scene is missing. Columns 2 and 3 are split by district-day medians of officer hours. In Panel B, Call-to-Dispatch time is estimated with an additional interaction term in order to show that there is no difference in Call-to-Dispatch time when there is missing on-scene data. Note that in these specifications, the fixed effects are also interacted to get a similar interpretation as if there were two separate regressions estimated. All controls utilized in these regressions are consistent with the preferred specification and are estimated using OLS.

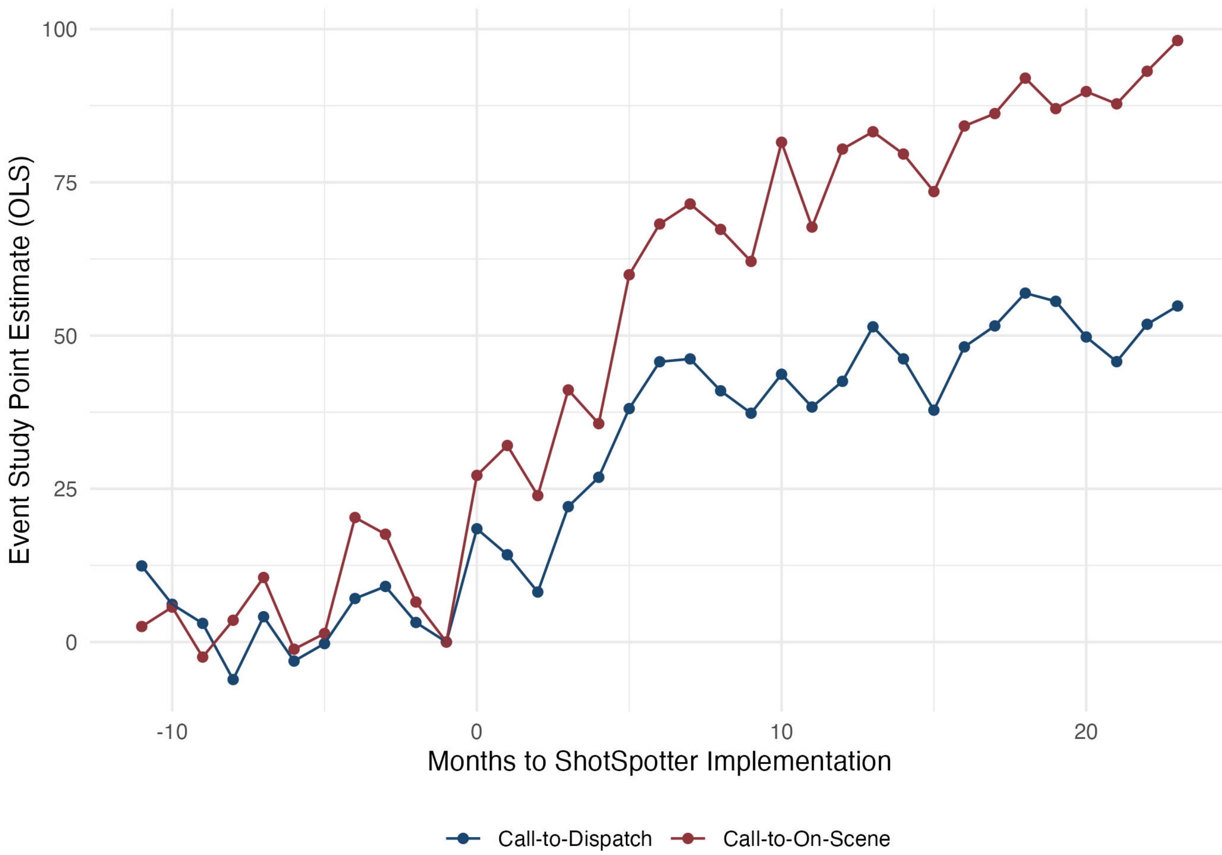


Figure A1: Corresponding Event Study Trends

Note: This figure plots the point estimates of the event study specifications in Equation 2 for both Call-to-Dispatch (blue) and Call-to-On-Scene (red). In effect, this figure shows that the trends for each of these outcomes are similar. The y-axis denotes the point estimate in seconds and the x-axis displays the number of months to ShotSpotter implementation. Recall that Call-to-Dispatch has no missing data, while Call-to-On-Scene is approximately 45 percent missing. This figure is intended to show that Call-to-Dispatch, a mechanism underlying slower on-scene times, has a similar trend to Call-to-On-Scene, suggesting that missing data may not be a substantial issue.

A Appendix B: Coinciding Initiatives

In this appendix, we discuss two initiatives that were implemented in the Chicago Police Department (CPD) near the timing of ShotSpotter: Strategic Decision Support Centers and Body-worn Cameras. While neither of these exactly coincide with ShotSpotter implementation, we perform several sets of analyses to mitigate concerns that these, rather than ShotSpotter, are causing increases in response times.

A.1 Strategic Decision Support Centers

Strategic Decision Support Centers (SDSC) are command and control centers created to give police officers more awareness of what is occurring in their districts, and decide on responses. The main objective of SDSCs is to reduce crime, improve officer safety, and reduce service times. Each SDSC has staff members which include a dedicated supervisor (usually a sworn officer who is a lieutenant or sergeant) and a data analyst.

These support centers act as a hub for all of Chicago's policing technologies whereby they can relay real-time information to police officers in the field. In particular, these centers are constantly analyzing data from automated license plate readers, social media monitoring, police observation cameras and devices, and geospatial predictive police software (Hunchlab).[12] While most of these technologies have already been in utilization by the CPD prior to SDSCs,[13] the Hunchlab software is implemented at the exact timing of an SDSC.

A.1.1 SDSC Technology Effect on Police Patrolling

There may be reason to suspect that Hunchlab, the geospatial predictive policing technology implemented with SDSCs, affects police response times. Hunchlab functions by creating location hot-spots in which police officers are supposed to visit more frequently in their

[12] Hunchlab was bought by ShotSpotter in fall of 2018 and is now known as ShotSpotter Missions. We refrain from using this terminology as it might be confusing to a reader.

[13] Automated license plate readers began as early as 2006, social media monitoring as early as 2014, and police observation cameras and devices as early as 2003.

patrols. These hot-spots are places where Hunchlab algorithms are predicting crime to occur. Hence, Hunchlab could affect response times by placing officers closer (or farther) to reported incidents of crime, or by placing them in areas where they are more likely to make arrests/stops and be unavailable for dispatch.

Despite this potential limitation, a thorough analysis of this exact technology is provided in YENS. Specifically, they find that Hunchlab causes significant changes in police patrolling behavior for only two police districts (Distrct 7 and District 9). The null results they report in the other police districts are attributed to commanders or officers disregarding the software's suggestions.

A.1.2 Main Results Controlling for SDSCs

In this subsection, we re-estimate the main specification and corresponding event studies on Call-to-Dispatch and Call-to-On-Scene times while controlling for the SDSC implementation. SDSCs are implemented in a district-by-district roll-out that is similar (although not exact) to ShotSpotters implementation. Appendix Table B1 reports the districts and corresponding dates of their implementation. On average, SDSCs are implemented 76 days prior to ShotSpotter.

Appendix Table B2, shows consistent findings of the effects of ShotSpotter on response times while controlling for the roll-out of SDSCs. In Columns 1, we use the OLS estimator while in Column 2, we use the Gardner (2022) estimator to account for possible treatment heterogeneity across groups and over time given the staggered design. In Panel A, Call-to-Dispatch times show increases of approximately one-minute, while in Panel B, Call-to-On-Scene times exhibit slightly smaller estimates than the main findings, but remain statistically significant at the 5% level. Reassuringly, there appears to be no effect of the SDSC roll-out on Call-to-Dispatch times, suggesting that the Hunchlab technology in the SDSCs is not incapacitating officers' availability. On the other hand, there is suggestive evidence that SDSCs may be increasing Call-to-On-Scene times. However, this increase is not statistically significant at the 5% level.

In Columns 3 and 4 of of Appendix Table B2, we re-estimate the specifications from Columns 1 and 2, but exclude police districts 7 and 9 which have been found to have changes in police patrolling behavior following the SDSC rollout YENS. In doing so, we focus the analysis on districts in which there are no patrolling changes whereby response times could be affected. The results for both Call-to-Dispatch and Call-to-On-Scene are consistent with the main findings, and in addition, show larger effect sizes than the entire pooled sample. This suggests that the Hunchlab technology utilized in the SDSCs, when properly utilized, may mitigate some of the response time lag attributed to ShotSpooter.

Next, we estimate the event study specifications in Equation 2 while controlling for SDSC implementation. Appendix Figures B1 and B2 plot the event studies for Call-to-Dispatch and Call-to-On-Scene times using both the OLS estimator (red) and the Gardner (2022) estimator (blue). In both plots, the standard errors get significantly larger relative to the models without SDSC controls. This is likely due to the proximity of both ShotSpotter implementation and SDSCs. However, despite these larger standard errors, the pre-period shows no visual evidence of a violation of the common trends assumptions, and the post period results appear similar to the main event studies in Figures 3 and 4.

A.2 Body-Worn Cameras

In this subsection, we show that controlling for the body-worn camera (BWC) implementation in Chicago has no effect on the response time results. As mentioned in the main text, the district implementation of BWCs differs by 283 days on average (see Appendix Table B1) from the ShotSpotter roll-out (see Appendix Table B1). Moreover, 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), there is little reason to suspect that they significantly affect an officers ability to rapidly respond.

Columns 5 and 6 of Appendix Table B2 report the results for both Call-to-Dispatch and Call-to-On-Scene times while controlling for BWC implementation. The results are consistent with the main findings, and the negative coefficient on BWC does not show any evidence of affecting response times.

Table B1: Implementation Dates of ShotSpotter/SDSC/BWC

District	ShotSpotter	SDSC	BWC	Difference SDSC	Difference BWC
2	2018-05-16	2018-03-01	2016-06-29	76 days	686 days
3	2018-01-04	2018-01-01	2017-11-06	3 days	59 days
4	2018-02-01	2018-01-01	2016-08-13	31 days	537 days
5	2018-03-07	2018-01-01	2017-11-20	65 days	107 days
6	2017-09-24	2017-03-15	2016-08-04	193 days	416 days
7	2017-01-13	2017-01-07	2017-05-01	6 days	108 days
8	2018-04-01	2018-03-01	2017-10-02	31 days	181 days
9	2017-06-01	2017-03-15	2016-08-18	78 days	287 days
10	2017-10-16	2017-03-15	2016-07-25	215 days	448 days
11	2017-03-01	2017-02-17	2017-06-05	12 days	96 days
15	2017-05-13	2017-03-15	2016-06-13	59 days	334 days
25	2018-04-24	2018-01-01	2017-12-04	113 days	141 days
14			2016-06-01		
1			2017-03-10		
18			2017-03-31		
24			2017-10-16		
20			2017-10-23		
19			2017-10-30		
22			2017-10-30		
16			2017-11-20		
17			2017-11-27		
12			2017-12-04		

Note:
This table shows the implementation dates of ShotSpotter technology and Strategic Decision Support Centers (SDSC). SDSCs are implemented in similar, although not the same time period. The Difference column shows the number of days between the SDSC implementation and ShotSpotter activation. On average, this is approximately 73 days. SDSCs contain many police prediction softwares, however, only Hunchlab, a location prediction software, is implemented in conjuction with these. This software has been found to only change patroling behaviors in districts 7 and 9 as discussed in Kapustin et al. (2022). Further robustness of the results including SDSC implementation dates as controls are shown in Appendix Table B2.

Table B2: Robustness of Estimates Controlling for Other Technologies

	SDSC Controls				BWC Controls	
			Omitting Districts 7 and 9			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Call-to-Dispatch						
ShotSpotter Activated	50.097**	69.056***	57.445**	86.995***	61.256***	71.856***
	(22.185)	(20.481)	(23.098)	(19.580)	(20.988)	(22.523)
SDSC Activated	16.921		17.795			
	(22.102)		(22.342)			
BWC Activated					-30.735	
					(20.755)	
Mean of Dependent Variable	281.890	281.890	289.018	289.018	281.890	281.890
Observations	3,582,560	3,582,528	3,198,525	3,198,500	3,582,560	3,582,528
Wild Bootstrap P-Value	0.008	0.003			0.062	
Panel B: Call-to-On-Scene						
ShotSpotter Activated	68.486**	100.562***	72.692**	123.226***	98.403***	120.214***
	(27.013)	(28.118)	(29.436)	(24.756)	(27.843)	(28.246)
SDSC Activated	43.771*		48.562*			
	(24.711)		(25.830)			
BWC Activated					-40.821	
					(26.223)	
Mean of Dependent Variable	770.863	770.863	790.897	790.897	770.863	770.863
Observations	1,997,102	1,997,076	1,762,676	1,762,656	1,997,102	1,997,076
Wild Bootstrap P-Value	0.008	0.003			0.062	
Gardner (2022) Robust		X		X		X

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. Columns 1-2 of Panel A show Call-to-Dispatch estimates when controlling for Strategic Decision Support Center (SDSC) rollout. In Columns 3 and 4, police districts 7 and 9 are omitted as Kapustin et al. (2022) shows that SDSCs affect police patrolling in these districts. Panel B is similar to Panel A, with the outcome of interest being Call-to-On-Scene times. In Columns 5 and 6, we control for Body-Worn Camera (BWC) adoption. Note that in each specification, controls are consistent with the preferred specification. OLS estimates are reported in odd-numbered columns, while Gardner (2022) robust estimates are reported in even columns.

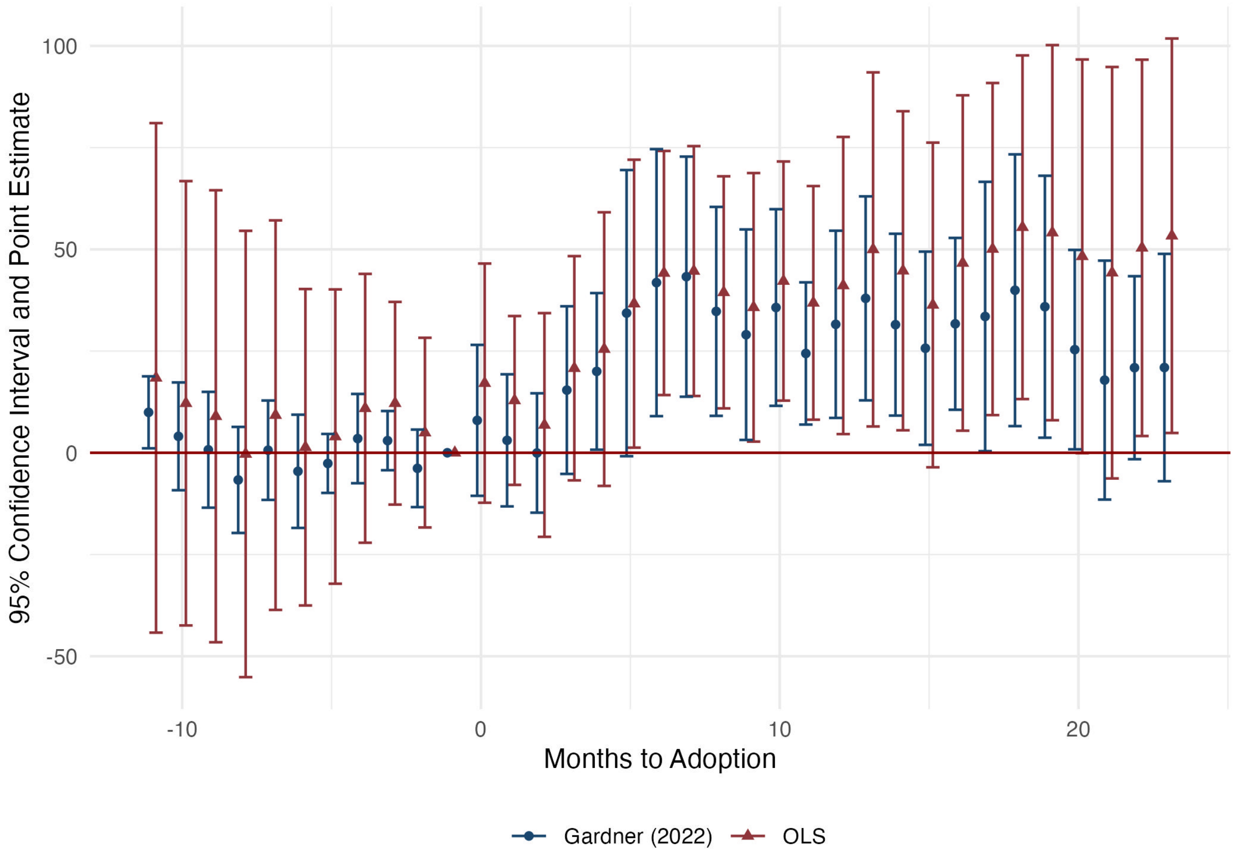


Figure B1: Event Study w/ SDSC Controls (Call-to-Dispatch)

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 23 post-periods are reported as the -12 and +24 are binned endpoints. Controls are synonymous with the preferred specification in addition to SDSC rollout. Standard errors are clustered at the district level.

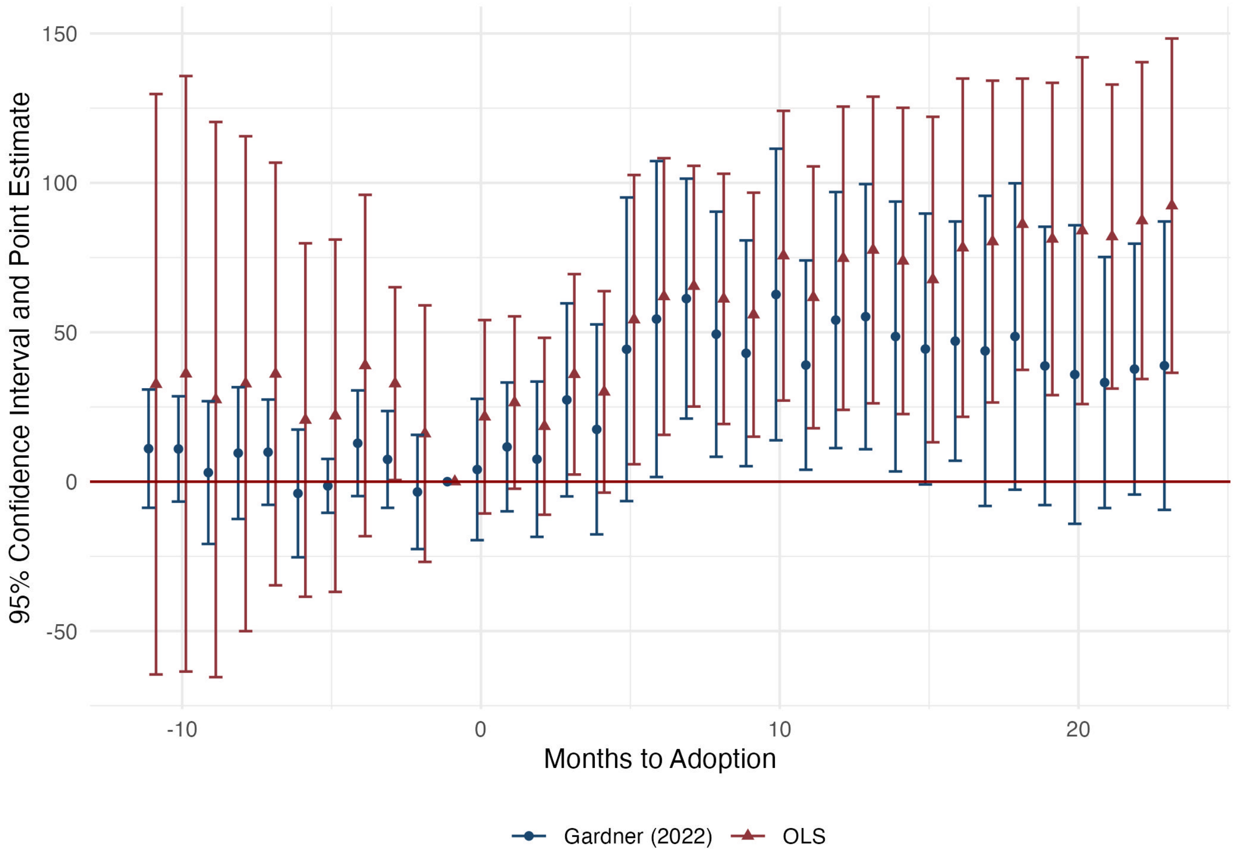


Figure B2: Event Study w/ SDSC Controls (Call-to-On-Scene)

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 23 post-periods are reported as the -12 and +24 are binned endpoints. Controls are synonymous with the preferred specification in addition to SDSC rollout. Standard errors are clustered at the district level.

A Appendix C: Sensitivity Analysis of Event Studies

In this appendix, we conduct analysis following Rambachan and Roth (2023) on the OLS event study specifications in Figures 3 and 4 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 C1 and C2 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.

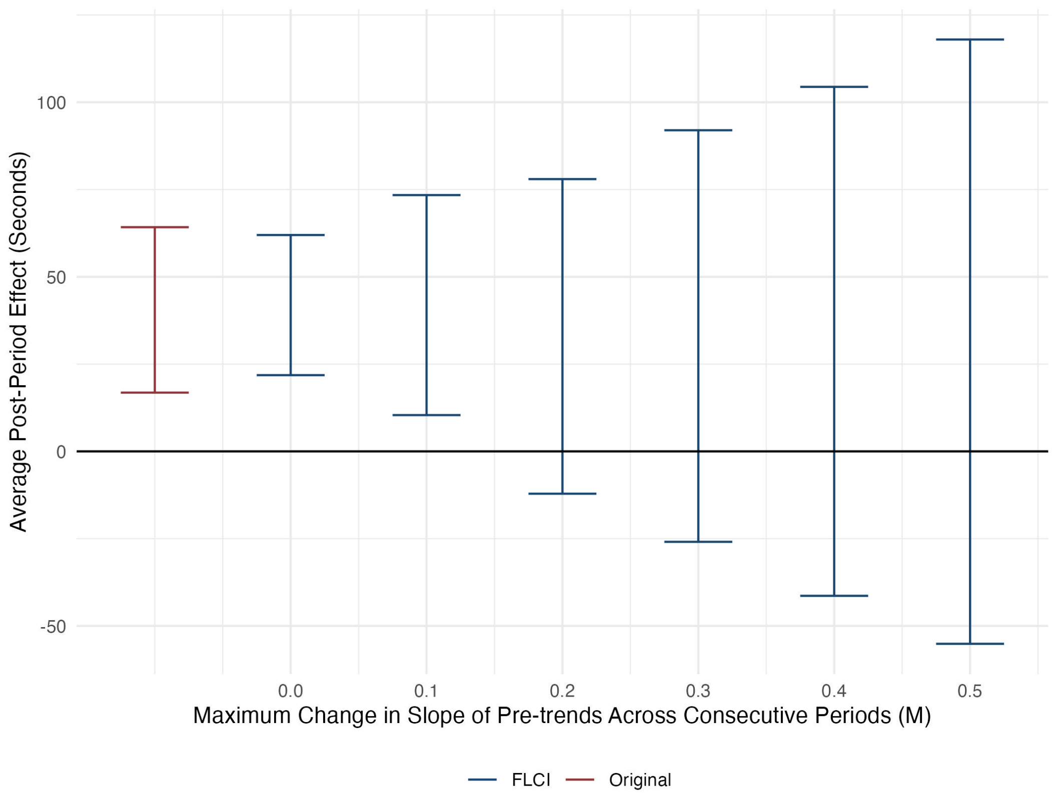


Figure C1: Sensitivity Analysis of Pre-Trends

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 0.2 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 0.2.

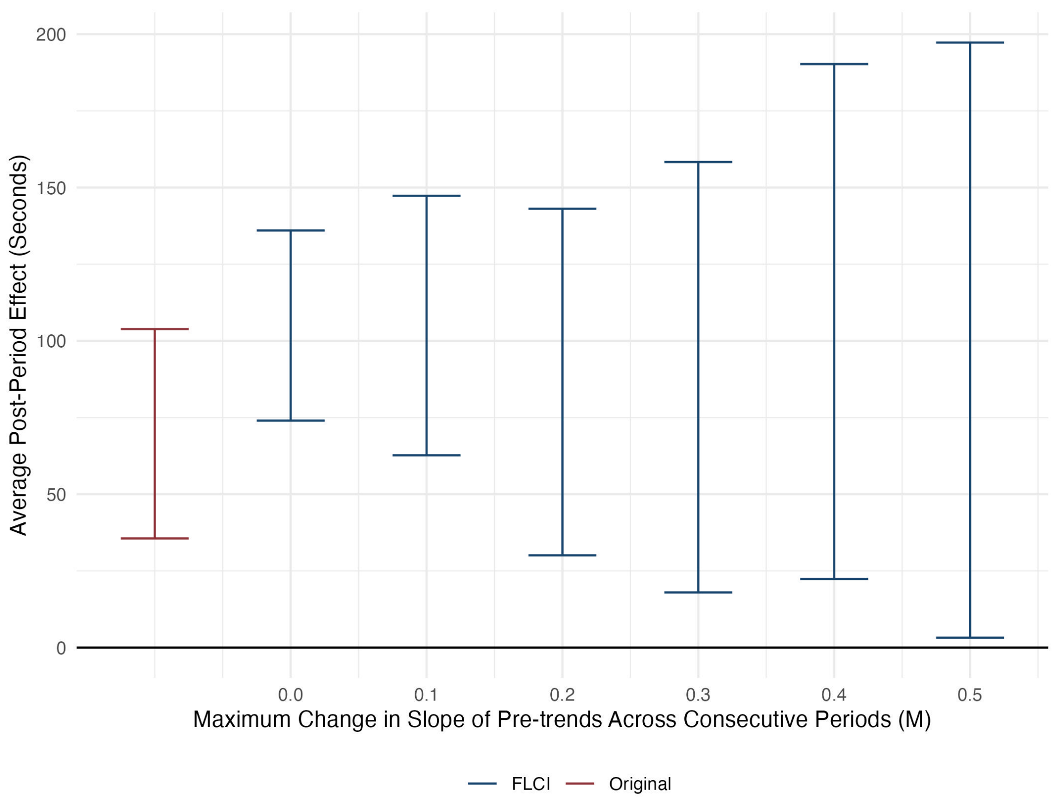


Figure C2: Sensitivity Analysis of Pre-Trends (Call-to-On-Scene)

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 larger than 0.5 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 a number larger than 0.5.

A Appendix Figures

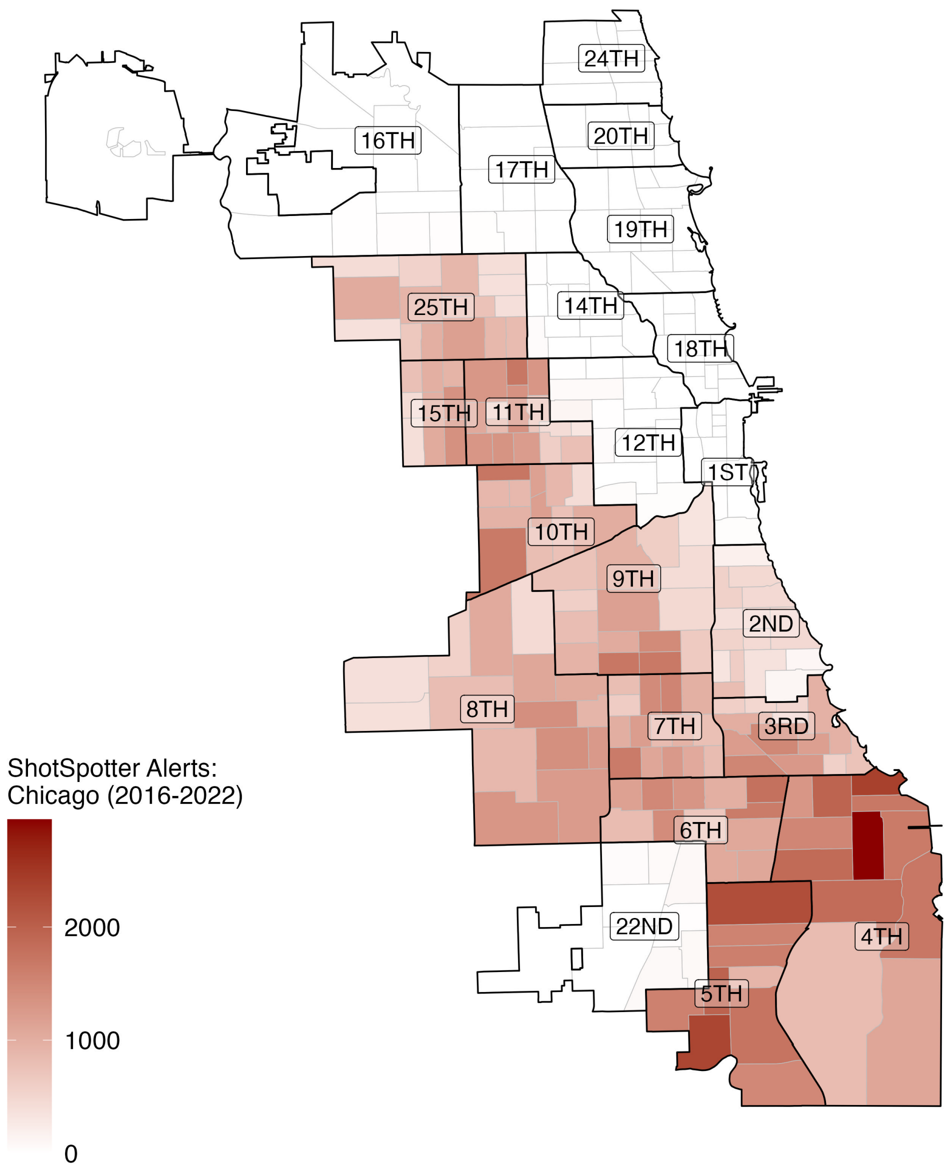


Figure D1: Map of ShotSpotter Districts in Chicago

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.

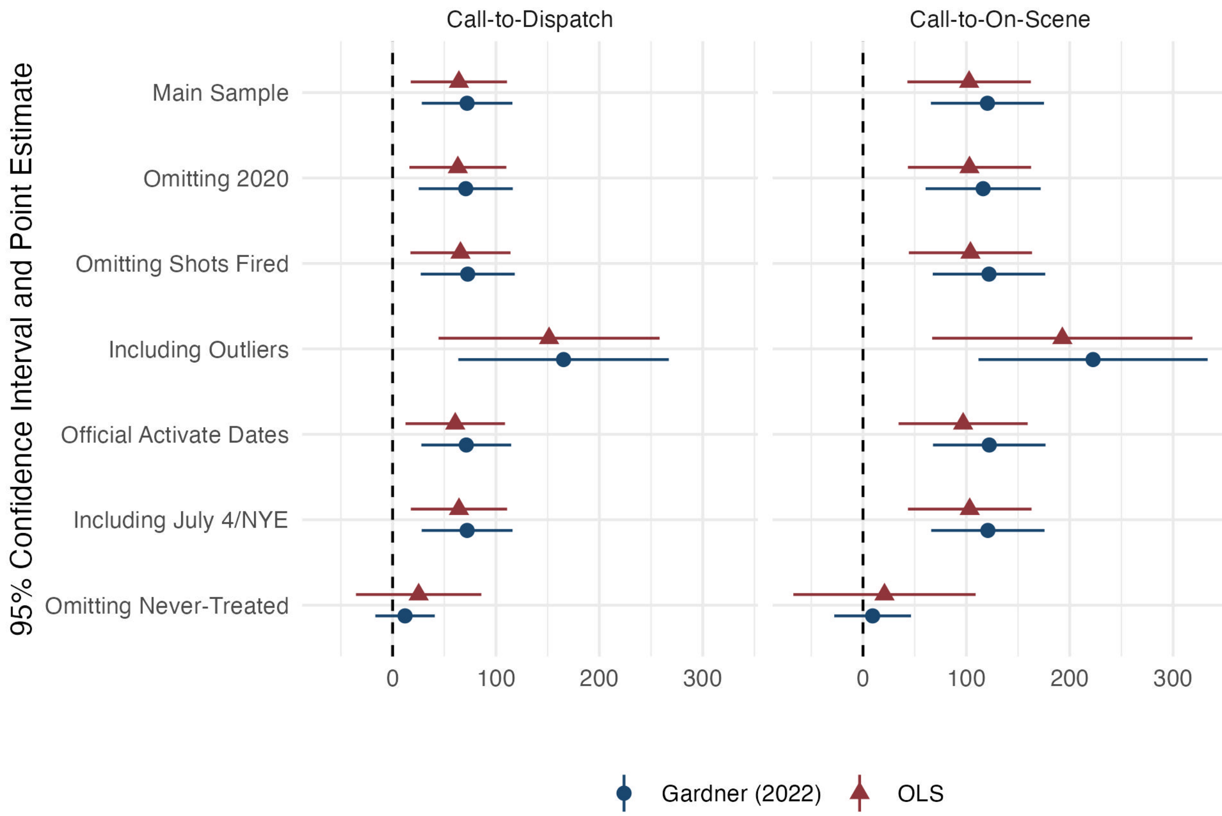


Figure D2: Robustness of Main Results

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. Next, we include July 4th, New Years Eve, and New Years day, which are excluded from the preferred sample since there may be many false-positive reports of gunfire. Last, Omitting Never-Treated uses the full sample, but omits any police districts that did not receive ShotSpotter technology.

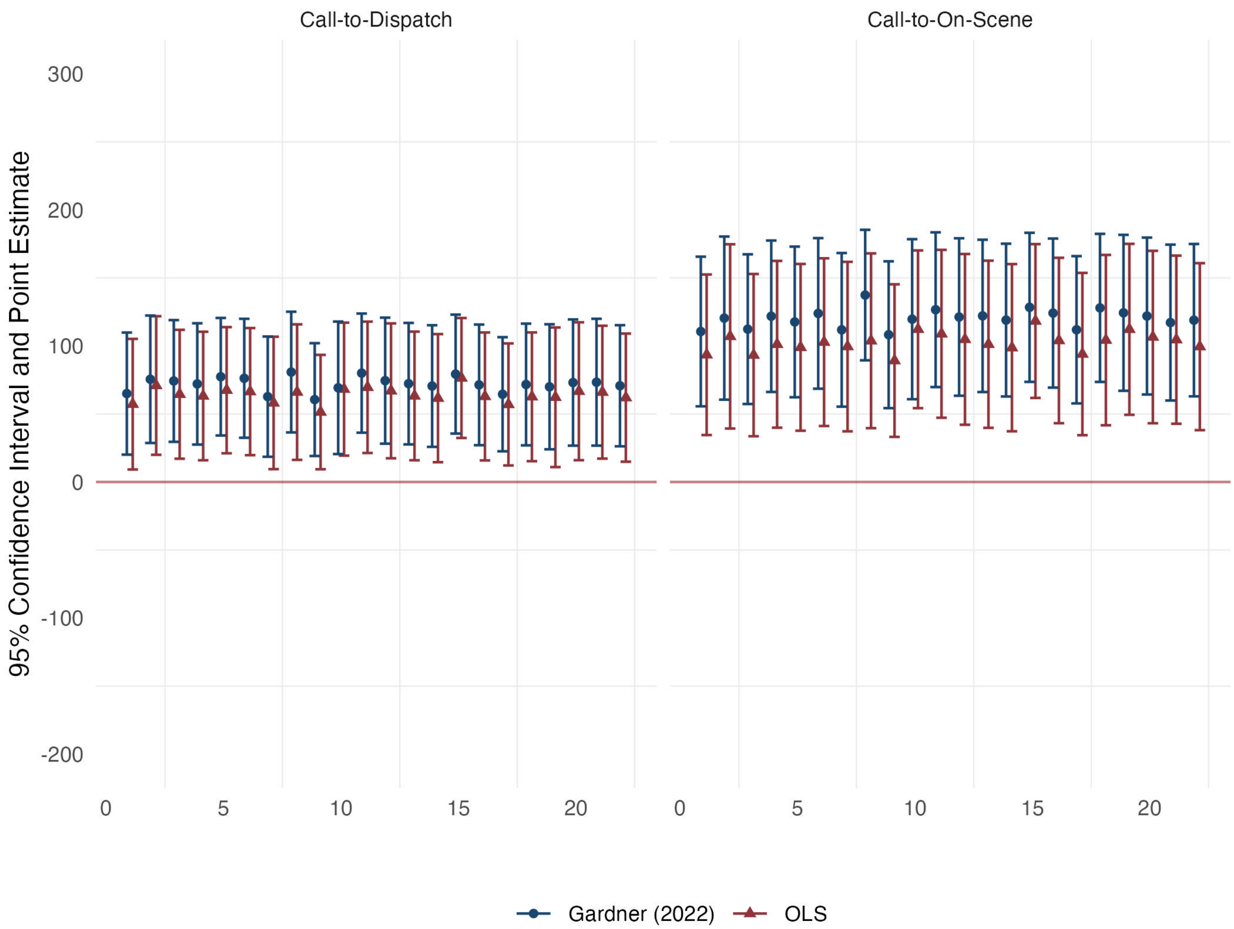


Figure D3: Leave-One-Out Analysis

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.

B Appendix Tables

Table D1: Bad Controls (OLS)

	(1)	(2)
Panel A: Number 911 Dispatches		
ShotSpotter Activated	-4.087***	-4.301***
	(0.989)	(1.241)
Mean of Dependent Variable	73.011	73.011
Observations	3,582,560	3,582,528
Panel B: Officer Hours		
	(22.204)	(26.299)
Mean of Dependent Variable	1,259.497	1,259.497
Observations	3,582,560	3,582,528
FE: Day-by-Month-by-Year	X	X
FE: District	X	X
FE: Hour-of-Day	X	X
Gardner (2022) Robust		X

Note:
All descriptions are from confirmed 911 dispatches that resulted in an injury. Time-Insensitive 911 dispatches are those in which an injury has likely been realized prior to an officer dispatch. On the other hand, Time-Sensitive dispatches are those in which a victim may avoid injury if an officer can rapidly respond in time to intervene.

Table D2: Categorization for Injury-Related Dispatches

Injury-Confirmed Call Descriptions		
Injury Realized	Potential for Injury	Gun-Related
DOMESTIC BATTERY	DOMESTIC DISTURBANCE	PERSON WITH A GUN
BATTERY IP	SHOTS FIRED	SHOTS FIRED
PERSON SHOT	PERSON WITH A GUN	
PERSON STABBED	PERSON WITH A KNIFE	
ROBBERY JO	CHECK WELL BEING	
EMS	PERSON CALLING FOR HELP	
CRIM SEX ASSLT RPT	VIOLATION ORDER OF PROT	
ASSAULT IP	MENTAL HEALTH DISTURBANCE	
CRIM SEX ASSLT JO	CRIM DAM. TO PROP IP	
SEX OFFENSE OTHER	SUSPICIOUS PERSON	
CHILD ABUSE	DISTURBANCE MENTAL	
KIDNAPPING J/O	ROBBERY IP	
ALARM CTA TRAIN	BURGLARY IP	
DEATH UNKNOWN	CRIMINAL TRESPASS IP	
HAZ MAT	HOLDING OFFENDER (CITZ.)	
1-Oct	THEFT IP	
BOAT ACCIDENT PROPERTY DAMAGE	ASSIST POLICE	
EXPLOSION	ALARM BUS	
SUSPICIOUS MAIL	ALARM PANIC	
	THREATENING SUICIDE	
	ALARM HOLD UP	
	KIDNAPPING I/P	
	FIRE	
	KIDNAPPING REPORT	
	AUTO THEFT IP	
	CRIM SEX ASSLT IP	
	DUI DRIVER	
	CHILD LEFT ALONE	
	ARSON IP	
	DECEPTIVE PRACTICE IP	
	VOOP - Cindy Bischof' Law	
	BANK HOLD UP ALARM	
	MISSING PER. TENDER AGE	
	CHILD ABDUCTION	
	ALARM CAB	
	LOOTING	
	BOMB THREAT	
	MARINE DISTRESS	

Note:
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