The Effect of ShotSpotter Technology on Police Response Times

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Abstract

ShotSpotter is an acoustic gunfire detection technology utilized by police departments in over 150 cities world-wide with the intention of rapidly dispatching police officers to violent crime scenes in an effort to reduce gun violence. In Chicago, this amounts to approximately 70 instances per-day whereby officers are immediately dispatched to potential instances of gunfire. However, this allocation diverts police resources away from confirmed reports of 911 emergencies, creating delays in rapid response—a critical component of policing with health and safety implications. In this paper, we utilize variation in timing from ShotSpotter rollouts 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 implementation causes police officers to be dispatched one-minute slower (23% increase) and arrive on-scene nearly two-minutes later (13% increase). Moreover, these effects are driven by periods with fewer police on-duty and times of day with larger numbers of ShotSpotter-related dispatches. Consequently, when responding to emergency calls, police officers' success rate in arresting perpetrators decreases by approximately 9%, with notably large decreases in arrests for domestic battery (14%).

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1 Introduction

In the contemporary workplace, artificial intelligence (AI) possesses the potential to serve as either a substitute or complement to human capital—police forces are no exception. As of 2023, police departments are utilizing AI technologies as substitutes, effectively functioning as ‘eyes-on-the-street’ through facial recognition and traffic cameras, as well as a collaborative complement in targeting high-crime areas. These AI technologies are seen as imperative for public safety moving forward, addressing the issues of both officer shortages and eroding public opinion of the police (Gallup, 2022). Nevertheless, the integration of officers and AI systems is fundamentally reshaping the nature of policing.

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 with the goal of reducing gun violence. The technology utilizes an array of microphones and sensors placed on streetlights 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. In effect, previous studies have utilized this feature of ShotSpotter as a measure of underlying crime that is independent of reporting habits (Carr and Doleac, 2016, 2018; Ang et al., 2021). As a result, it has been estimated that only 12% of gunfire is reported, leaving a significant portion of these occurrences unattended (Carr and Doleac, 2016). Therefore, ShotSpotter offers a solution wherein police officers are dispatched to additional instances of potential gunfire. In Chicago, the setting of this paper, this results in approximately 70 ShotSpotter-related dispatches each day, equating to 75 total hours of officer time.[1] This represents a two-fold increase in the number of gunfire reports that require officers to engage in rapid response.[2]

However, reallocating resources to gunfire detection changes an officer's time allocation. On one hand, this reallocation could be beneficial—ShotSpotter may frequently place officers closer to locations that foster higher volumes of crime. In this situation, an officer's time of arrival may be reduced. On the other hand, these investigations of previously unreported gunfire may incapacitate officers from attending to confirmed reports of other crimes in the form of 911 calls—a lifeline for citizens in distress. In effect, these calls may suffer from increased response times, as officers are busy investigating ShotSpotter detections.[3] Consequently, this may have far-reaching implications given the critical importance of rapid response, which has shown to alter the probability of crime clearance (Blanes i Vidal and Kirchmaier, 2018) and victim injury (DeAngelo et al., 2023). Furthermore, response times may affect timely medical treatment, as emergency medical personnel are required to delay their services until police arrive if their safety is compromised.[4] Thus, while ShotSpotter is implemented with the intention of enhancing public safety, it may have unintended consequences that are socially costly.

In this paper, we utilize variation in timing from the staggered ShotSpotter rollout across Chicago police districts from 2016-2022 to estimate the causal effect of ShotSpotter technology on the response times from 911 calls designated as Priority 1—the most

[1]A ShotSpotter investigation takes roughly 20 minutes to complete. While we cannot delineate between the number of officers dispatched to the scene for our entire sample period, we find, using another source of data from 2019-2023, that the average number of officers dispatched to a ShotSpotter detection is approximately 3.35. On the other hand, a lower bound, assuming only one officer dispatched to each ShotSpotter alert, would result in 23 total hours.

[2]This statistic is based on the average number of 911 dispatches relating to a 'Shots Fired' report and the average number of ShotSpotter dispatches post-implementation in all police districts.

[3]Two reports from Chicago show descriptive evidence that ShotSpotter dispatches may be unproductive (Ferguson and Witzburg, 2021; Manes, 2021). As discussed in Section 7, we find descriptive evidence corroborating these. However, given the data limitations, we cannot truly verify whether ShotSpotter dispatches are more or less productive than a 911 dispatch.

[4]This is found from the Chicago EMS System Policies and Procedures: `https://chicagoems.org/wp-content/uploads/sites/2/2017/08/2017-PP_APPROVED.pdf`

frequent call classification in Chicago which pertains to life-threatening and time-sensitive events. Using 911 call dispatch data from the Chicago Police Department (CPD), we construct two measures of police response: the time from a 911 call to when a dispatcher finds an available police officer for dispatch (Call-to-Dispatch) and the time from a 911 call to when the officer arrives on-scene (Call-to-On-Scene). By applying a staggered difference-in-differences framework, we find that both Call-to-Dispatch time and Call-to-On-Scene time are significantly increased following the implementation of ShotSpotter by approximately one minute (23%) and two minutes (13%) respectively. These estimates are robust to a variety of sensitivity tests and estimators.

Moreover, we find that the delays in response times are driven by resource-constrained periods, consistent with the hypothesis that ShotSpotter is affecting police officers' time constraints. We test this using days when there are fewer officers on-duty and times of day with higher numbers of ShotSpotter detections. Each of these subsets show significantly larger effect sizes during these resource-constrained periods, suggesting that ShotSpotter forces officers to make trade-offs in favor of responding to ShotSpotter alerts. Consistent with this mechanism, response times from other time-sensitive calls (Priority 2) are also increased, and in addition, time-insensitive calls (Priority 3) show suggestive evidence of longer delays, providing further evidence of heightened officer responsibilities.

Consequently, these elevated response times come at a significant cost. In Section 5.3, 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 arrested, consistent with Blanes i Vidal and Kirchmaier (2018) who attribute faster rapid response to higher crime clearance rates. The effect is particularly strong in calls regarding domestic battery (14%) and domestic disturbances (13%)—two situations where reoffending is likely (Maxwell et al., 2001). However, distinct from this previous work, we are able to closely examine a determinant of rapid-response directly, rather than focus solely on its consequences.

Despite these unintended consequences, we also find suggestive evidence that ShotSpotter may reduce the probability of gun-related 911 calls resulting in a victim injury. Although only suggestive, this hints at the possibility that gun-related 911 calls may benefit from ShotSpotter technology by corroborating 911 reports of gunshots and providing more accurate location information for police officers to rapidly intervene (Piza et al., 2023). However, we find no evidence of these effects for non-gun-related 911 calls and cannot rule out the possibility of increases in victim injuries from delayed police response, as found in DeAngelo et al. (2023).

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. While previous studies have found positive effects of criminal justice and police technology 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), military-grade equipment (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 unintended consequences that are both fiscally and socially expensive.[5]

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 elasticity of crime with respect to police presence, generally finding that increased police presence lowers crime (Chalfin and McCrary, 2018; Weisburd, 2021; Mello, 2019). Of these works, the most related is Weisburd (2021), which leverages changes in police locations, prompted by service calls, to explore a reduction in the availability of police officers that arises from

[5]Chicago is estimated to spend approximately 8.9 million each year on ShotSpotter technology. For comparison, a 2016 estimate put body-worn cameras at 6.5 million annually.

increased demand for police officer time. However, in contrast to Weisburd (2021), this study unpacks a mechanism which determines response times, allowing us to explore how the time constraints of police officers affect their availability to respond to crime. We find that when police resources are stretched thin, the effectiveness of a police force to respond to crimes and arrest perpetrators is diminished. As a result, our findings suggest that implementing a personnel-intensive policy should be paired with an increase in officer availability, achieved through hiring or redistributing responsibilities, in order to prevent under-policing in communities.

Lastly, we build upon the rapid-response literature related to health outcomes. In Section 6.1 we find that police dispatches for emergency medical services are delayed by nearly one minute due to ShotSpotter implementation. As mentioned earlier, this could prolong treatment to critical injuries if ambulance personnel are waiting for police to arrive to a crime scene. In turn, this could have significant implications, as longer travel times and ambulance response times have been linked to higher mortality rates (Avdic, 2016; Wilde, 2013).

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, mechanism, and effect on arrest probability, Section 6 discusses other outcomes and implications, and Section 7 concludes.

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 police departments to rapidly deploy police officers to the potential crime scene. Over the past decade, this technology has seen significant expansion and is now operational in over 150 cities globally. The rationale for adopting ShotSpotter is to enable police departments to respond to gunfire faster and with more geographic precision. Moreover, the unique functionality of ShotSpotter allows police departments to bypass their reliance on civilian reporting, which only accounts for approximately 12% of gunfire occurrences (Carr and Doleac, 2016). While previous studies support some of these rationales in the form of geographic accuracy (Piza et al., 2023) and faster gun-related dispatch times (Choi et al., 2014), others have found little impact on gun violence (Mares and Blackburn, 2012) and case resolution (Choi et al., 2014).

The technology relies on machine learning algorithms to classify sounds of potential gunfire.[6] When a potential gunshot is detected, the sensors triangulate the location of the noise and data/recordings of 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 then send officers to the scene. This entire process from gunshot noise to police dispatch is known as a *ShotSpotter dispatch*.

[6] 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.

In Chicago, ShotSpotter technology has been implemented in 12 of the 22 police districts in order to respond to gun-related issues faster and with more geographic accuracy.[7] The staggered roll-out began in January 2017, coinciding closely with new Strategic Decision and Support Centers (see Section 4.2 for more details), in response to the large influx in gun violence in 2016.[8] ShotSpotter was first implemented in the districts with the highest rates of gun violence, and after evaluation, was subsequently implemented in less violent areas.[9] The expansion ended in May 2018, with no further police districts receiving the technology. Appendix Figure D1 shows the locations of the 12 police districts in Chicago that received ShotSpotter technology. As mentioned, 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 and Communications (OEMC) and the Chicago Police Department (CPD). The OEMC oversees 911 calls and dispatches police officers from the CPD. Each 911 call is prioritized on a scale of imminent danger/threat ranging from Priority 1 (immediate dispatch) to Priority 3 (routine dispatch).[10]

When a 911 call is made, the call is received by an OEMC call-taker who records

[7] In Chicago, each police district has a population of approximately 100k.

[8] This wide-scale adoption follows previous testing of select areas between 2003 and 2007, 2012, and again in 2016. However, to our knowledge, 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/). Moreover, there appears to be no ShotSpotter dispatches in the data prior to the official dates. In an abundance of caution, we conduct a leave-one-out analysis and find that the results are consistent.

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

[10] Technically, there are six 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.

the caller's information, assigns a call type that they believe best characterizes the incident, and forwards this information to the dispatcher.[11] Next, the dispatcher assigns the event to an available CPD unit in the call's police district. Once the scene has been cleared, officers will notify the OEMC and will be marked as available for future call assignments.

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, estimated 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, there is a clear distinction between 911 calls and ShotSpotter dispatches. A 911 call is the result of a civilian reporting a crime, while a ShotSpotter dispatch is a police dispatch to the location of a potential gunfire sound from ShotSpotter sensors. The focus of this paper concerns only 911 calls, which we show to be impacted by the *presence* of ShotSpotter dispatches.

However, both 911 calls and ShotSpotter dispatches share a variety of operating procedure similarities. For instance, each ShotSpotter dispatch 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.[12] Hence, both Priority 1 911 calls and ShotSpotter dispatches share the same dispatch procedures and responding officers. Furthermore, the OEMC prioritizes both 911 calls and ShotSpotter dispatches to rapid response units and police officers within the police district of occurrence.[13] Only in

[11]Later in Section 3.1, we define the beginning of a 911 call as the time when a call-taker assigns a call-type. This is done rapidly and allows us to more closely target delays due to police officers.

[12]Priority 1 calls account for roughly 43% of all 911 calls during the sample period.

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

rare circumstances are police officers assigned to these emergencies outside their district.[14]

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. As a comparison, gun-related 911 calls prior to ShotSpotter average approximately 65 minutes.[15]

3 Data

3.1 Data Sources

The main sample contains several data sources from years 2016 to 2022 that are obtained through Freedom of Information Act requests to the Chicago Police Department (CPD). These data include 911 call dispatches, officer shifts of sworn police officers, incidents of crime, arrest reports, and district-level ShotSpotter activation dates.

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

[14] 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.

[15] This surprising discrepancy may be due to the productivity of ShotSpotter dispatches relative to 911 calls. Some reports in 2021 on the effectiveness of ShotSpotter dispatches in Chicago from the Office of the Inspector General and The MacArthur Justice center show descriptive evidence that ShotSpotter dispatches do not result in more gun-related evidence. However, this study stays ambivalent to these claims, as the data we use does not contain the same information.

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 the priority-level of the call, a brief description, a block-level location, and a case report number that can be linked to arrests and incident reports.

Based on this information, we construct the two main outcome variables: the time from the beginning of a 911 call to an officer being dispatched (Call-to-Dispatch) and the time from the beginning of a 911 call to an officer's arrival (Call-to-On-Scene). We define the beginning of the 911 call as the time that a 911 call-taker creates an event number for the associated incident—an action that typically occurs immediately following the call being received. Notably, while Call-to-Dispatch contains no missing data, approximately 45% of the Call-to-On-Scene information is missing. This is likely due to officers failing to report when they arrive at the scene (OIG, 2023). However, we address this potential limitation in Appendix A where we provide several analyses to maintain confidence in the Call-to-On-Scene results.

These two measures of rapid response capture separate degrees of police availability. First, if an officer is too busy, they will be delayed or unable to be dispatched. In particular, the officer will not be classified as available to take Priority 1 calls on the Computer Aided Dispatch (CAD) system, and a dispatcher will not assign them to a call. This increase in time would be observed as a higher Call-to-Dispatch time and is a function of the coordination between the 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 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 records provide only the month of implementation, we rely on the raw data corresponding to ShotSpotter dispatches to determine the specific activation day for each police district. Nonetheless, we observe several small discrepancies in the activation dates when comparing to the number of ShotSpotter dispatches in District 6, 9, 10, and 15. In particular, these districts have no ShotSpotter dispatches 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 plots the monthly trend of dispatches relating to both ShotSpotter and civilian reports of gunshots. In addition, the ShotSpotter activation dates are plotted with dashed red lines. In this figure, each police district exhibits an increase in ShotSpotter dispatches as time progresses. This is possibly due to a combination of ShotSpotter's machine learning algorithms refining with time, and the increasing amounts of gun violence which began in 2020. Notably, this figure also depicts the substantial increase in police resources devoted to gunfire post-implementation due to the addition of ShotSpotter detections.

3.2 Sample Restrictions

The main sample is restricted to only 911 call dispatches of Priority 1—the highest priority level.[16] Priority 1 is defined as any situation that may involve an imminent threat to life, bodily injury, or major property damage/loss. By including only Priority 1 calls, the analysis focuses only on the types of calls that require the most time-sensitive responses. However, for completeness, Section 6.1 analyzes lower-priority calls of Priority 2 and Priority 3.

As an important distinction, recall that 911 call dispatches do not include dispatches for ShotSpotter gunshot detections. While ShotSpotter detections are classified as Priority 1 and responded to by the same police units, these are not reported by civilians. By implementing this restriction, we ensure that we are comparing similar distributions of civilian reports of crime before and after the ShotSpotter rollout.

Three further restrictions are implemented to reduce potential noise in the response time data. 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, Call-to-Dispatch and Call-to-On-Scene outliers that exceed three standard deviations from the mean are omitted, which account for 0.4% and 1.6% of each outcome, respectively. This restriction mitigates the impact of potentially erroneous outliers on the ordinary least squares estimator, which is sensitive to extreme values. We relax this restriction in Appendix Figure D2 to verify the consistency of the results. Last, specific dates including January 1, July 4, and December 31 are excluded from the analysis. These dates coincide with celebratory gunfire and fireworks that may generate many false-positive ShotSpotter alerts. However, we also show that the results are robust to including these dates in

[16] Priority 0 is actually the highest level of priority, but this is a special case reserved for situations where police or firefighters are calling for assistance in life-threatening situations. These are extremely rare, and make up only 0.01% of the top four priority dispatches.

Appendix Figure D2.

3.3 Descriptive Statistics

Table 1 shows summary statistics of the main outcome variables in Panel A and corresponding secondary outcomes and control variables in Panel B. All statistics are based on only Priority 1 911 dispatches unless otherwise noted. Panel A reports that the average Call-to-Dispatch time is approximately five minutes, while the average Call-to-On-Scene time is approximately 13 minutes. Additionally, the distribution of these outcomes are plotted in Figure 2 showing that response times can be particularly lengthy (1+ hours) in rare cases. Furthermore, the probability of making an arrest on a 911 dispatch is low, with an average of 2%, while the likelihood of a victim being injured is roughly 3%.

In Panel B, Priority 2 and Priority 3 calls are reported to be less frequent than Priority 1. Priority 2 calls are defined as those in which timely police action has the potential to affect the outcome of an incident, while Priority 3 calls are those in which a reasonable delay in police action will not affect the outcome of the incident. Consistent with these definitions, Priority 2 and Priority 3 have slower response times for both Call-to-Dispatch and Call-to-On-Scene measures.

Furthermore, statistics on the number of Priority 1 911 dispatches, ShotSpotter dispatches, and number of officer hours, are reported in Panel C of Table 1—each measured at the district-day level. The average number of Priority 1 dispatches within each district-day is approximately 73, although these have considerable variability, with a maximum of 223. ShotSpotter dispatches are reported to be an average of approximately three per-district-day, yet this includes both time periods and districts that do not necessary have ShotSpotter implemented. When restricting the sample to only post-ShotSpotter implementation dates, the average number of ShotSpotter dispatches in each treated district-day is six ($\sim$ 70 city-wide). Finally, due to 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 231 officer hours to as many as 6,558 officer hours. We later analyze this heterogeneity in Section 5.2 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 equation using ordinary least squares (OLS):

$$ResponseTime_{cdt} = \beta ShotSpotter_{dt} + \eta_{\tilde{c}} + \delta_d + \gamma \mathbb{X}_{f(t)} + \varepsilon_{cdt} \tag{1}$$

where $ResponseTime_{cdt}$ is the Priority 1 Call-to-Dispatch or Call-to-On-Scene time for call c, in police district d, at time t. The treatment variable is $ShotSpotter_{dt}$, which is an indicator variable equal to one if police district d is equipped with ShotSpotter at time t. Moreover, $\eta_{\tilde{c}}$ and δ_d, are call-type and police district fixed effects respectively. $\mathbb{X}_{f(t)}$ is a vector of time-varying controls which include day-by-month-by-year and hour-of-the-day fixed effects. Last, ε_{cdt} 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 the main results as recommended by Cameron et al. (2008) since the number of clusters is below 30. Intuitively, Equation 1 is comparing response times on days with ShotSpotter activated to days without ShotSpotter activated, while accounting for the expected differences in call types, police districts, and different times of the year and day.

Controlling for the type of call, $\tilde{c}$, accounts for the fixed differences between different 911 calls.[17] While we restrict the main sample to only Priority 1 types, there is a possibility that dispatchers or officers may innately prioritize responding to certain call-types that they believe are most critical. By including call-type fixed effects, we circumvent this particular issue. Additionally, 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 controls for these fixed differences. Finally, day-by-month-by-year and hour-of-the-day fixed effects, $\mathbb{X}_{f(t)}$, are included to control for time-varying fluctuations that occur over particular days of each year and different times of the day.

4.2 Identification

The coefficient of interest in Equation 1 is β, which measures the average change in 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 in ShotSpotter districts would have continued on a similar trend to non-ShotSpotter districts in the absence of ShotSpotter, there is no change in 911 dispatching procedures post-ShotSpotter implementation, the distribution of 911 calls/dispatches did not change post-ShotSpotter, 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 ShotSpot-

[17] Each 911 call is given a final dispatch code. When controlling for type of call, we use the final dispatch code as the distinction.

ter would have continued to have similar response times to 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:

$$ResponseTime_{cdt} = \sum_{\substack{i=-12, \\ i\neq -1}}^{24} \beta^i \text{ShotSpotter}^i_{dt} + \eta_{\tilde{c}} + \delta_d + \gamma \mathbb{X}_{f(t)} + \varepsilon_{cdt} \tag{2}$$

where $ShotSpotter^i_{dt}$ is a set of indicators that are set to 1 if ShotSpotter is adopted i months from time t in district d. Each period is relative to the month before ShotSpotter adoption. Twelve periods pre-ShotSpotter are estimated to maintain a balanced panel, and 24 periods post-ShotSpotter are estimated, where the first and final periods are binned endpoints as described in Schmidheiny and Siegloch (2023). 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. Moreover, 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-differences imputation estimator (Gardner, 2021) and the OLS estimator. The two-stage difference-in-differences 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). Unlike the estimators proposed in Sun and Abraham (2021) and Callaway and Sant'Anna (2021), this estimator allows us to maintain the preferred day-by-month-by-year fixed effects while simultaneously estimating monthly bins without aggregation. Moreover, this estimator allows for comparisons of treated units between *both* never-treated and not-yet treated units. 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 5.1 (and more thoroughly in Appendix C) with a sensitivity test as described in Rambachan and Roth (2023) where we allow for relaxations of the common trends assumption.

The second assumption states that there is no change in how police are dispatched to 911 calls in the presence of ShotSpotter. Recall that this study only analyzes 911 call dispatches, and there is no indication that the operating procedures for 911 calls changes (CPD, 2016). However, the same police units that respond to Priority 1 911 dispatches also respond to ShotSpotter alerts, and therefore ShotSpotter increases an officer's set of responsibilities.

Third, we address the assumption that the distribution of 911 calls is not changing due to ShotSpotter implementation. For instance, one concern may be that dispatchers are combining 911 calls that relate to gunfire with ShotSpotter alerts in order to save officer resources. To mitigate this issue, we estimate Equation 1 removing 911 dispatches relating to civilians hearing gunfire.[18] The results remain consistent as shown in Appendix Figure D2. Additionally, in Section 6.1, we analyze distinct call-types and show that the effects persist even when analyzing individual types of 911 call.

For the final assumption that there are no other police department policies that directly coincide with ShotSpotter implementation, we discuss two initiatives that are implemented at similar (although not exact) time periods as ShotSpotter: Strategic Decision Support Centers (SDSCs) and Body Worn Cameras (BWC). A more thorough description

[18]This is approximately 8% of Priority 1 911 calls.

and analysis of these is presented in Appendix B, yet we report the key takeaways here.

To begin, SDSCs have the most similar implementation dates to ShotSpotter with an average of 73 days apart, although not all SDSCs are equipped with ShotSpotter technology as shown in Appendix Table B1. SDSCs are housed with policing technology software such as police observation displays, geospatial predictive policing software, and social media monitoring. However, only one of these technologies coincides directly with the SDSC roll-out (geospatial predictive policing), and the others have been utilized in Chicago for years prior. While we understand that predictive policing software may change officer patrolling patterns, and therefore affect response times, a thorough study of this particular software implementation is discussed in Kapustin et al. (2022) where they find patrolling changes in only two of Chicago's police districts. In Appendix B, we estimate the main results and the corresponding event studies while controlling for SDSC roll-out dates, and report consistent findings with the main results. In addition, we perform separate analysis removing the two districts where patrolling tactics changed, and find similar conclusions. Finally, in Section 5.2, we present intensive margin estimates of ShotSpotter using the number of ShotSpotter dispatches as identifying variation. This variation is less correlated with the SDSC roll-out, and provides further evidence that ShotSpotter is causing the increase in response times.

Last, BWCs are another technology that are implemented near ShotSpotter dates, although the district-timing differs by 283 days on average (see Appendix Table B1). In Appendix Table B2, we control for the BWC implementation and find little differences from the main results. This aligns with intuition, as body worn cameras have been found to affect complaints (Kim, 2019; Braga et al., 2022; Zamoff et al., 2022; Ferrazares, 2023) and stops (Braga et al., 2022; Zamoff et al., 2022), but are unlikely to affect an officer's ability to rapidly respond.

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 analyze dynamic effects and present evidence that ShotSpotter affects response times by constraining officer resources. Last, we show that increased response times lead to fewer perpetrators being arrested, thereby showing that ShotSpotter has costly implications.

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 receive ShotSpotter show a substantial increase in the average Call-to-Dispatch and Call-to-On-Scene times. 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 or subsequently arrives at the scene, respectively. First, in Column 1 of Table 2, we estimate Equation 1 with only the time and group fixed effects. We find a statistically significant increase in Call-to-Dispatch and Call-to-On-Scene times of 64 seconds and 101 seconds, respectively. Remarkably, the Call-to-On-Scene estimates show that travel time is increasing by approximately 40 seconds in addition to the delays in finding responding officers to dispatch. This suggests that ShotSpotter is not placing officers in areas closer to the majority of other 911 call locations, whereby travel time may be reduced.

Column 2 of Panel A and Panel B report estimates from the preferred specification outlined in Section 4.2 where we supplement the model in Column 1 with controls for time-of-day and the type of 911 call. When including these controls, the results for both Call-to-Dispatch and Call-to-On-Scene times are similar, showing increases from the mean of approximately 22% and 13%, respectively. In Column 3, we further enrich the model to include controls for both the number of 911 dispatches and officer hours per-district-day 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. However, prior literature suggests that controls that are significantly affected by treatment could cause substantial bias in the coefficient estimates (Angrist and Pischke, 2009; Wooldridge, 2010). While we find ShotSpotter implementation is unrelated to the number of 911 dispatches and officer hours (Appendix Table D1), we omit these from the preferred specification out of an abundance of caution.

Given the staggered difference-in-differences research design, Column 4 reports estimates that are robust to treatment heterogeneity across groups and over time using the two-stage difference-in-differences imputation estimator (Gardner, 2021). This estimator equally weights each district-date estimate, making it robust 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). We opt to use this estimator since it allows for comparisons of treated units between *both* never treated units and not-yet treated units and requires no aggregation, unlike similar approaches discussed in Callaway and Sant'Anna (2021). The estimates, albeit slightly larger than the preferred specification, remain consistent with the main findings.

Furthermore, we consider spillover effects in Column 4 by including an indicator variable (Border Activated) equal to one for any police district that is adjacent to a ShotSpotter-activated district. In effect, the coefficient on the indicator for a neighboring ShotSpotter district measures the spillover impacts of the implementation. 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 discussed in Section 2.2 whereby officers are only dispatched outside their beat/district of patrol in rare circumstances.

Next, to analyze the dynamic effects of ShotSpotter implementation over time, we estimate an event study using Equation 2. We estimate this model using both OLS and the Gardner (2021) robust estimator to account for potential treatment heterogeneity across groups and time periods. Figure 3 and Figure 4, for Call-to-Dispatch and Call-to-On-Scene respectively, show that the effect of ShotSpotter implementation takes several months post-implementation to significantly alter response times. In each figure, the red error-bars represent the 95% confidence intervals using OLS, while the blue error bars are derived from the Gardner (2021) estimator. We attribute the delayed effect in response times to a composition of ShotSpotter's functionality and overall violence in the city. 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. Moreover, violent crime also began to increase in Chicago beginning in 2020, which may also contribute to this slightly delayed response. As shown previously in Figure 1, the number of ShotSpotter dispatches appears to be increasing over time across each district.

Importantly, these main results are robust to a variety of sample selections and sensitivity tests. First, Appendix Figure D2 shows estimations of Equation 1 for six different sample selections estimated with both OLS and the Gardner (2021) robust estimator: omitting the year 2020 (Covid-19 pandemic), omitting 911 calls for gun shots fired (in case 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, including January 1/July 4/December 31 which may have many false-positive 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 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 unique 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. Finally, in Appendix C, we conduct analysis following Rambachan and Roth (2023) to illustrate the sensitivity of the event study 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 while maintaining a significant post-treatment average treatment effect. As explained further in Appendix B, we find that the average of all post-implementation periods maintain their statistical significance under both a linear extrapolation of the pre-period and increasing amounts of non-linearity for both the Call-to-Dispatch and Call-to-On-Scene time.

5.2 Mechanism - Resource Constraints

In this subsection, we provide evidence that the longer response times associated with ShotSpotter are a result of the allocation of scarce police resources. Recall from Section 3.3 that post-implementation, there are approximately 70 ShotSpotter dispatches each day in Chicago—a two-fold increase in the number of gunfire-related incidents officers must respond to compared to pre-implementation. These dispatches are resource-intensive, taking an average of 20 minutes each, which collectively amounts to roughly 75 hours of officer time allocated to ShotSpotter.[19] To establish this link, we conduct three sets of analyses to show that ShotSpotter creates longer 911 response time delays on both the extensive margin (implementation) and the intensive margin (number of ShotSpotter dispatches).

First, on the extensive margin, we differentiate the effect of ShotSpotter by officer watch schedules, which represent times when officers begin and end their shift. This division allows us to examine periods with varying levels of ShotSpotter dispatches, wherein officers may be more or less constrained by attending to ShotSpotter investigations. Panel A of Figure 6 plots the distribution of ShotSpotter dispatches by the hour of the day and corresponding watch. As shown in the figure, the nighttime shifts of Watch 1 (11:00pm - 7:00am) and Watch 3 (3:00pm - 11:00pm) have significantly higher counts of ShotSpotter dispatches than Watch 2 (7:00am - 3:00pm).[20]

In Panel B of Figure 6, we plot estimations of Equation 1 by officer watch and show that shift times with higher levels of ShotSpotter dispatches have longer response time delays. On the x-axis, each coefficient estimate and 95% confidence interval is plotted for the corresponding watch number on the y-axis. For both Call-to-Dispatch and Call-to-On-Scene times, the magnitude of the effects correspond to the distribution of ShotSpotter dispatches in Panel A; Watch 1 and Watch 3 exhibit effects that are both statistically significant and larger in magnitude than Watch 2. Moreover, while the Call-to-On-Scene delays reach nearly 3 minutes in Watch 3, the Call-to-On-Scene estimates are near-zero for Watch 2, and are not statistically significant.

[19]As mentioned in the introduction, we calculate this using the average number of officers that are dispatched to ShotSpotter detections over a sample period of 2019-2023 (roughly three officers). Unfortunately, records retention schedules did not allow us to receive this data for our sample period.

[20]The typical police watches in Chicago last for 9 hours total with a 45-minute briefing to begin the shift. We use 8-hour intervals to account for these briefings.

Second, also on the extensive margin, we show that the longer response times are driven by district-days that have fewer officers on duty. Similar to the prior analysis, this tests the notion that times with less officer availability will result in larger effects. In Columns 2 and 3 of Table 3, we split the sample by the district-day median of officer availability. We measure officer availability using the number of working hours from all police officers within a district-day. Column 2 shows estimates from district-days that have officer availability above the median and are therefore less resource constrained. The percentage change for both Call-to-Dispatch and Call-to-On-Scene are 14% and 8% respectively, suggesting that ShotSpotter does not impact response times as significantly when there are ample officer resources. On the other hand, Column 3 shows that when officer availability are below the district-day median, ShotSpotter's effect on response times are greatly increased. In particular, Call-to-Dispatch and Call-to-On-Scene times exhibit percentage changes of 27% and 17%, which are higher than the pooled estimates of 23% and 13% in Column 1, respectively. Interestingly, the larger effects in both outcomes suggest that dispatchers struggle to find an available officer to dispatch and that officers are placed in areas increasingly far away from other reports of crimes.

Finally, on the intensive margin, we exploit an alternative source of variation to test whether ShotSpotter allocates resources away from 911 calls: the number of daily ShotSpotter dispatches within a district. Recall from Section 2 that ShotSpotter dispatches are the result of ShotSpotter sensors detecting gunfire, which are distinct from civilian 911 calls. To do so, Equation 1 is modified to the following:

$$ResponseTime_{dt} = \zeta ShotSpotterDispatches_{dt} + \delta_d + \gamma_t + \varepsilon_{dt} \tag{3}$$

where $ShotSpotterDispatches_{dt}$ is the number of dispatches attributed to ShotSpotter alerts in district d at time t, δ_d are police district fixed effects, and γ_t are day-by-month-by-year fixed effects. Importantly, since the identifying variation is at the district-day level (rather than the call-level), we aggregate the call-level response times to the district-day. Hence, $ReponseTime_{dt}$ represents the *average* response time in police district d at time t. Furthermore, the identifying assumption in this specification is that the number of detected gunshots within a district-day is uncorrelated with confounding factors in ε_{dt} that may affect response times. To ensure we isolate the effects of the intensive margin, rather than ShotSpotter implementation itself, we restrict the sample to treated police districts and days when ShotSpotter has been implemented.

Consequently, this alternative specification more precisely tests the hypothesis that ShotSpotter affects response times by diverting officer resources away from 911 calls. If true, 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 measures the marginal effect of an additional ShotSpotter dispatch.

Column 4 of Table 3 shows that one additional ShotSpotter dispatch is associated with an increase in the average Call-to-Dispatch time of 6 seconds and an increase in the average Call-to-On-Scene time of 8 seconds. These results are statistically significant at the 1% level. However, we note that these results are under the assumption of a linear relationship between the number of ShotSpotter dispatches and response times. We show the plausibility of this assumption in Appendix Figure D4 where we split the number of ShotSpotter dispatches into deciles and re-estimate Equation 3. Interestingly, we find that each response time increases monotonically with ShotSpotter dispatches, further implicating the incapacitation effect that ShotSpotter has on police officers.

Taken together, these findings underscore the significance of police resource allocation within a day. If ShotSpotter affects response times by overloading officer responsibilities, then it is imperative to reallocate the appropriate amount of staffing to times when ShotSpotter dispatches are more frequent.

5.3 Impact on Arrest Probability

Although the findings demonstrate that ShotSpotter affects police officer response times, we acknowledge that this influence might not necessarily yield detrimental consequences if it does not affect the likelihood of apprehending perpetrators. To address this concern, we examine the potential changes in arrest probability associated with the observed increases in response times. We begin by merging the 911 dispatch data with arrest records, utilizing incident report number as the common identifier.[21] In doing so, we build on the results of Blanes i Vidal and Kirchmaier (2018), who find that increases in response times lowers the likelihood of a crime being cleared. Similarly, 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.

Table 4 shows the results from estimation of Equation 1 focusing on the probability of arrest for Priority 1 dispatches as the dependent variable.[22] In Column 1, the analysis reveals that the arrest likelihood decreases by 9% relative to the mean. This finding is statistically significant at the 1% level and highlights the substantial costs that extended response times impose on community safety and crime resolution.

Column 2 and Column 3 separate the effect on arrests into 911 calls that are categorized as gun-related and non-gun-related calls.[23] Notably, Column 3 highlights that

[21] We use two sets of arrest data. Arrests from the arrest database, and also case reports that end in arrests. Based on conversations with the Chicago Police Department, this is the best way to map 911 calls to arrests.

[22] In addition, we estimate this table using logistic regressions rather than OLS. The results are shown in Appendix Table D2. The results remain consistent.

[23] We classify gun-related 911 calls as those with descriptions of ‘person with a gun’, ‘shots fired’, and ‘person shot’.

the decline in arrest probability is driven by 911 calls that are unrelated to gun crimes. Conversely, Column 2 suggests that there is no change in the probability of a gun-related 911 call ending in an arrest, indicating that ShotSpotter might effectively guide officers to the vicinity of gun-related incidents, thus mitigating the impact of a delayed response.

In Columns 4-6, we isolate the effects for the three most frequent calls that end in arrests: domestic battery, domestic disturbance, and battery. Columns 4 and 5 report that the arrest probability for domestic disturbance and domestic battery both exhibit a statistically significant decline of 13% and 14%, respectively.

In light of these findings, it is evident that the observed impacts of ShotSpotter-induced delays extend beyond their immediate effect on police arrival. Specifically, the decreases in arrest rates for domestic disturbance and battery could potentially have significant implications for the victims, as domestic violence offenders are likely to reoffend (Maxwell et al., 2001). These results not only highlight the importance of efficient response times in enhancing crime resolution, but also underscore the health implications that may arise in terms of domestic battery.

6 Discussion

6.1 How does ShotSpotter affect other priority response times?

Within this subsection, we pivot the analysis beyond response times for Priority 1 dispatches to lower level priorities, Priority 2 (rapid dispatch) and Priority 3 (routine dispatch).[24] In doing so, we show implications that extend beyond Priority 1 dispatches, introducing trade-offs that dispatchers and officers face for lower-level reports of crime.

[24] A Priority 2 dispatch is defined as a response in which timely police action which has the potential to affect the outcome of an incident. A Priority 3 dispatch is defined as a response to a call for service that does not involve an imminent threat to life, bodily injury, or major property damage/loss, and a reasonable delay in police action will not affect the outcome of the incident.

Specifically, we find a 'trickle-down' effect, wherein time-sensitive lower-priority calls (Priority 2) are also impacted by ShotSpotter implementation. Interestingly, we find suggestive evidence that time-insensitive dispatches (Priority 3) may also be affected, implying a potential strain on officers' responsibilities when ShotSpotter is implemented. Moreover, we separately analyze the five most frequent types of calls within each priority. This provides two benefits; first, we are able to determine which types of calls drive the overall results, and second, we can mitigate the concern that ShotSpotter is leading to a change in the distribution of call types. Surprisingly, this analysis leads to significant health implications where ShotSpotter may be unintentionally costly for victims in need of medical services.

First, Equation 1 is estimated by priority on Call-to-Dispatch and Call-to-On-Scene times in Figures 7 and 8, respectively. In each figure, the point estimates and confidence intervals are divided by the mean of the dependent variable to show percentage changes. As an example, the top rows of each corresponding priority, labeled "Pooled Estimate," represent the 95% confidence intervals for the percentage change from the mean. Moreover, within each priority, the five most frequent call types are uniquely estimated and plotted in descending order of their mean response time. For instance, in the Priority 1 panel of Figure 7, the call description Battery in Progress has the lowest average Call-to-Dispatch time, while Suspicious Person and Check Well Being have the second and third lowest. Using this ranking, we find that the Priority 1 call-types that have the fastest response times exhibit the largest effects for both outcomes after ShotSpotter implementation.

As shown in the first row of both Figure 7 and Figure 8, labeled Pooled Estimate, Priority 2 response times for both outcomes show significant increases. Priority 2 calls are categorized as incidents that are non-life-threatening, but where police intervention may affect the outcome of the event. This significant increase in Priority 2 response times suggests a 'trickle down' effect from delays in Priority 1 dispatches. Intuitively, an officer that is delayed for a higher priority call, may also be delayed for less important tasks. However, for Priority 3 calls, which are time insensitive, we find only suggestive evidence of increased response times as Call-to-Dispatch is not statistically significant and Call-to-On-Scene is significant at the 10% level. Despite this, the point estimates for Priority 3 calls are positive, and the insignificant estimates may be a result of the large average response times for Priority 3 call types. As shown in the first row of Figures 7 and 8, the average response times for Priority 3 Call-to-Dispatch and Call-to-On-Scene are 16 minutes and 31 minutes, respectively. Given that these averages are substantially larger than Priority 1 and Priority 2, the estimated change in average time may not be large enough to detect. Despite this limitation, the positive coefficient estimates support the notion that officers' responsibilities are strained in the presence of ShotSpotter, creating further delays in responding to time-insensitive calls.

Second, as mentioned, Equation 1 is estimated for each of the five most frequent call types by priority. The results of these estimations are also plotted in Figures 7 and 8 below the Pooled Estimate. For Priority 1 and Priority 2 calls, we find consistent evidence of increased delays for both response times for nearly all call-types, thus showing that the effects are wide-spread across different emergency situations. Of notable importance, Figure 7 reports longer Call-to-On-Scene times for Emergency Medical Services (EMS), which may have significant health implications. In particular, the point estimate reports a 69-second increase in the response time for EMS calls. According to the Chicago EMS System Policies and Procedures, treatment and transport of injured civilians should be delayed pending police arrival if the safety of the EMS personnel could be jeopardized. Therefore, this observed delay in police response may postpone critical medical services. Specifically, Wilde (2013) find that a minute increase in response times increases mortality between 8-17%. Given the additional minute increase we find in Call-to-On-Scene times, ShotSpotter may have significant social costs beyond a lower likelihood of arresting perpetrators, and may hinder injured civilians from receiving timely care.

6.2 Are victim injuries more likely?

Given that faster police response times have been shown to lower the probability of a victim injury (DeAngelo et al., 2023), we study this possibility in our setting where ShotSpotter is causing slower response times. Specifically, we create a binary outcome variable for any Priority 1 911 call that results in a victim being injured. We perform two analyses: first, we estimate the overall effect of ShotSpotter implementation on the likelihood of a 911 call resulting in a victim injury, and second, we separate this effect by gun-related calls and non-gun-related calls. In doing so, we test the notion that ShotSpotter may have differential effects on gun-related calls, since ShotSpotter can increase locational precision of 911 calls regarding gun-violence (Piza et al., 2023).

In Column 1 of Table 5, there is little evidence of a change in the probability of a victim injury following a 911 call. Column 1 is estimated using Equation 1 where the dependent variable is an indicator equal to one if the 911 call resulted in a victim injury.[25] Although the coefficient estimate is negative, there is no statistical significance.

Moving on, Columns 2 and 3 of Table 5 split the sample by gun-related and non-gun-related 911 calls, respectively.[26] While there appears to be no change in the probability of a victim injury for non-gun-related calls, Column 2 shows suggestive decreases in victim injuries for gun-related calls of approximately 6% which is statistically significant at the 10% level. This result suggests that ShotSpotter may place officers closer to particular gun-related 911 calls. For instance, if a 911 call is corroborated with a ShotSpotter

[25] We also estimate these results using logistic regressions as shown in Appendix Table D3. The results are mostly consistent, showing that the effects are driven by gun-related 911 calls. However, the pooled estimates show statistical significance when using this estimation.

[26] Gun-related crimes are those that have the call descriptions 'SHOTS FIRED', 'PERSON WITH A GUN', and 'PERSON SHOT'.

alert, ShotSpotter's triangulation component may provide officers better locational precision, placing them closer to the crime scene whereby they can intervene. As mentioned earlier, there is evidence that ShotSpotter increases the locational precision of the crime scene that is relayed to officers.

Importantly, the pooled and non-gun-related findings in Columns 1 and 3 do not rule out the possibility of increased victim injury, as found in DeAngelo et al. (2023). Moreover, we note several differences in our analysis; we focus on Priority 1 calls rather than Priority 2, and we are unable to observe a victim injury if the victim is a minor (approximately 11% of all victims).[27] Therefore, although we find suggestive evidence of decreases in victim injuries for gun-related 911 calls, we cannot reject the possibility of increases in victim injuries for non-gun-related calls.

7 Conclusion

In this study, we analyze the effect of ShotSpotter technology on two measures of police response times, Call-to-Dispatch and Call-to-On-Scene. Using a comprehensive dataset of all Priority 1 911 calls that result in police dispatch over a seven-year period (2016-2022), we find that response times are significantly increased following the implementation of ShotSpotter in Chicago. Specifically, we find that 911 dispatchers exhibit a minute increase in finding an available officer to dispatch (Call-to-Dispatch) and officers subsequently arrive at the scene of the crime approximately two minutes slower (Call-to-On-Scene). These increases have significant implications, as officers exhibit a decrease in the likelihood of arresting perpetrators following a 911 dispatch (9%)—a result driven by calls associated with domestic violence.

[27] Minors are protected under the Freedom of Information Act. Therefore, we could only receive aggregate numbers of juvenile victims. This accounted for approximately 11% of all victims over the course of the sample period.

Furthermore, we find evidence that ShotSpotter increases response times by reallocating scarce police resources from confirmed reports of crime (911 calls) to ShotSpotter-detected gunfire alerts (ShotSpotter dispatches), resulting in a significant time tradeoff. Given the substantial resources that ShotSpotter requires, police officers are forced to allocate a significant portion of their time to fulfill ShotSpotter requirements, thereby incapacitating them from attending to 911 calls. In particular, we show that the effects are driven by times when there are fewer police officers on-duty and times of the day when ShotSpotter dispatches are most frequent. On the intensive margin, we find that each additional ShotSpotter dispatch results in a six-second increase in Call-to-Dispatch time and an eight-second increase in Call-to-On-Scene time, further implying that ShotSpotter is creating a costly time allotment.

Importantly, we do not rule out the possibility that ShotSpotter may be an effective tool for police departments. As a limitation, the data cannot evaluate the productivity of a ShotSpotter dispatch in comparison to a 911 dispatch over the sample period.[28] However, based on a small subset of the data (2019-2022), we find descriptive evidence that approximately 2.2% of all ShotSpotter dispatches result in an arrest.[29] For context, gun-related 911 calls in ShotSpotter districts prior to implementation end in an arrest approximately 3.5% of the time. Despite this discrepancy, we emphasize that an arrest is not the only productivity measure in a dispatch; police may gather valuable intelligence at the crime scene, or the presence of officers may produce a deterrence effect from subsequent crimes occurring in the area (Chalfin and McCrary, 2017). As a result, further research is needed to understand the productivity of ShotSpotter dispatches to perform a rigorous cost-benefit

[28]Two reports from Chicago have raised concerns over ShotSpotter's productivity (Ferguson and Witzburg, 2021; Manes, 2021).

[29]Officers were not required to note whether an arrest was associated to ShotSpotter until after February 2019 according to a Freedom of Information Act request for such information. This number is found using the total number of distinct arrests that are associated with a ShotSpotter and dividing by the number of ShotSpotter dispatches post-February 2019.

analysis.

Hence, we cannot advocate for, nor against ShotSpotter, but only inform policymakers of the substantial unintended consequence it creates. However, given the analysis, we understand that ShotSpotter creates a resource constraint problem where officers have too many responsibilities. Therefore, we recommend that police departments carefully evaluate whether their departments have the staffing required to accommodate the intensive resources that this technology requires in order to mitigate the consequences. In our setting, a back-of-the-envelope calculation shows that in order to eliminate the on-scene time delays, 36% more officers are needed.[30] This underscores the notion that artificial intelligence technology such as ShotSpotter, as of now, can possibly act as a valuable complement for police officers, but not as a perfect substitute.

[30] To calculate this, we estimate the specification in Equation 3, replacing the $NumberSSTDispatches_{dt}$ with the number of officers within district d at time t and the number of officers within district d at time t squared. The marginal effect of an additional officer on response times using this model is to 1.78 seconds increased in on-scene time. We then use the average increase in Call-to-On-Scene from Column 2 of Table 2 (103.7) and divide by the 1.78 to find the number of officers needed to negate this effect. Using the average number of officer hours (1277.86), and dividing by 8 (the average shift time), we find the average number of officers within a district (159.73). Finally, dividing the number of officers needed by the average number of officers within a district gives the percentage increase (36%).

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8 Figures

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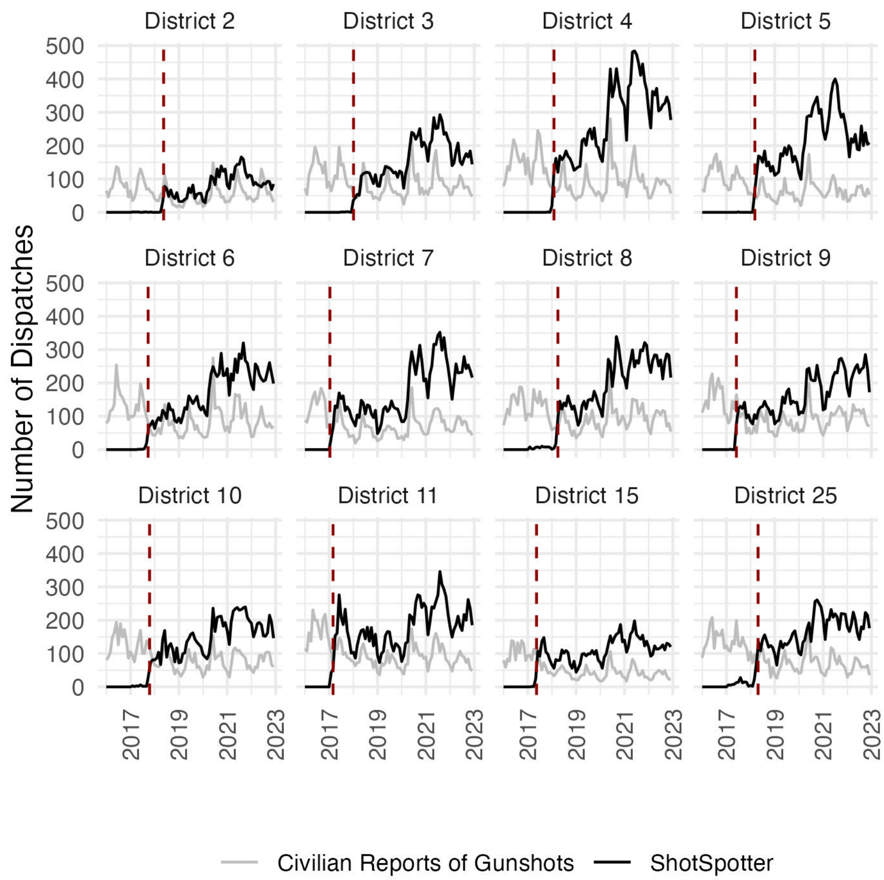

District	Enactment
2	2018-05-16
3	2018-01-04
4	2018-02-01
5	2018-03-07
6	2017-09-24
7	2017-01-13
8	2018-04-01
9	2017-06-01
10	2017-10-16
11	2017-03-01
15	2017-05-13
25	2018-04-24

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 D2. Prior to implementation, some districts may observe some ShotSpotter dispatches if sensors in a neighboring district detect gunshots from afar. However, this is a rare occurrence.

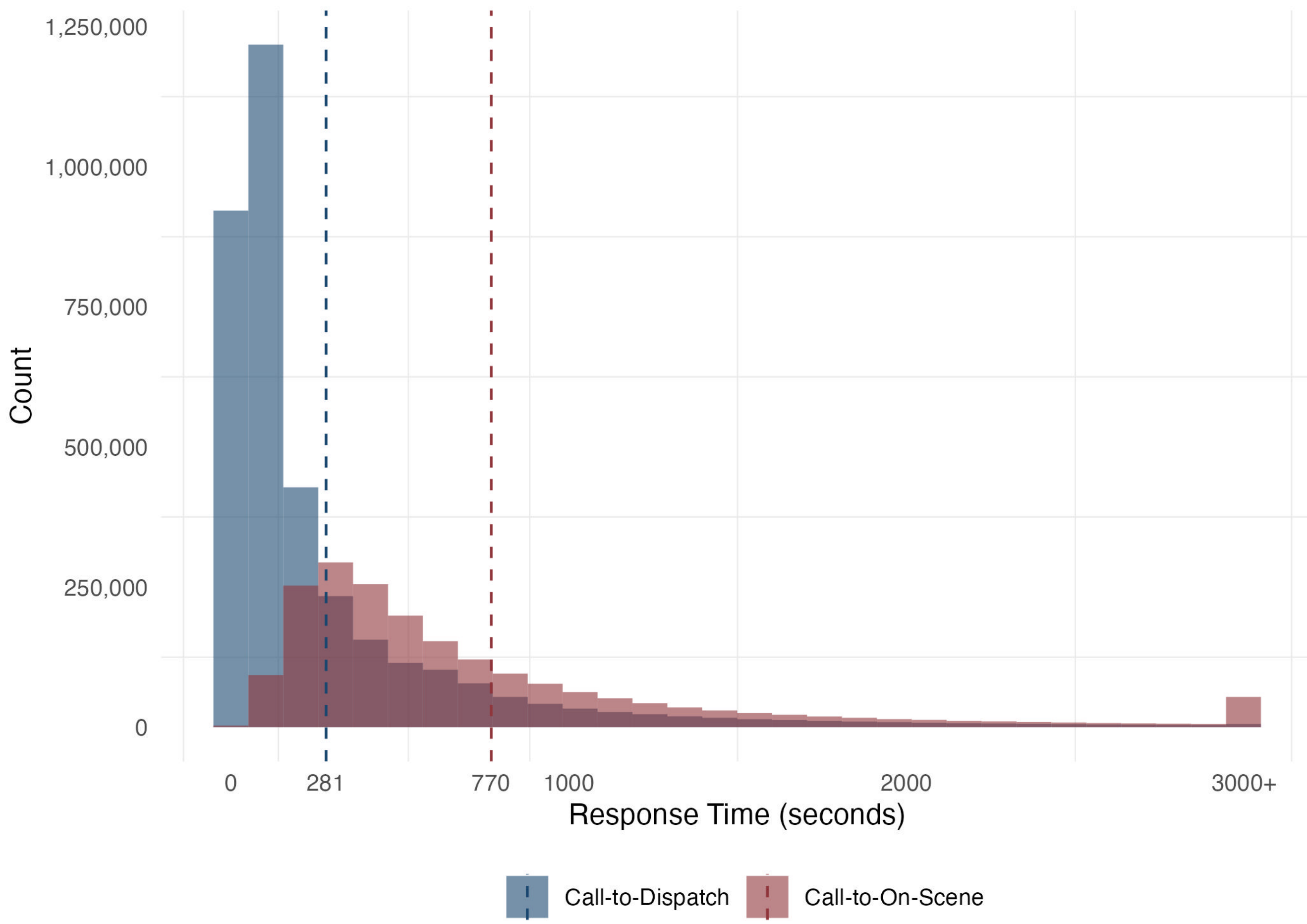

Figure 2: Distribution of Outcome Variables

Note: The two plotted variables are Call-to-Dispatch and Call-to-On-Scene. Call-to-Dispatch is the time from a 911 call to when a police officer is dispatched to the crime scene. 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. Observations with response times higher than 3000 seconds are binned. However, the main results remain consistent when including these outliers, as shown in Appendix Figure D2. The dashed blue line represents the mean of Call-to-Dispatch time, while the dashed red line represents the mean of Call-to-On-Scene time.

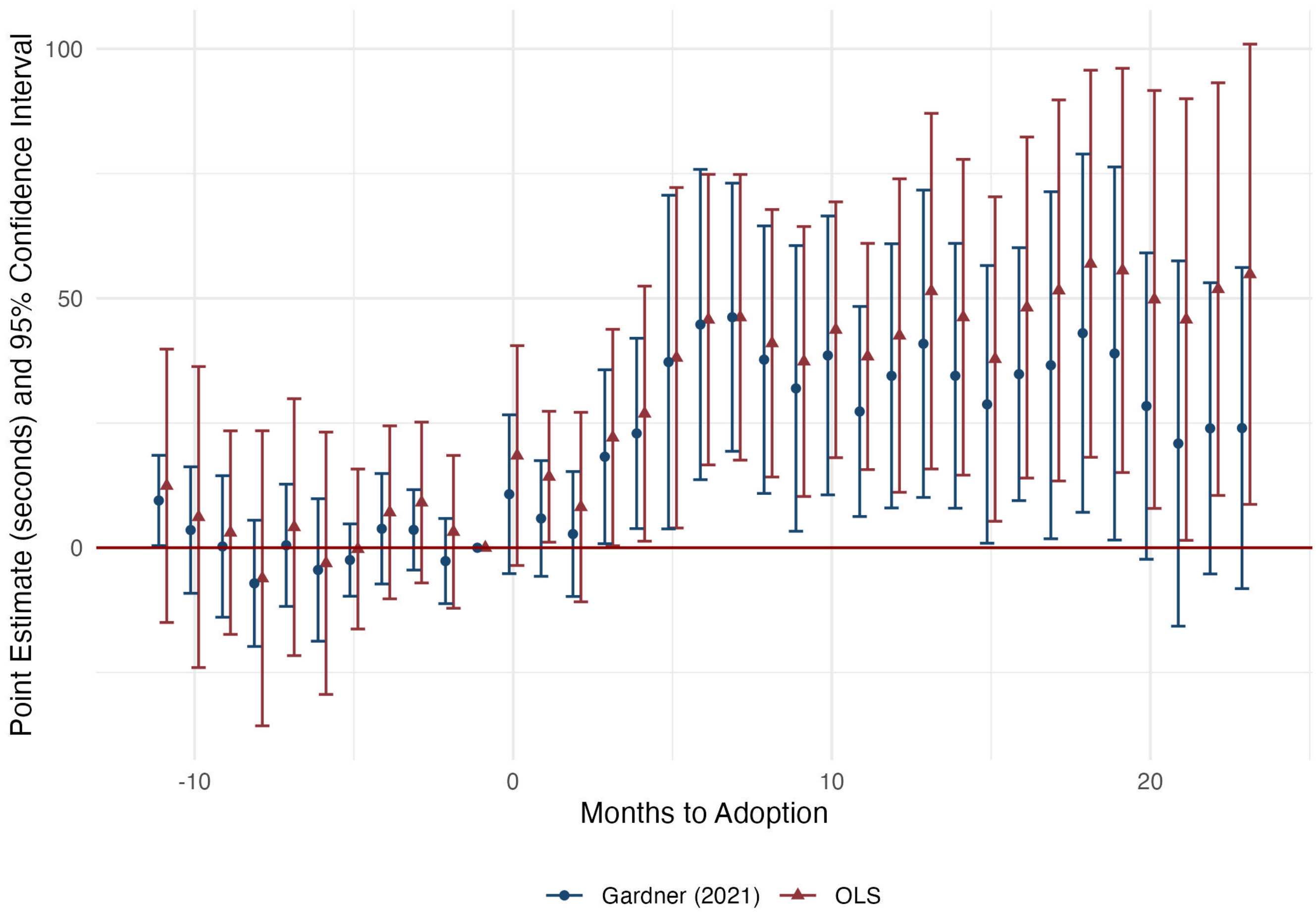

Figure 3: Event Study (Call-to-Dispatch)

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 error-bars/points represent confidence intervals/point estimates from OLS estimation while the blue are using the Gardner (2021) two-stage difference-in-difference estimator, which is robust to heterogeneous treatment effects in staggered adoptions. All pre-/post-periods are relative to the month before ShotSpotter adoption. Twelve pre-periods (24 post-periods) are estimated, but only 11 pre-periods (23 post-periods) are reported, as the -12 (+24) is a binned endpoint. Controls match the preferred specification. Standard errors are clustered at the district level.

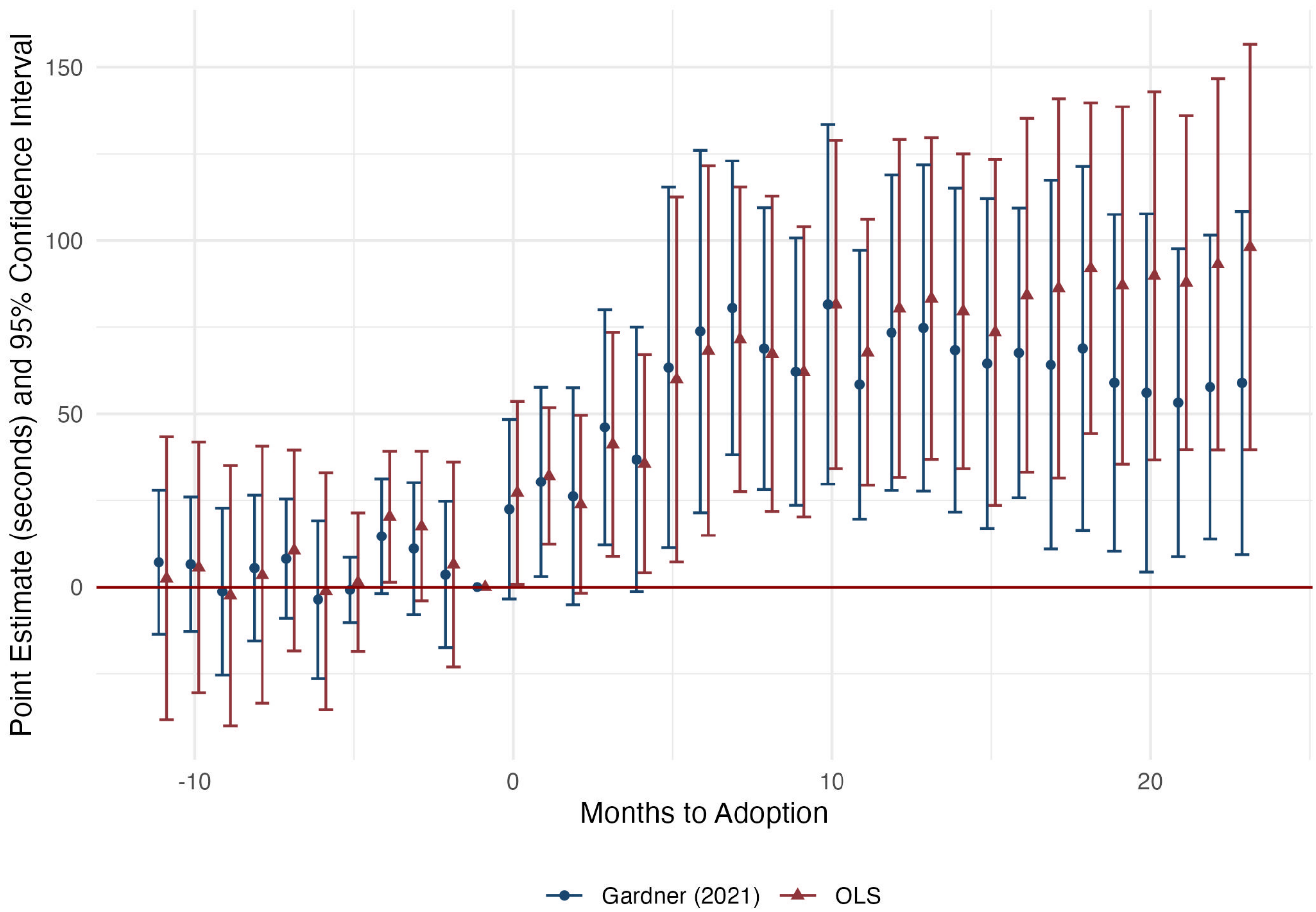

Figure 4: Event Study (Call-to-On-Scene)

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 error-bars/points represent confidence intervals/point estimates from OLS estimation while the blue are using the Gardner (2021) two-stage difference-in-difference estimator, which is robust to heterogeneous treatment effects in staggered adoptions. All pre-/ post-periods are normalized by the month before ShotSpotter adoption. Twelve pre-periods (24 post-periods) are estimated, but only 11 pre-periods (23 post-periods) are reported, as the -12 (+24) is a binned endpoint. Controls match the preferred specification. Standard errors are clustered at the district level.

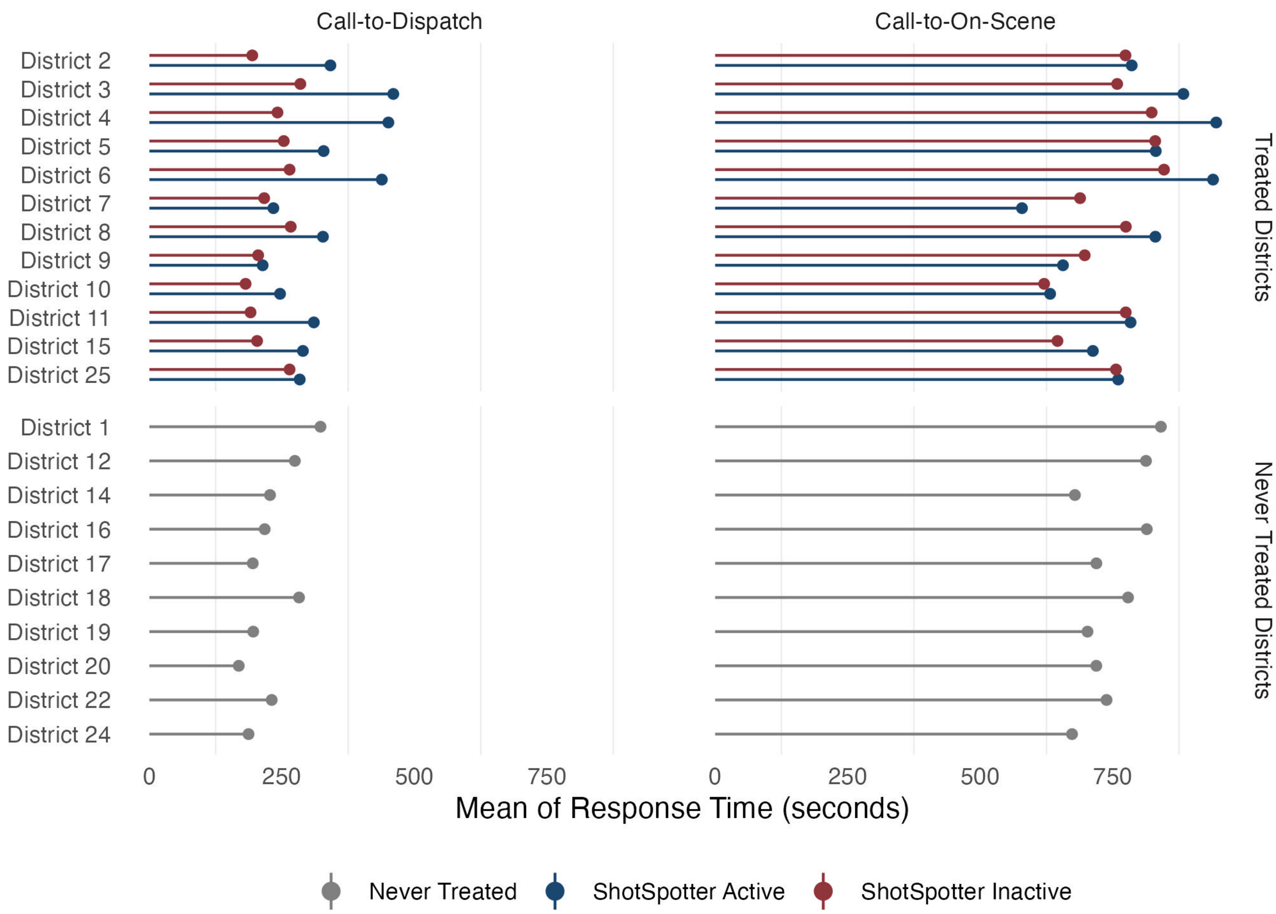

Figure 5: Average Outcomes in Police Districts

Note: Each police district is plotted on the y-axis, and the average of Call-to-Dispatch and Call-to-On-Scene (seconds) is on the x-axis. In the top panel, police districts that receive ShotSpotter technology are plotted. In the bottom panel, police districts that never receive ShotSpotter are plotted. All ShotSpotter-implemented districts have two distinctions: ShotSpotter Active and ShotSpotter Inactive. The red lines correspond to periods prior to ShotSpotter implementation, and the blue bars correspond to post-implementation. There are 12 of 22 police districts in Chicago that receive ShotSpotter technology.

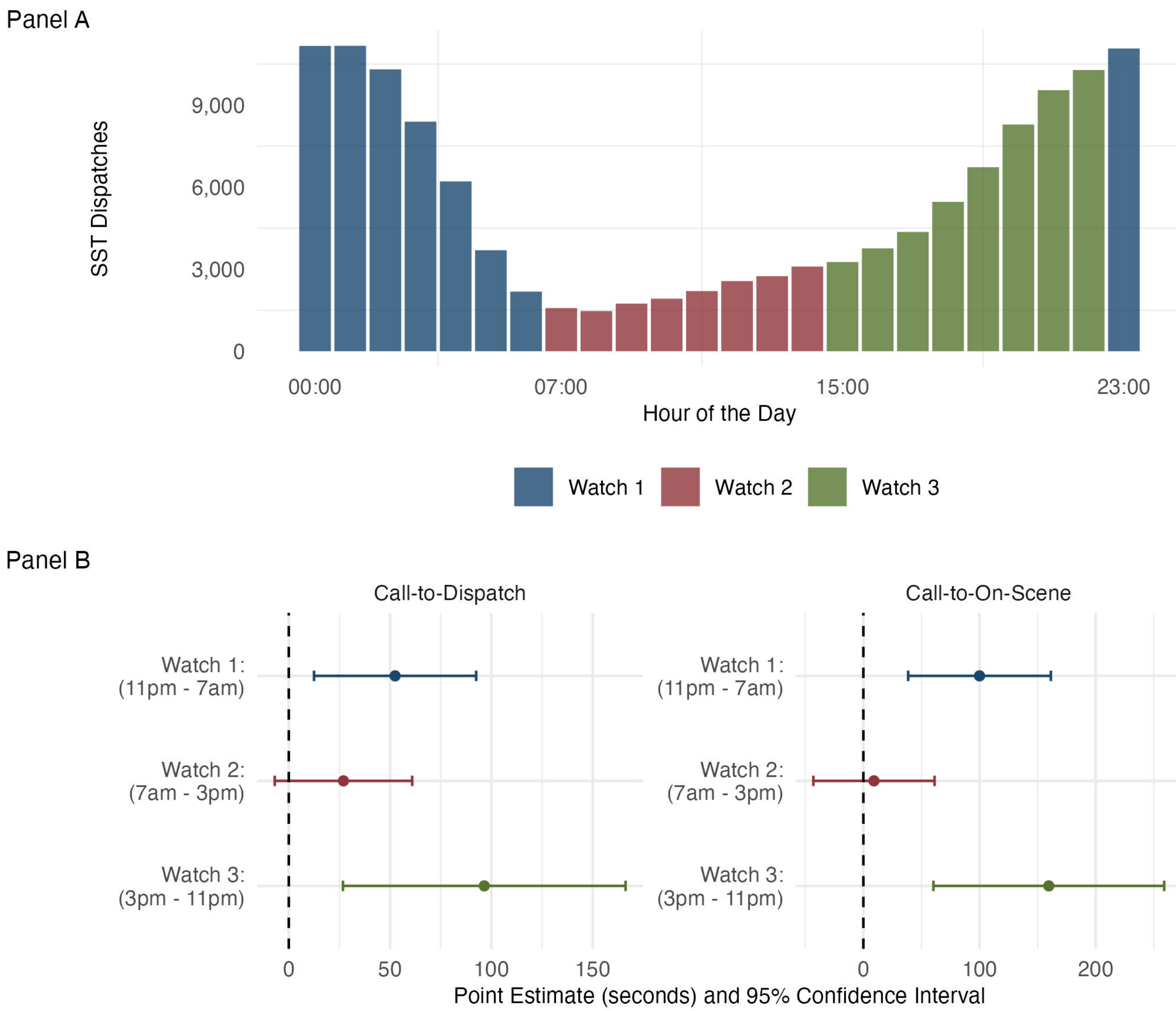

Figure 6: Effect of ShotSpotter by Officer Watch Times

Note: This figure shows that in times when officers are responding to more ShotSpotter (SST) detections, their response times are slower. In Panel A, the number of ShotSpotter dispatches are plotted by the hour of occurrence. The y-axis is the number of ShotSpotter dispatches, 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 (11:00pm-7:00-am), Watch 2 (7:00am-3:00pm), and Watch 3 (3:00pm-11:00pm).

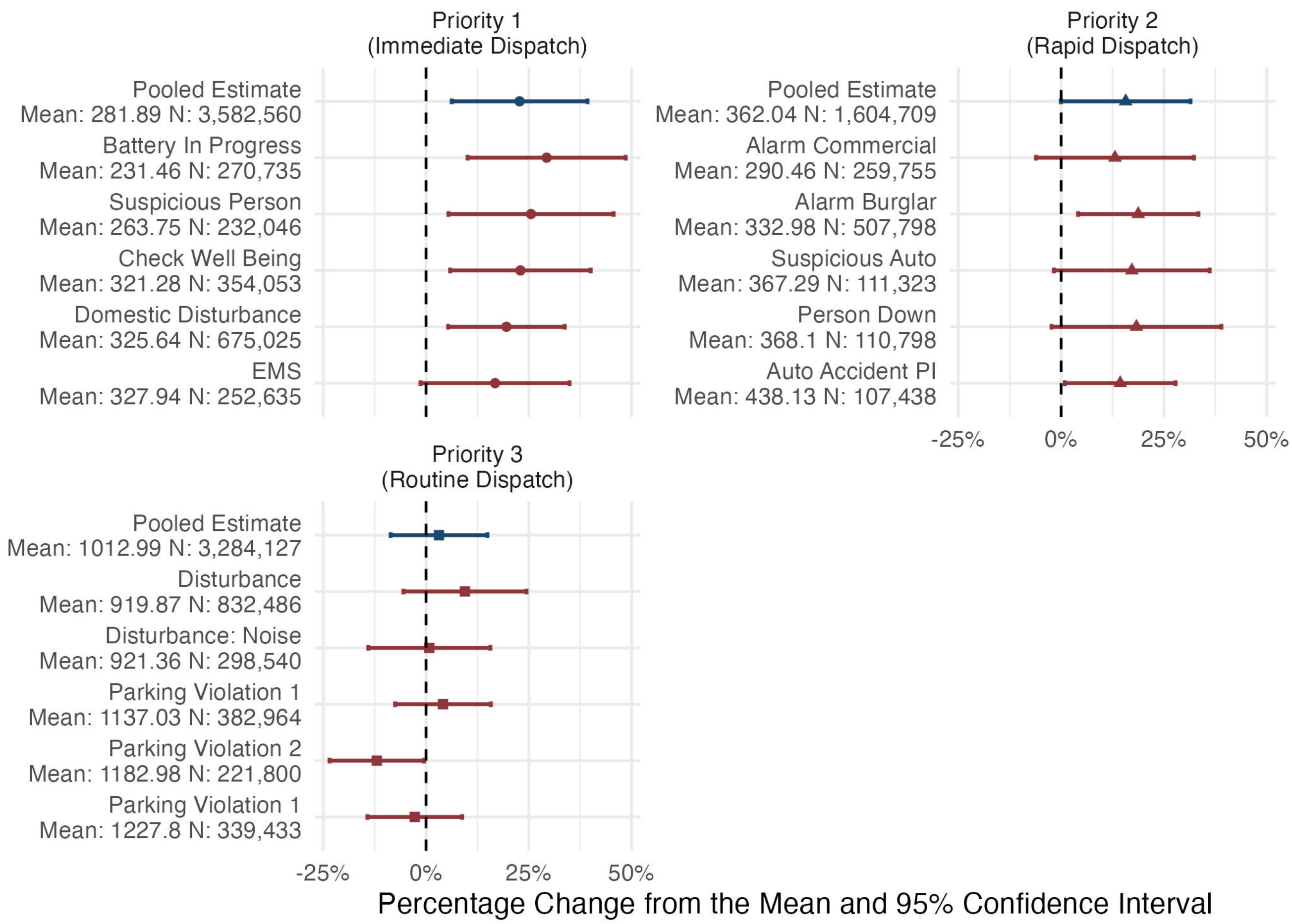

Figure 7: Effect of ShotSpotter by Priority (Call-to-Dispatch)

Note: This figure plots the effects of ShotSpotter on Call-to-Dispatch times by priority and by most frequent call-type. In the first row of each panel, the pooled estimate combining all respective call types is reported. The subsequent rows report estimates for the most frequent call-types, ranked by their average Call-to-Dispatch time. For instance, in Priority 1, Battery in Progress has the lowest average Call-to-Dispatch time, while Suspicious Person has the second lowest. The x-axis shows the percent change from the mean (i.e., the point estimate divided by the mean of the outcome), as well as the corresponding 95% confidence interval using the specification from Equation 1. The number of observations and means are shown in the y-axis for each call-type. All estimations are estimated using OLS and the preferred specification.

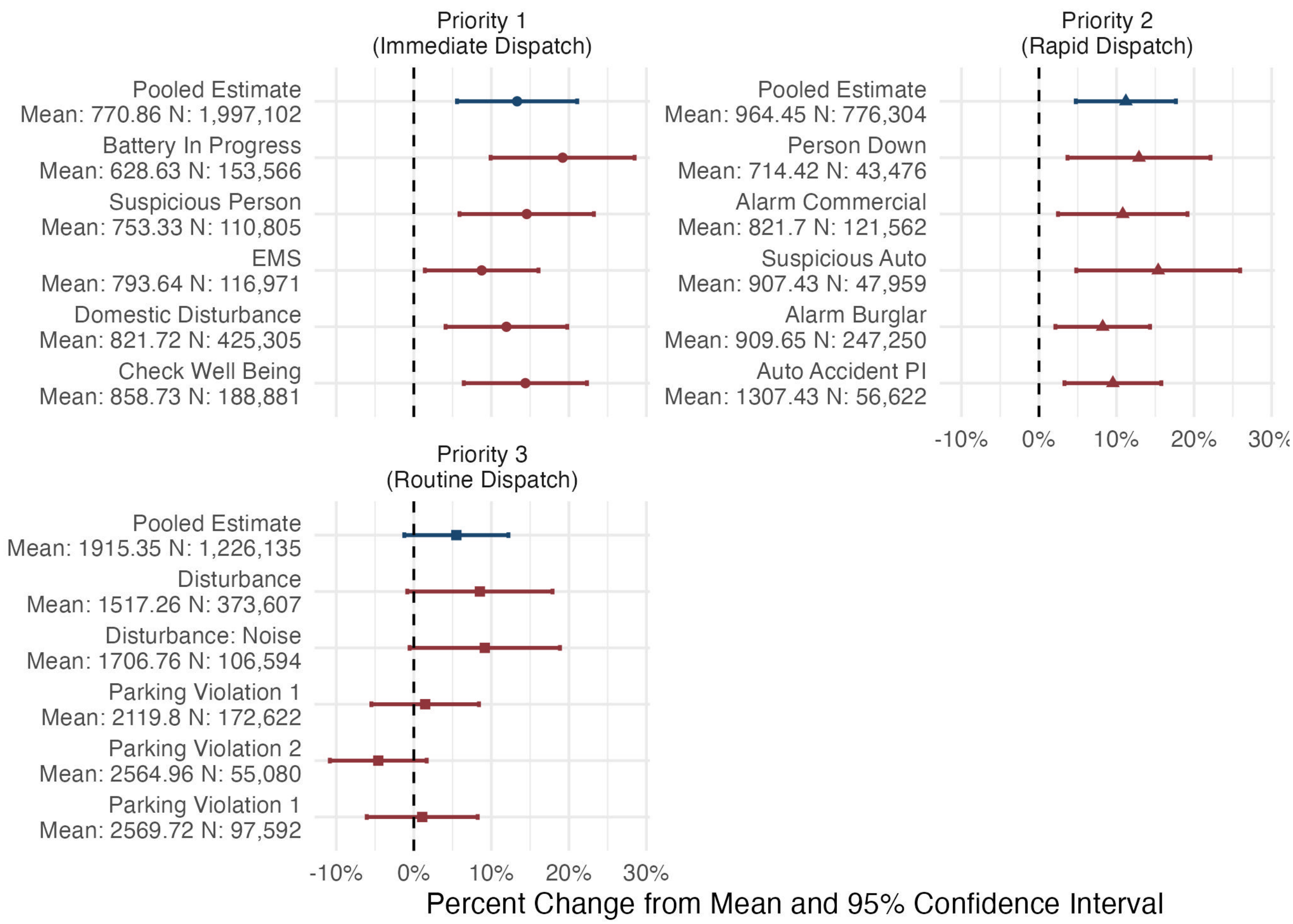

Figure 8: Effect of ShotSpotter by Priority (Call-to-On-Scene)

Note: This figure plots the effects of ShotSpotter on Call-to-On-Scene times by priority. In the first row of each panel, the pooled estimate combining all respective call types is reported. The subsequent rows report estimates for the most frequent call-types, ranked by their average Call-to-On-Scene time. For instance, in Priority 1, Battery in Progress has the lowest average Call-to-On-Scene time, while Suspicious Person has the second lowest. The x-axis shows the percent change from the mean (i.e., the point estimate divided by the mean of the outcome), as well as the corresponding 95% confidence interval using the specification from Equation 1. The number of observations and means are shown in the y-axis for each call-type. All estimations are estimated using OLS and the preferred specification.

9 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	0.03	0.17	0.00	1.00	3,582,560
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:					
Priority 1 911 Dispatches	73.01	24.63	8.00	223.00	3,582,560
ShotSpotter Dispatches	2.96	4.19	0.00	57.00	3,582,560
Officer Hours	1,342.21	395.08	231.00	6,558.10	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. Priority 1 Call-to-On-Scene is missing approximately 45 percent of on-scene times. This is discussed further in Appendix A. Arrest Made is and indicator equal to one if the 911 dispatch resulted in an arrest. Victim Injury is an indicator equal to one if the 911 dispatch resulted in a victim injury. Priority 1 refers to an immediate dispatch, Priority 2 a rapid dispatch, and Priority 3 a routine dispatch. Priority 1 911 Dispatches is the number of Priority 1 dispatches at the district-day level. ShotSpotter Dispatches is the number of dispatches due to ShotSpotter detections. Importantly, ShotSpotter Dispatches is also at the district-by-day level and includes days in which ShotSpotter is not implemented. The average number of ShotSpotter dispatches on post-implementation days is approximately 6. The average daily number of ShotSpotter dispatches across Chicago once all 12 districts have implemented ShotSpotter is approximately 70. Note that New Years Eve/New Years Day/Fourth of July are excluded from the sample as these days correspond with high amounts of celebratory gunfire. Officer Hours are the number of working hours sworn police officers work at the district-day level.

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***	65.659***	71.929***	61.373***
	(21.541)	(22.394)	(21.888)	(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***	105.146***	120.721***	101.392***
	(26.205)	(28.801)	(28.269)	(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 (2021) Robust				X	

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by district. All coefficient estimates are in seconds. 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 only time and group fixed effects. Column 2 reports the preferred specification from Equation 1, which includes hour-of-day and call-type fixed effects. Column 3 includes number of Priority 1 dispatches and Officer Hours as controls. However, considering these may be correlated with treatment, we do not consider this the preferred specification. Column 4 reports estimates using the Gardner (2021) estimator which is robust to heterogeneous treatment effects across groups and time periods in staggered designs. Due to its two-stage method, some observations are dropped if unable to predict values in the first stage. Column 5 includes Border District Activated which is an indicator for when a district is adjacent to a ShotSpotter implemented district. Wild cluster bootstrap p-values using 999 iterations are also reported as the number of clusters (22) is below the threshold of 30 put forth in Cameron et al. (2008). The bootstrap cannot be performed using the Gardner (2021) estimator.

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

	ShotSpotter Rollout			ShotSpotter Dispatches
		Officer Availability		
	Pooled	> Median	<= Median	Pooled
	(1)	(2)	(3)	(4)
Panel A: Call-to-Dispatch				
ShotSpotter Activated	64.131***	34.500**	85.180***	
	(22.379)	(13.630)	(27.959)	
Number SST Dispatches				6.094***
				(1.513)
Mean of Dependent Variable	281.890	239.951	323.077	269.365
Observations	3,582,560	1,775,086	1,807,474	47,933
Panel B: Call-to-On-Scene				
ShotSpotter Activated	102.682***	59.706***	138.102***	
	(28.724)	(21.061)	(37.671)	
Number SST Dispatches				8.023***
				(1.842)
Mean of Dependent Variable	770.863	711.409	827.843	770.462
Observations	1,997,102	977,332	1,019,770	47,932
FE: Day-by-Month-by-Year	X	X	X	X
FE: District	X	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. ShotSpotter Activated is a binary equal to one when a district has ShotSpotter technology (extensive margin). Number SST Dispatches refers to the number of ShotSpotter dispatches that occur within a district-day (intensive margin). All coefficient estimates are in seconds. Panel A reports results for Call-to-Dispatch while Panel B reports results for Call-to-On-Scene. Officer availability is measured by number of officer hours within a district-day. Column 2 corresponds to district-days that have officer hours above their district median (more officer availability), while Column 3 corresponds to district-days that have officer hours below their district median (less officer availability). Analyses for Columns 1-3 are on the extensive margin, and utilze call-level data. The coefficients for these analyses are interpreted as average effects. Analysis for Column 4 is on the intensive margin, and the data is aggregated to the district-day level. The coefficients of interest for Column 4 are interpreted as marginal effects. We aggregate to the district-day since the number of ShotSpotter dispatches is measured at the district-day. Because of this, we cannot use call-level data to correctly identify the marginal effects. Moreover, we restrict the sample to only post-implementation days for treated districts to ensure that only the intensive margin, rather than extensive margin, is identified. Further explanation of this model is given in Section 5.3.

Table 4: Effect of ShotSpotter Enactment on 911 Arrest Likelihood (OLS)

		Gun-Relation		Most Frequent Arrest 911 Calls		
	All	Gun	Non-Gun	Domestic Disturbance	Domestic Battery	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
ShotSpotter Activated	-0.221***	-0.157	-0.221***	-0.829***	-0.281**	-0.303
	(0.063)	(0.189)	(0.066)	(0.241)	(0.123)	(0.177)
Mean of Dependent Variable	2.449	3.355	2.361	6.110	2.021	4.185
Observations	3,582,560	317,937	3,264,623	224,022	675,025	270,735
Wild Bootstrap P-Value	0.001	0.412	0.003	0.003	0.049	0.109
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. All coefficient estimates are in percentages. The dependent variable is an indicator equal to one if a 911 call ended in an arrest. Column 1 reports the pooled estimates using the entire sample. Columns 2 and 3 subset Column 1 by gun-related and non-gun-related 911 calls. Gun-related crimes are those corresponding to the following 911 code descriptions: ‘person with a gun’, ‘shots fired’, or ‘person shot’. Columns 4-6 report the three most frequent 911 calls that end in arrest: Domestic Disturbance, Domestic Battery, and Robbery. Wild cluster bootstrap p-values using 999 replications are also reported since the number of clusters (22) is below the threshold of 30 put forth in Cameron et al. (2008).

Table 5: Effect of ShotSpotter Implementation on Likelihood of 911 Victim Injury (OLS)

	Likelihood of Victim Injury		
	Pooled	Gun Dispatch	Non-Gun Dispatch
	(1)	(2)	(3)
ShotSpotter Activated	-0.062	-0.422*	-0.007
	(0.051)	(0.211)	(0.054)
Mean of Dependent Variable	2.990	4.185	2.874
Observations	3,582,560	317,937	3,264,623
Wild Cluster Bootstrap P-Value	0.245	0.067	0.895
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. All coefficient estimates are in percentages. The main variable is the probability of a victim being injured during a 911 call dispatch. The Pooled column reports estimates using the entire sample of Priority 1 dispatches. Gun Dispatch (Column 2) is restricted to only gun-related 911 call dispatches which have the following 911 code descriptions: ‘person with a gun’, ‘shots fired’, or ‘person shot’. Non-Gun Dispatch (Column 3) are all other 911 call dispatches that are not related to gun descriptions. In all columns the preferred specification is estimated using OLS. Wild cluster bootstrap p-values using 999 replications are also reported since the number of clusters (22) is below the threshold of 30 put forth in Cameron et al. (2008).

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. 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 relaying 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 biased downward.

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).[31] 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. This result instills confidence that officers are likely still arriving on-scene at later times even in missing data cases, as there appears to be no change in Call-to-Dispatch times when on-scene times are missing.

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

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

Call-to-On-Scene findings.

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

		Officer Availability	
	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. All coefficient estimates are in seconds. 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, which contains no missing data, is estimated with an additional interaction term which interacts Call-to-Dispatch time with the indicator for whether on-scene time is missing. The coefficient estimate on this term shows that 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.

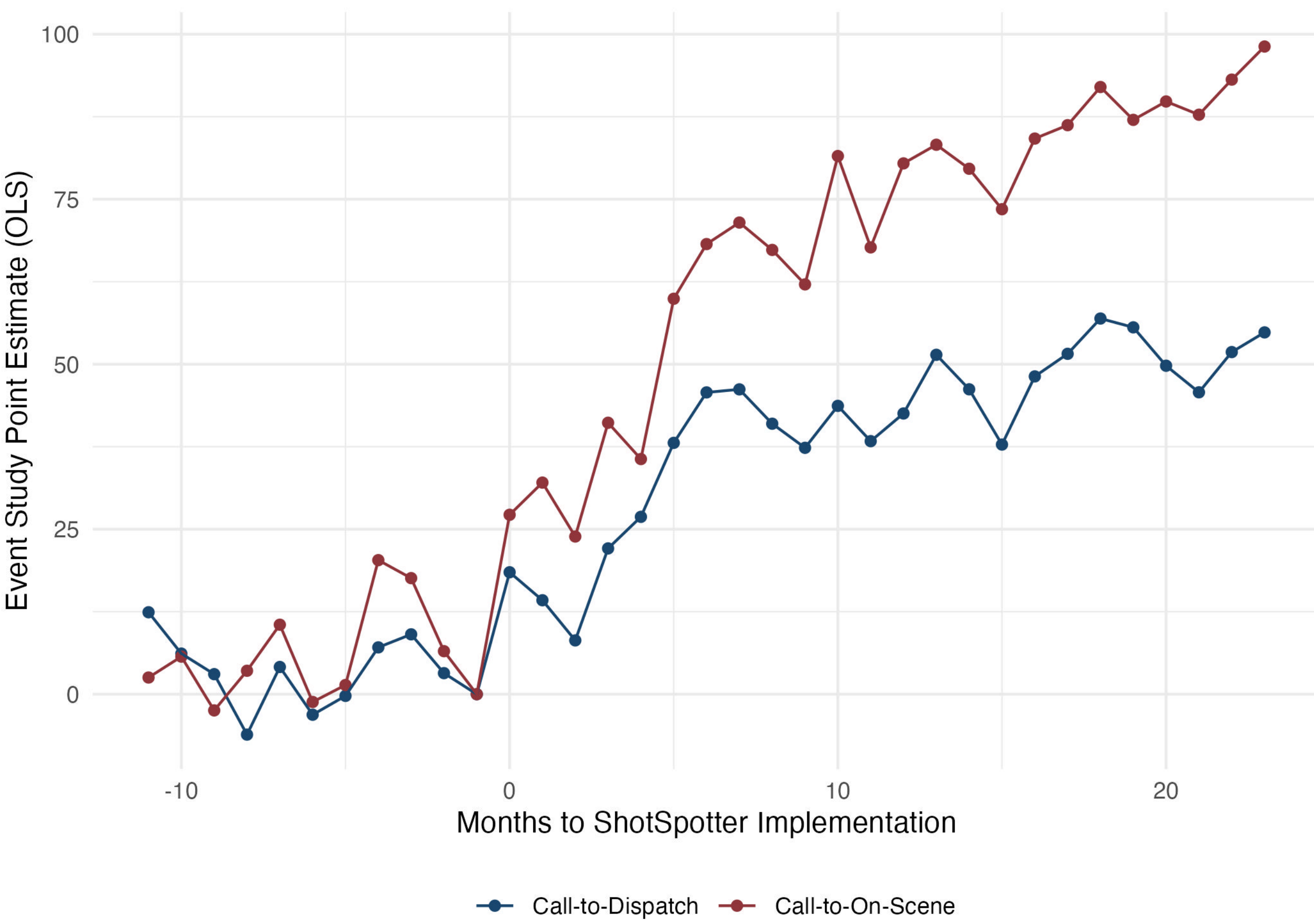

Figure A1: Event Study Point Estimates Trend

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.

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.

B.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).[32] While most of these technologies have already been in utilization by the CPD prior to SDSCs,[33] the Hunchlab software is implemented at the exact timing of an SDSC.

Importantly, as described in further detail in Kapustin et al. (2022), the implementation of an SDSC did not include an infusion of officers in the form of new officers

[32]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.

[33]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.

being hired, existing officers being relocated, or officers working extra hours. Moreover, SDSCs were told not to implement new policing strategies, but to only assist department members with crime forecasting.

B.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 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 Kapustin et al. (2022). Specifically, they find that Hunchlab causes significant changes in police patrolling behavior for only two police districts (District 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.

B.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 ShotSpotter's implementation. Appendix Table B1 reports the districts and corresponding dates of their implementation. On average, SDSCs are implemented 76 days prior to ShotSpotter, although not every district with an SDSC receives 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 (2021) 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 increases of two-minutes—each statistically significant at the 1% level. On the other hand, there appears to be a decrease in response times due to the SDSC roll-out on both Call-to-Dispatch and Call-to-On-Scene times, suggesting that the Hunchlab technology in the SDSCs is not incapacitating officers' availability, and that the SDSCs may provide some efficiency gains with the reorganization of intelligence software.

In Columns 3 and 4 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 (Kapustin et al., 2022). 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 ShotSpotter.

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 (2021) 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.

B.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 officer's 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
1		2020-06-01	2017-03-10		
12		2018-03-01	2017-12-04		
14		2019-02-25	2016-06-01		
16			2017-11-20		
17		2019-02-25	2017-11-27		
18		2018-08-01	2017-03-31		
19		2019-02-01	2017-10-30		
20		2019-02-25	2017-10-23		
22		2019-02-25	2017-10-30		
24		2019-02-01	2017-10-16		

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 in districts that have both ShotSpotter and an SDSC. SDSCs contain many police prediction softwares, however, only Hunchlab, a location prediction software, is implemented in conjuction with these as the others had been previously used in Chicago prior to SDSCs. Hunchlab has been found to only change patrolling 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 (OLS)

	SDSC Controls				BWC Controls	
			Omitting Districts 7 and 9			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Call-to-Dispatch						
ShotSpotter Activated	75.429***	71.817***	84.736***	90.334***	61.256***	71.856***
	(25.028)	(22.497)	(26.894)	(22.057)	(20.988)	(22.523)
SDSC Activated	-36.742**		-48.221**			
	(16.585)		(16.930)			
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	0.006		0.004		0.010	
Panel B: Call-to-On-Scene						
ShotSpotter Activated	120.530***	120.080***	127.822***	145.931***	98.403***	120.214***
	(30.436)	(28.141)	(32.875)	(24.339)	(27.843)	(28.246)
SDSC Activated	-60.324***		-71.208***			
	(18.978)		(20.381)			
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	0.002		0.001		0.002	
Gardner (2021) Robust		X		X		X

Note:
* p < 0.1, ** p < 0.05, *** p < 0.01
Standard errors are clustered by district. Coefficient estimates are in seconds. Columns 1 and 2 of Panel A show Call-to-Dispatch estimates when controlling for the implementation of Strategic Decision Support Centers (SDSC). 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 (2021) robust estimates are reported in even columns. The coefficient estimates of controls when using Gardner (2021) estimator are not reported as the two-stage method only returns the coefficient estimate of interest on the treated variable. In addition, the two-stage procedure may drop observations in the first stage if unable to predict values. This happens infrequently as shown in the observation counts, but is worth noting. Finally, wild cluster bootstrap p-values using 999 iterations are also reported as the number of clusters (22) is below the threshold of 30 put forth in Cameron et al. (2008). The bootstrap procedure cannot be performed using the Gardner (2021) estimator.

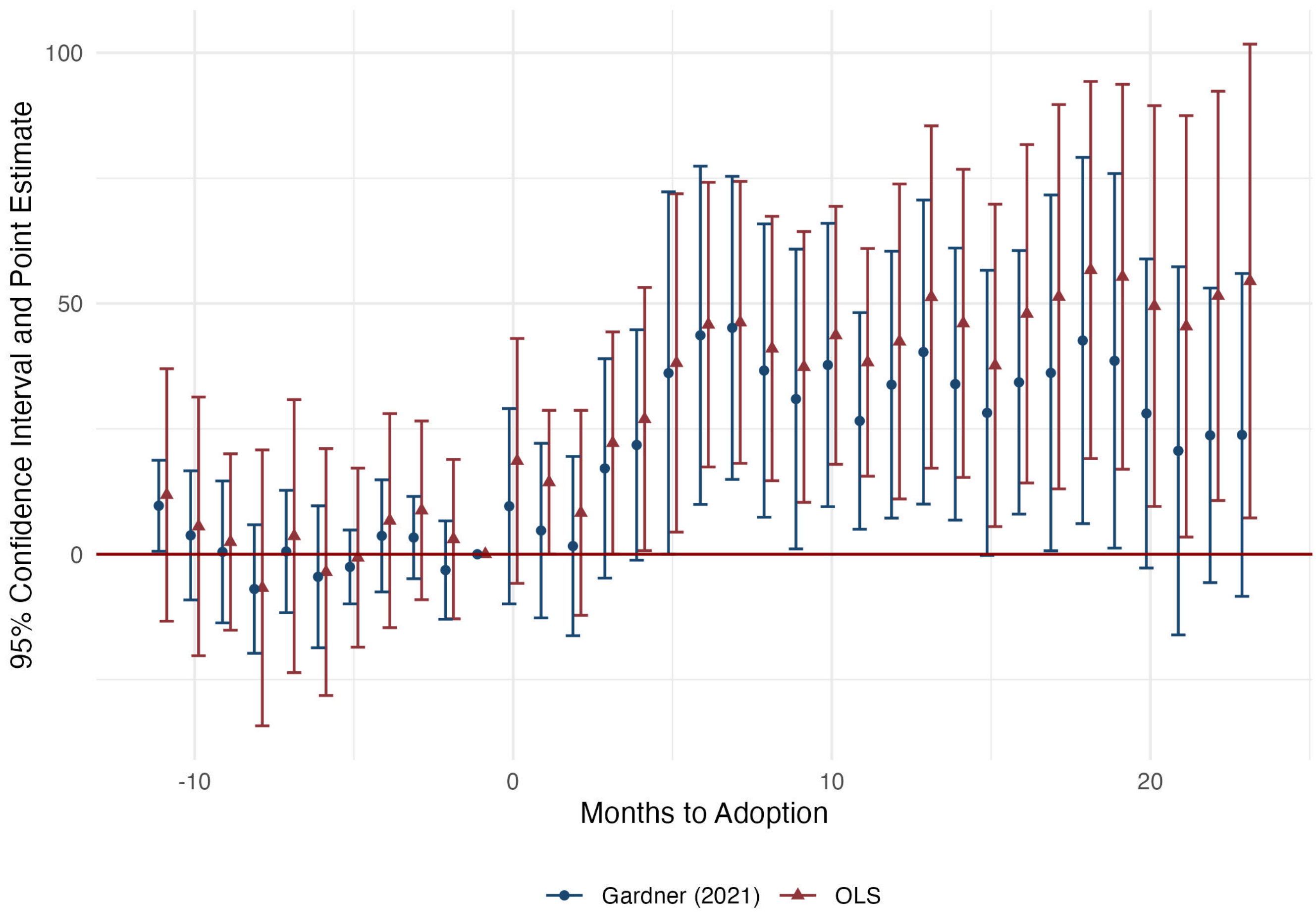

Figure B1: Event Study w/ SDSC Controls (Call-to-Dispatch)

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 error-bars/points represent confidence intervals/point estimates from OLS estimation, while the blue are from Gardner (2021) 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 match the preferred specification in addition to SDSC rollout. Standard errors are clustered at the district level.

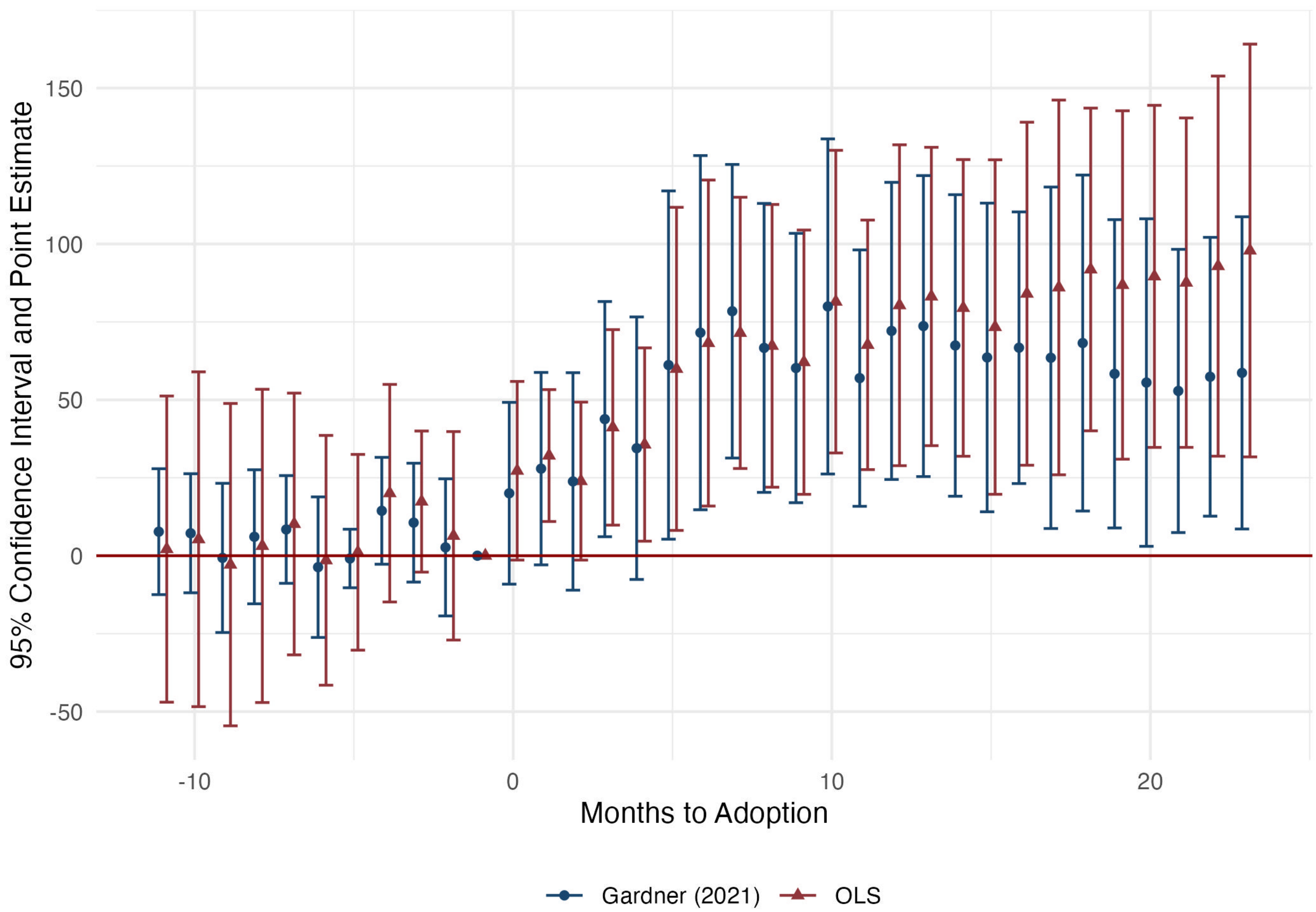

Figure B2: Event Study w/ SDSC Controls (Call-to-On-Scene)

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 error-bars/points represent confidence intervals/point estimates from OLS estimation, while the blue are from Gardner (2021) 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 match the preferred specification in addition to SDSC rollout. Standard errors are clustered at the district level.

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.

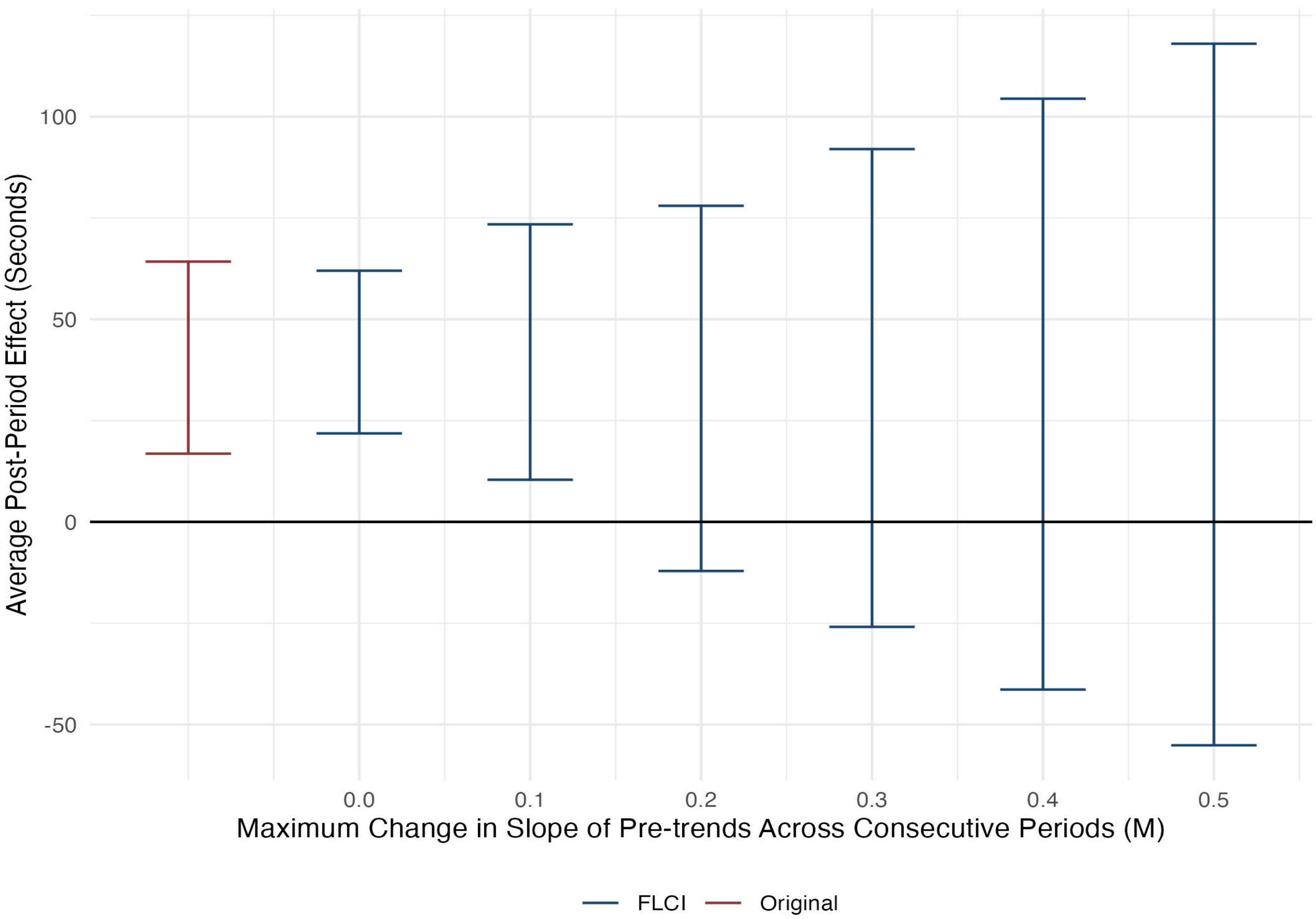

Figure C1: Sensitivity Analysis of Pre-Trends

Note: This figure shows sensitivity analysis of the event study plot in Figure 3. 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 (2023) 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.

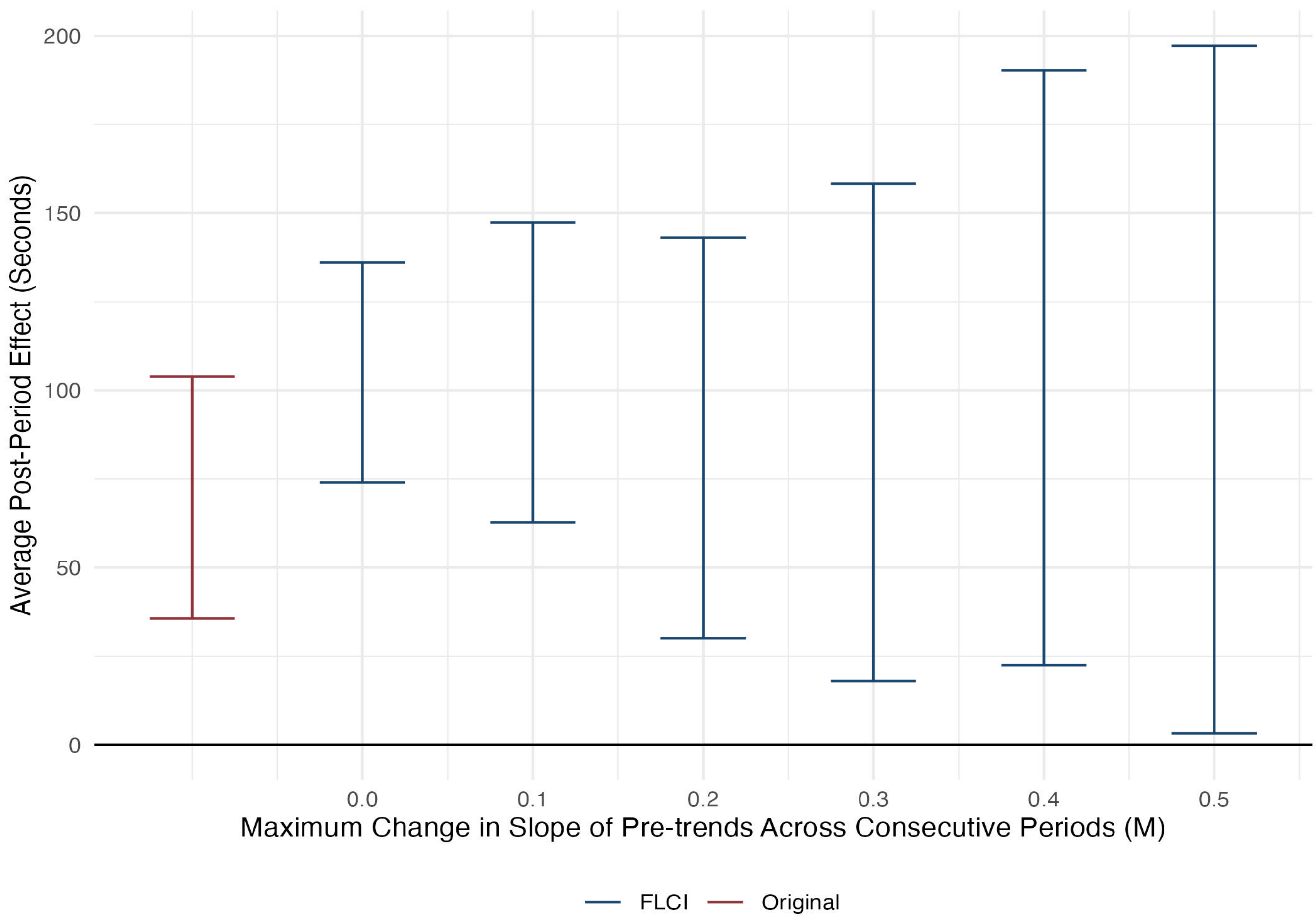

Figure C2: Sensitivity Analysis of Pre-Trends (Call-to-On-Scene)

Note: This figure shows sensitivity analysis of the event study plot in Figure 4. 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 (2023) 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.

Appendix D Supplemental Figures and Tables

Table D1: Effect of ShotSpotter Implementation on Confounding Controls (OLS)

	(1)	(2)
Panel A: Number 911 Dispatches		
ShotSpotter Activated	-3.378	-3.521
	(2.208)	(2.518)
Mean of Dependent Variable	151.864	151.864
Observations	55,792	55,792
Panel B: Officer Availability		
ShotSpotter Activated	-23.949	-42.806*
	(22.709)	(25.534)
Mean of Dependent Variable	1,277.860	1,277.860
Observations	55,792	55,792
FE: Day-by-Month-by-Year	X	X
FE: District	X	X
Gardner (2021) Robust		X

Note:
* p < 0.1, ** p < 0.05, *** p < 0.01
Standard errors are clustered by district. Coefficient estimates are reported in seconds. This table shows estimations on two outcome variables, Number of 911 Dispatches and Officer Availability, which are not included in the main specification due to the possibility of being confounding controls. Each panel refers to a distinct outcome variable. Since each outcome variable is at the district-day level, we aggregate the call-level data to the district-day. Hence, in these models, we cannot control for call-type nor hour of the day. Number 911 Dispatches is the number of 911 dispatches. Officer Availability is the number of police officer hours within a district. ShotSpotter Activated refers to the timing in which each district receives ShotSpotter technology. The Gardner (2021) estimator is robust to the heterogeneous treatment effects in staggered two-way-fixed-effects designs. January 1, July 4, and December 31 are omitted due to their correspondance with potential celebratory gunfire.

Table D2: Effect of ShotSpotter Enactment on 911 Arrest Probability (Logit)

		Gun-Relation		Most Frequent Arrest 911 Calls		
	All	Gun	Non-Gun	Domestic Disturbance	Domestic Battery	Robbery
	(1)	(2)	(3)	(4)	(5)	(6)
ShotSpotter Activated	-0.085***	-0.041	-0.092***	-0.144***	-0.130**	-0.077*
	(0.022)	(0.060)	(0.024)	(0.040)	(0.055)	(0.042)
Mean of Dependent Variable	0.025	0.034	0.024	0.062	0.020	0.042
Observations	3,523,729	312,283	3,205,792	220,976	668,286	266,890
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. All estimations are using logit estimation. The dependent variable is an indicator equal to one if a 911 call ended in an arrest. Column 1 reports the pooled estimates using the entire sample. Columns 2 and 3 subset Column 1 by gun-related and non-gun-related 911 calls. Gun-related crimes are those corresponding to the following 911 code descriptions: 'person with a gun', 'shots fired', or 'person shot'. Columns 4-6 report the three most frequent 911 calls that end in arrest: Domestic Disturbance, Domestic Battery, and Robbery. In some cases, some observations may be dropped due to no variation with certain fixed effects.

Table D3: Effect of ShotSpotter Implementation on Probablity of 911 Victim Injury (Logit)

	Probability of Victim Injury		
	Pooled	Gun Dispatch	Non-Gun Dispatch
	(1)	(2)	(3)
ShotSpotter Activated	-0.039**	-0.115**	-0.025
	(0.020)	(0.057)	(0.020)
Mean of Dependent Variable	0.030	0.042	0.029
Observations	3,520,402	314,375	3,202,465
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 Pooled column refers to using the entire sample of time-sensitive Priority 1 dispatches. Gun Dispatch is restricted to only gun-related dispatches including 'Person with a Gun', 'Person Shot', and 'Shots Fired'. Non-Gun Dispatch are all other dispatches. In all columns the preferred specification is estimated using logistic regressions. In some cases, some observations may be dropped due to no variation with certain fixed effects.

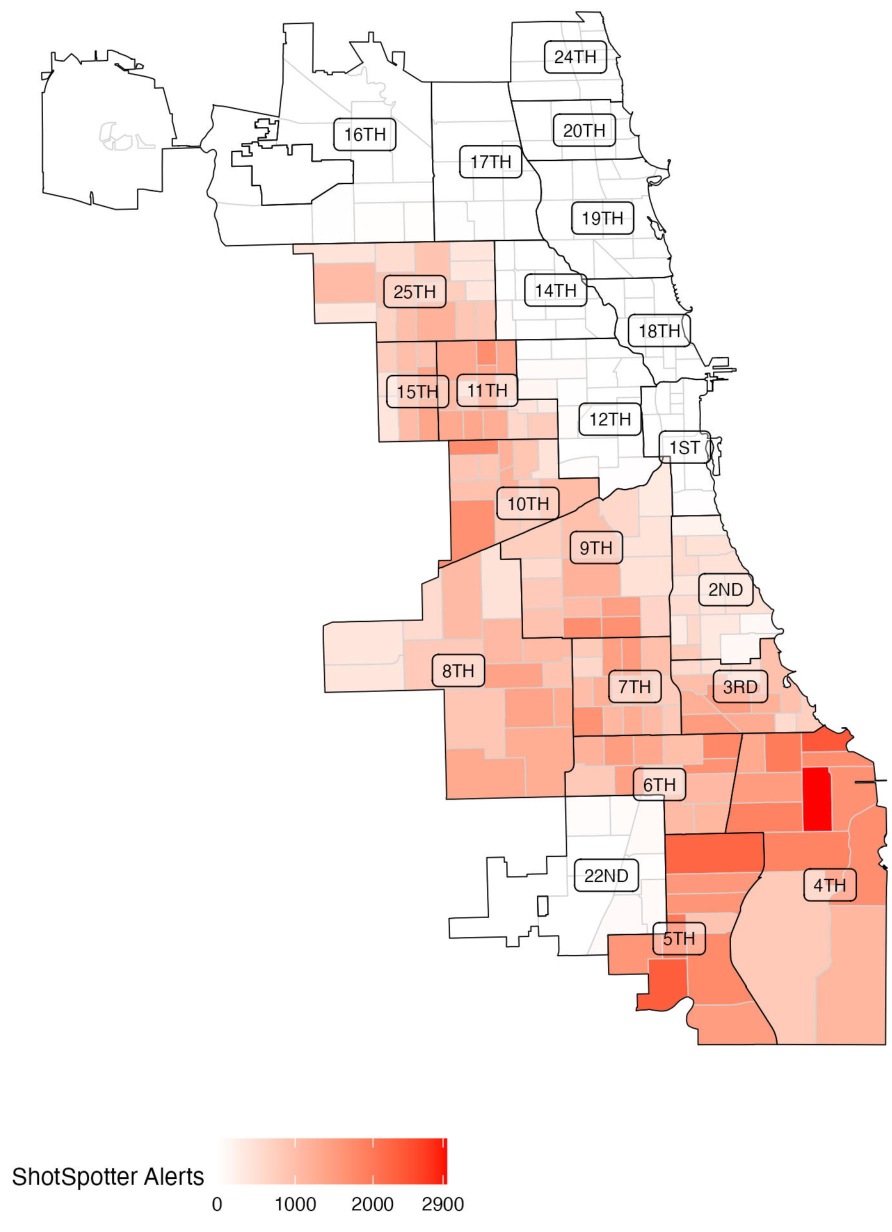

Figure D1: Map of ShotSpotter Districts in Chicago

Note: There are 22 police districts in Chicago, and 12 are equipped with ShotSpotter technology. Each district contains beats which are designated by the boxes within the district lines. ShotSpotter implementation began in January 2017 and ended in May 2018.

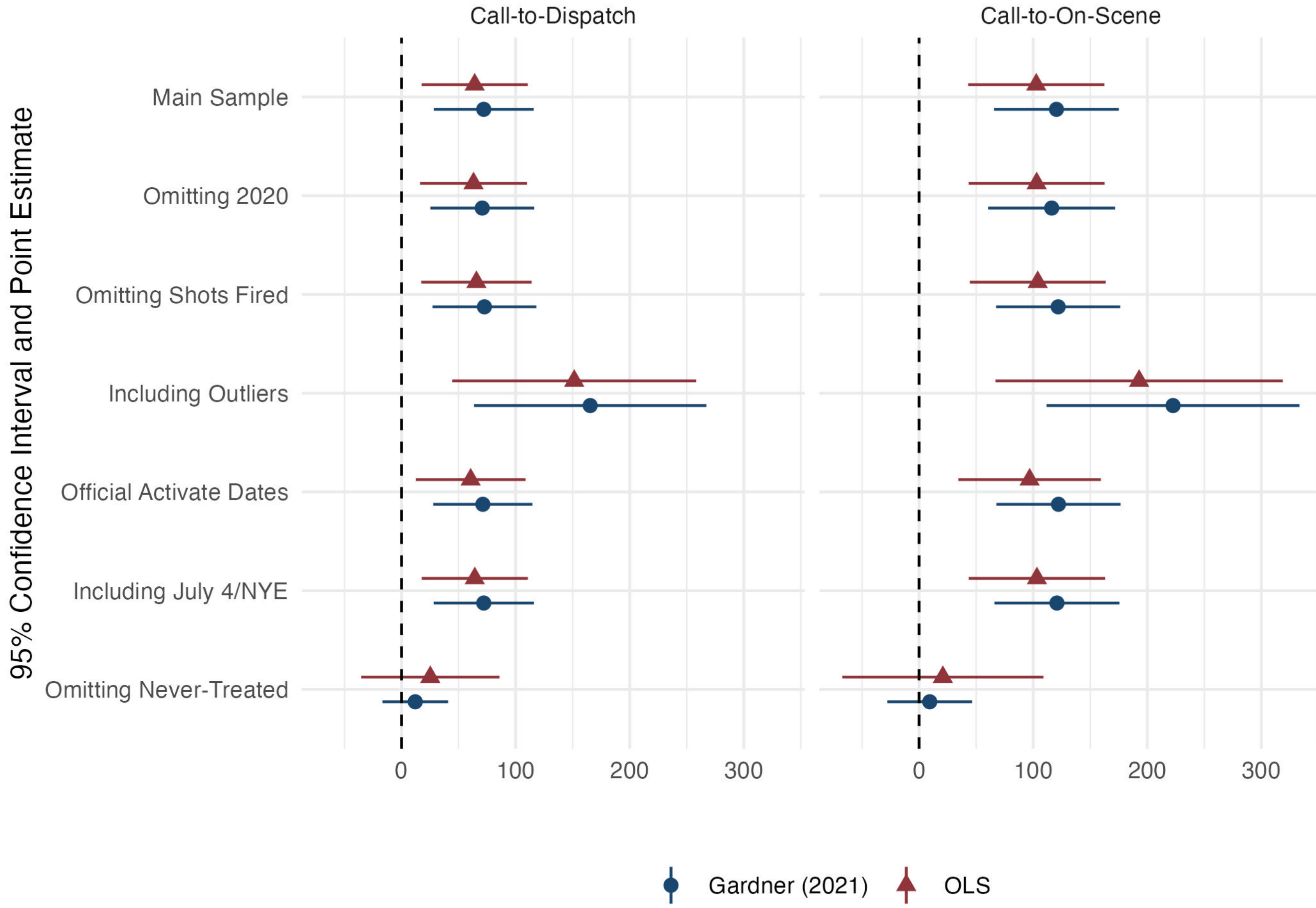

Figure D2: Robustness of Main Results

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 Year's Eve, and New Year's 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.

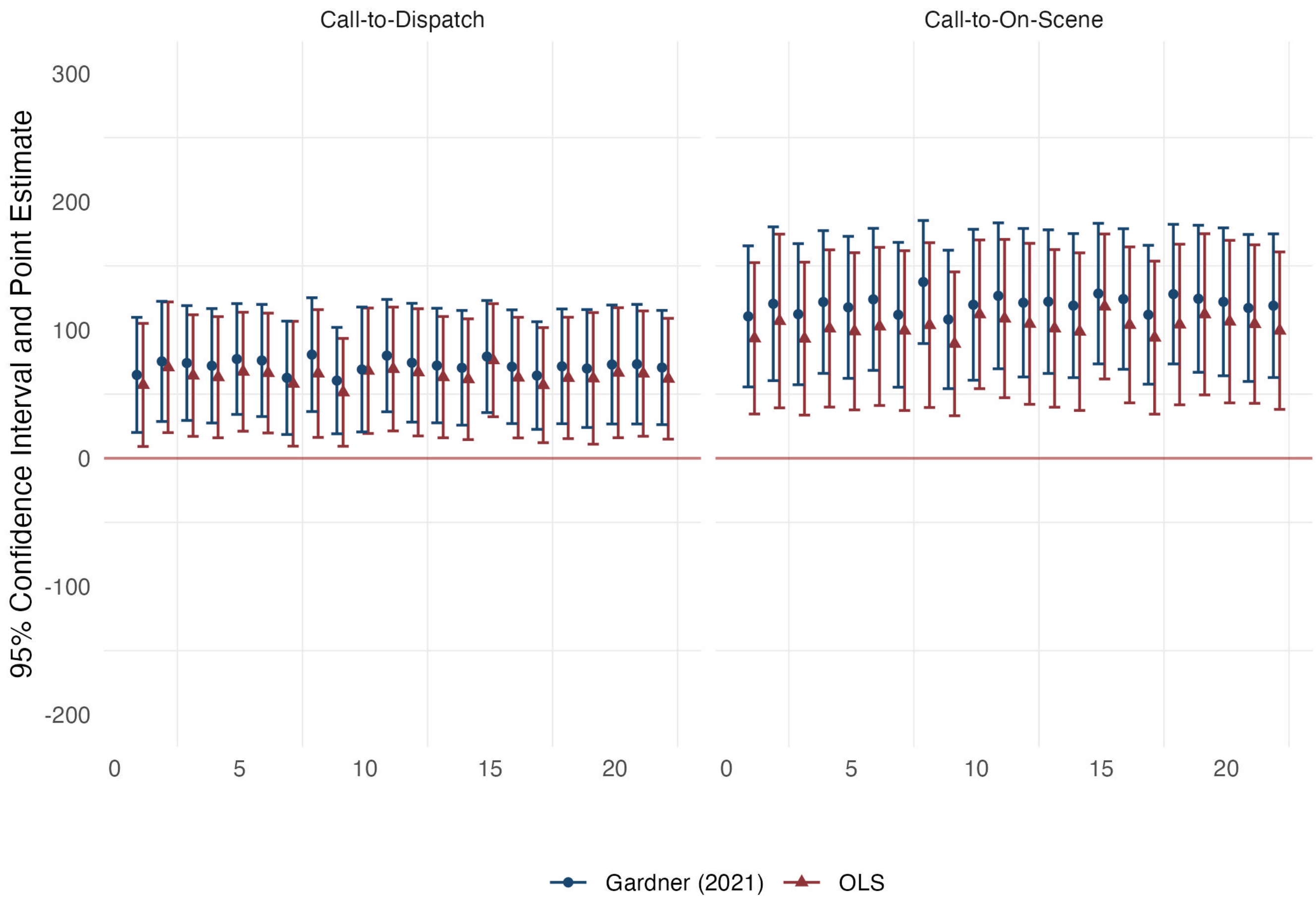

Figure D3: Leave-One-Out Analysis
Note: This figure shows the results from 22 distinct OLS and Gardner (2021) regressions using Equation 1. Both outcomes of Call-to-Dispatch and Call-to-On-Scene are pictured. In each iteration, one police district is removed from estimation to ensure that the effects of ShotSpotter are not driven by one district. The blue points and error-bars represent Gardner (2021) point estimates and 95% confidence intervals, which are robust to heterogeneous 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.

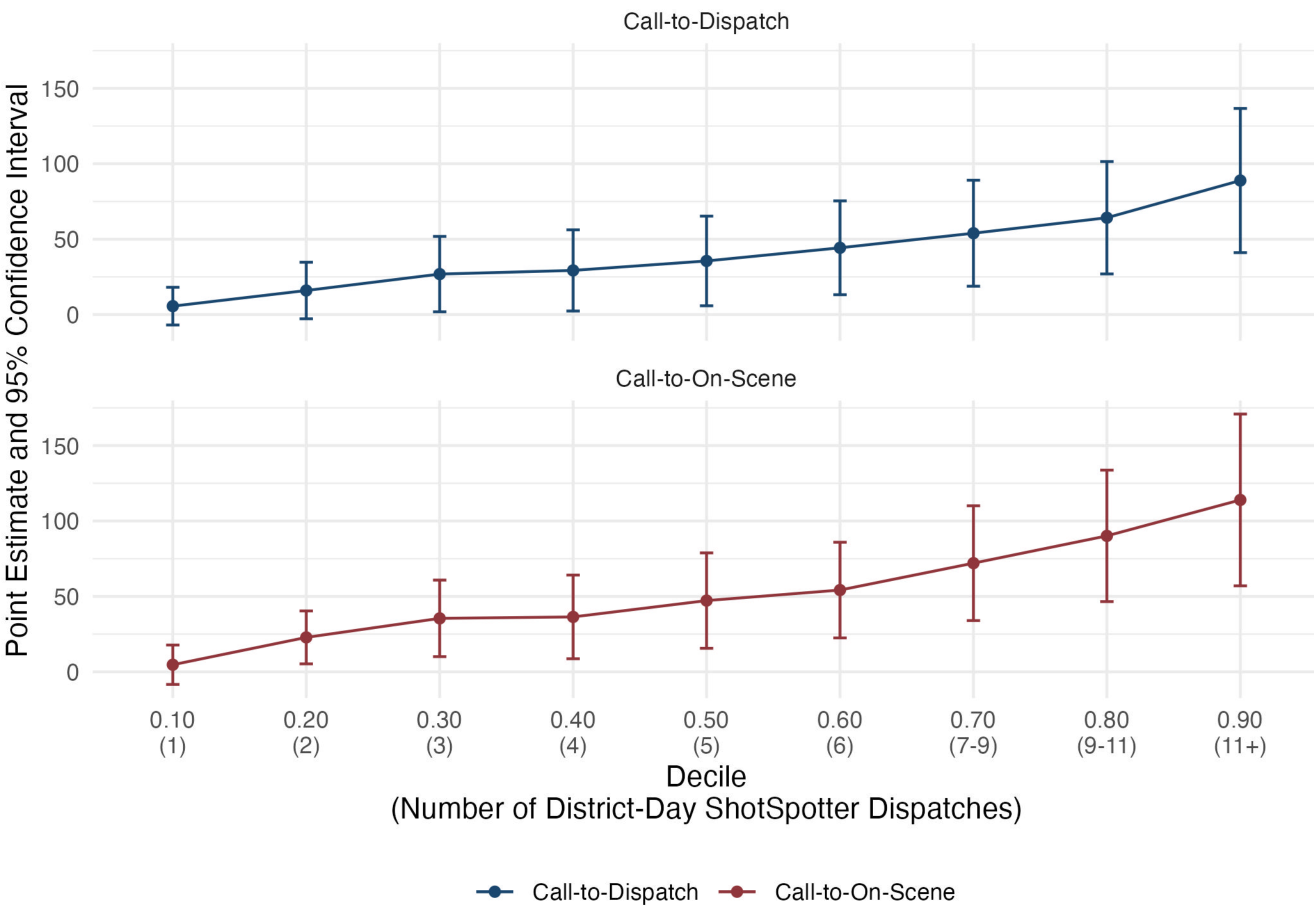

Figure D4: Marginal Effect of ShotSpotter Dispatches on Response Times (OLS)

Note: This figure shows the marginal effect of ShotSpotter dispatches as reported in Equation 3. However, the number of ShotSpotter dispatches is split into deciles to show the linear relationship between number of ShotSpotter dispatches and response times. In this figure, 9 deciles are plotted, with the reference decile being when the number of ShotSpotter dispatches is zero. All coefficient estimates are in seconds. Deciles are on the x-axis, and the number of ShotSpotter dispatches corresponding to each decile is in parentheses.