University of California
Santa Barbara

Essays in the Economics of Crime and Health

A dissertation submitted in partial satisfaction
of the requirements for the degree

Doctor of Philosophy
in
Economics

by

Michael Topper

Committee in charge:

Professor Heather Royer, Chair
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June 2024

The dissertation of Michael Topper is approved.

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May 2024

Essays in the Economics of Crime and Health

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EDUCATION

University of California, Santa Barbara: *PhD Economics* 2018 - Present

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PUBLICATIONS

The Effects of Fraternity Moratoriums on Alcohol Offenses and Sexual Assaults
Forthcoming: Journal of Human Resources 2023

I exploit variation in timing from 44 temporary university-wide halts on all fraternity activity with alcohol (moratoriums) across 37 universities over 2014-2019. I construct a novel data set, merging incident-level crime logs from university police departments to provide the first causal estimates of the effect of moratoriums on reports of alcohol offenses and sexual assaults. In particular, I find robust evidence that moratoriums decrease alcohol offenses by 26%. Additionally, I find suggestive evidence that moratoriums decrease reports of sexual assault on the weekends by 29%. However, I do not find evidence of long-term changes once the moratorium is lifted.

RESEARCH IN PROGRESS

Job Market Paper: *"The Effect of ShotSpotter Technology on Police Response Times"*
(with Toshio Ferrazares)

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%).

In Progress: *"Gunshot Noise and Birth Outcomes"*
(with Anna Jaskiewicz)

Gun violence is ubiquitous across the United States, with gun-related deaths reaching an all-time high in 2021. The prevalence of gunfire results in loud and potentially stress-inducing sounds, which may adversely affect critical stages of in-utero development. However, gunfire is largely unreported, creating a unique challenge for researchers to understand its consequences. In this paper, we mitigate this shortcoming by leveraging data from ShotSpotter—an acoustic gunshot technology which uses an array of sensors placed on city structures to detect the sound of gunfire. We combine this unique data source with the universe of births in San Francisco over a four-year period (2016-2020), each matched to a mother's residence. Using the variation in gunfire detections from ShotSpotter at the census-block level, we employ a difference-in-differences methodology and find that gunshot noise creates substantial decreases in gestation lengths, resulting in an increase in preterm deliveries. These effects are driven entirely by times of the day when civilians are awake, and are particularly concentrated among mothers with low levels of education. These results suggest that gunshot noise is a major factor contributing to the income inequities in pregnancy outcomes.

TEACHING EXPERIENCE

Instructor of Record: *University of California Santa Barbara* and *San Diego State University*

- Econ 199RA Data Hack - (UCSB: Spring 2022)
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Teaching Assistant: *University of California, Santa Barbara* and *San Diego State University*

- Introductory Microeconomics- (UCSB: Fall 2018/Winter 2019)
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- Introductory Econometrics A - (UCSB: Fall 2019/Winter 2020/Summer 2021)
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Course Curriculum Creator: *University of California, Santa Barbara*

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TEACHING RECOGNITION

Academic Senate Outstanding Teaching Assistant: *University of California, Santa Barbara*

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TEXTBOOK

Data Wrangling for Economists (with Danny Klinenberg)

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- Chapter 8 used in CSU San Luis Obispo course: GSE 570 Collaborative Software Development for Economists

SOFTWARE

R Package: panelsummary

- Creates publication-quality regression tables that have multiple panels. Available on CRAN. Over 1.5k downloads. Click here for the package website.

ACADEMIC RECOGNITION

M.C. Madhaven Prize Outstanding Graduate Student: *San Diego State University*

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Applied Microeconomics Lunch: *Santa Barbara* 2023

WORK EXPERIENCE

Research Assistant for Dr. Heather Royer: *University of California, Santa Barbara* 2020 - 2021

Appfolio: *Value Added Services: SaaS Intern* 2015 - 2016

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Abstract

Essays in the Economics of Crime and Health

by

Michael Topper

This dissertation contains three chapters on the economics of crime and health. In Chapter 1, we study how technology is integral to police departments, automating officer tasks, but inherently changing their time allocation. We investigate this by studying ShotSpotter, a technology that automates gunfire detection. Following a detection, officers are dispatched to the scene, thereby reallocating their time. We leverage this shock to officers' time allocation using the rollout of ShotSpotter across Chicago police districts to study the effects on 911 call response. We find substantial consequences—officers are dispatched to calls slower (23%), arrive on-scene later (13%), and the probability of arrest is decreased 9%. Consequently, police departments must evaluate their resource capacities prior to implementing technologies.

In Chapter 2, I exploit variation in timing from 44 temporary university-wide halts on all fraternity activity with alcohol (moratoriums) across 37 universities over 2014-2019. I construct a novel data set, merging incident-level crime logs from university police departments to provide the first causal estimates of the effect of moratoriums on reports of alcohol offenses and sexual assaults. In particular, I find robust evidence that moratoriums decrease alcohol offenses by 26%. Additionally, I find suggestive evidence that moratoriums decrease reports of sexual assault on the weekends by 29%. However, I do not find evidence of long-term changes once the moratorium is lifted.

Finally, in Chapter 3, we study the prevalence of gunfire, which results in loud and potentially stress-inducing sounds that may adversely affect critical stages of in utero development. However, gunfire is largely unreported, creating a unique challenge for researchers to understand its consequences. In this paper, we mitigate this shortcoming by leveraging data from ShotSpotter—an acoustic gunshot technology which uses an array of sensors placed on city structures to detect the sound of gunfire. We combine this unique data source with the universe of births from nine California cities, each matched to a mother's residence. Using the variation in gunfire detections from ShotSpotter at the census-block level, we employ a difference-in-differences methodology and find that gunshot noise creates substantial increases in very low birth weight (¡ 1,500 grams) and very pre-term births (¡ 32 weeks). These effects are driven by times of the day when mothers are likely to be at-home, and are particularly concentrated among mothers with low levels of education. These results suggest that gunshot noise is a major factor contributing to the income inequities in pregnancy outcomes.

The Unintended Consequences of Policing Technology: Evidence from ShotSpotter*

Michael Topper and Toshio Ferrazares**

Draft Date: July 11, 2024

Latest Version Link

Abstract

Technology is integral to police departments, automating officer tasks, but inherently changing their time allocation. We investigate this by studying ShotSpotter, a technology that automates gunfire detection. Following a detection, officers are dispatched to the scene, thereby reallocating their time. We leverage this shock to officers' time allocation using the rollout of ShotSpotter across Chicago police districts to study the effects on 911 call response. We find substantial consequences—officers are dispatched to calls slower (23%), arrive on-scene later (13%), and the probability of arrest is decreased 9%. Consequently, police departments must evaluate their resource capacities prior to implementing technologies.

JEL Codes: D04, H40, R41

* Previously circulated under the title "The Effect of ShotSpotter Technology on Police Response Times"

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1 Introduction

In the contemporary police department, technology possesses the potential to serve as either a substitute or complement to human capital. In particular, police departments are utilizing technologies both as substitutes, effectively functioning as 'eyes-on-the-street' through facial recognition and traffic cameras, as well as collaborative complements in targeting high-crime areas. These 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 technology systems is fundamentally reshaping the nature of policing.

One quickly expanding and widely adopted police technology is ShotSpotter—an acoustic gunfire detection technology that is currently implemented in over 150 cities worldwide. 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 gunfire. In Chicago, the setting of this paper, this results in approximately 70 ShotSpotter-related dispatches each day, equating to 75 hours of officer investigation time.[1] This represents a two-fold increase in

[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.

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 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 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

[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`

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 9% 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 effect of technology on policing, the criminal justice system, and in a wider context, efficient workforce allocation and policies. 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; Kim, 2019a), electronic monitoring (Williams and Weatherburn, 2022; Rivera, 2023), 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] As a consequence, our results give further evidence that efficient allocation and effective policies are imperative for better policing outcomes (Getty et al., 2016; Ba et al., 2021; Kapustin et al., 2022a; Rivera and Ba, 2023; Adger et al., 2023), and on a larger scale, general workforce productivity (Hsieh and Klenow, 2009; Fenizia, 2022).

More broadly, this study adds to the claim that police departments are personnel-constrained, and potentially understaffed 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 (Levitt, 1997; Chalfin and McCrary, 2018; Mello, 2019; Weisburst, 2019; Weisburd, 2021). 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 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 responsibili-

[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.

ties, in order to prevent under-policing in communities.

Lastly, we build upon the rapid-response literature related to health outcomes (Leslie and Wilson, 2020; DeAngelo et al., 2023). 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 at the 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; Connealy et al., 2024) 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*.

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.

[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.

[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.

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 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 distinc-

[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.

[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.

tion 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 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]

[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.

[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.

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 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 Appendix Figure D2.

[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.

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

[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.

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 ShotSpotter 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 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. (2022b) 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,

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

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, 2019b; 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 non-linearity 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.

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

[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.

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 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

[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'.

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. 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

[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.

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 widespread 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 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 adoption of a new policing technology that crowds out police officer time and disrupts the availability of officers. We do so by exploring the effect of 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'.

[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.

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.

Furthermore, we find evidence that ShotSpotter increases response times by reallocating scarce police resources from 911 emergency calls to ShotSpotter-detected gunfire alerts (ShotSpotter dispatches), resulting in a significant time trade-off. 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 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

[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.

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 analysis.

Hence, we cannot advocate for, nor against ShotSpotter, but aim to inform policy-makers of the substantial unintended consequence it, and similar technologies, creates. However, given the analysis, we find that ShotSpotter creates a resource constraint problem where officers have too many responsibilities. This is important given that police dispatch queuing models suggest that response time is highly sensitive to the arrival rate of 911 emergency calls.[30] 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.[31] This underscores the notion that police technology such as ShotSpotter, as of now, can possibly act as a valuable complement for police officers, but not as a perfect substitute.

[30] See Green and Kolesar (1989) for the M/M/d queuing model setup (also called Erlang-C) and application to empirical data.

[31] 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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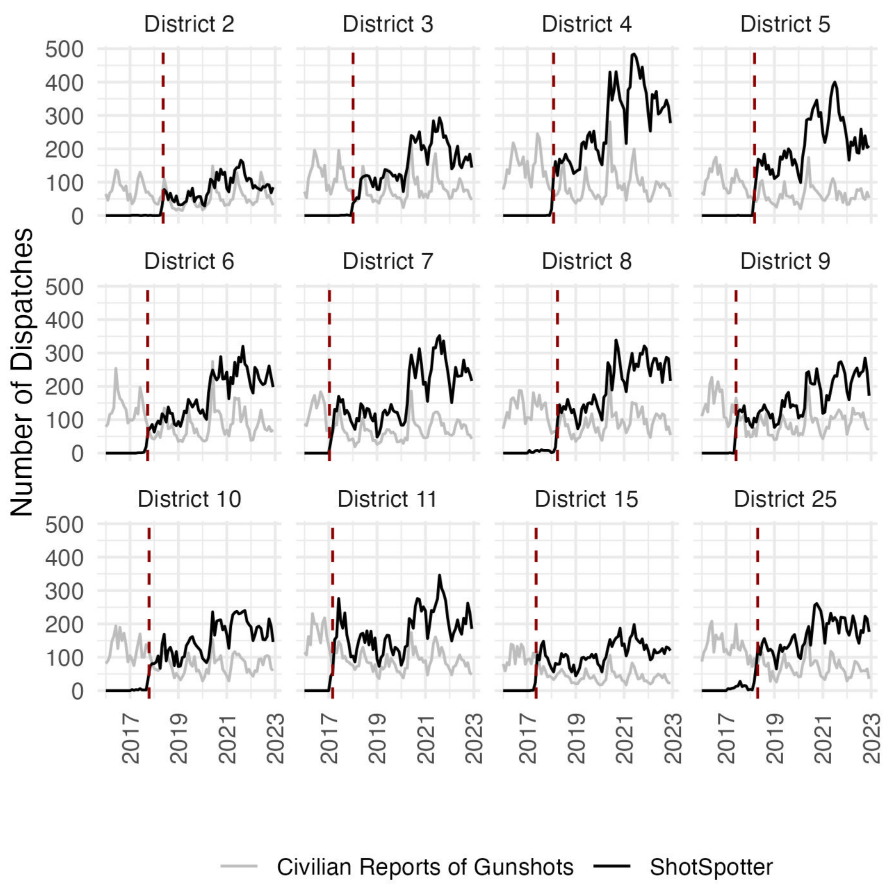


District	Enactment
2	2018-05-16
3	2018-01-04
4	2018-02-01
5	2018-03-07
6	2017-09-24
7	2017-01-13
8	2018-04-01
9	2017-06-01
10	2017-10-16
11	2017-03-01
15	2017-05-13
25	2018-04-24

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.

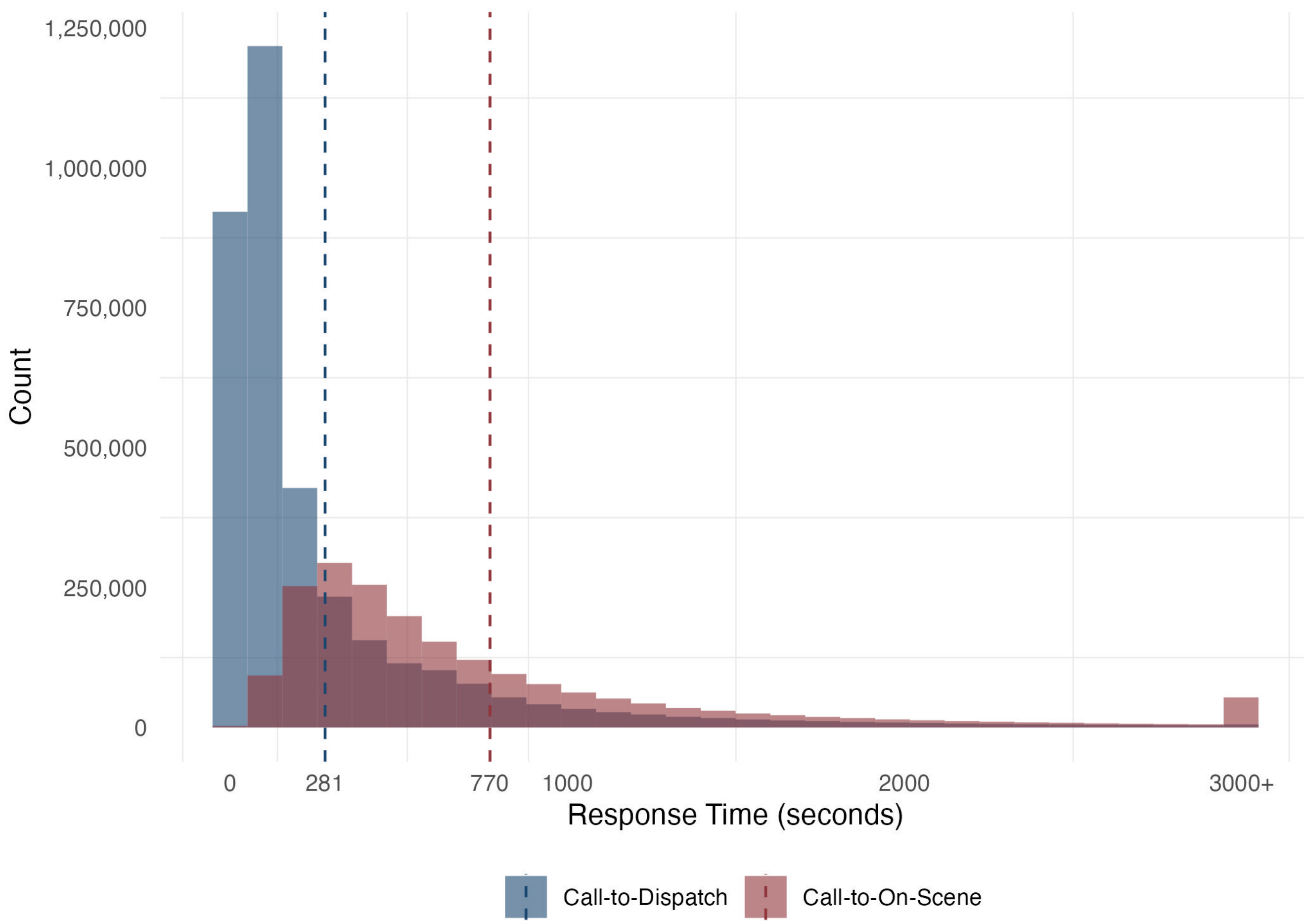


Figure 2: Distribution of Outcome Variables

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.

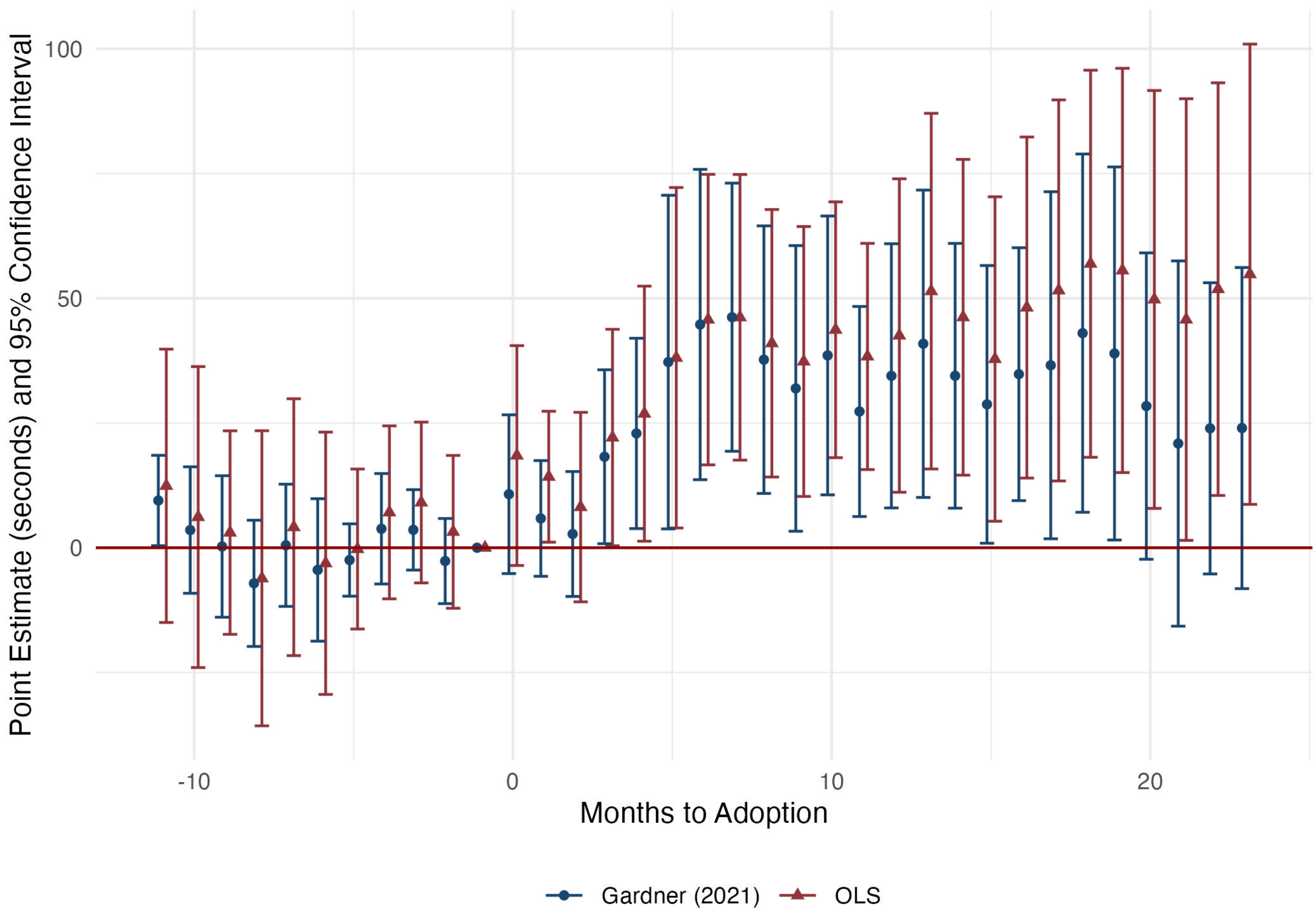


Figure 3: Event Study (Call-to-Dispatch)

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.

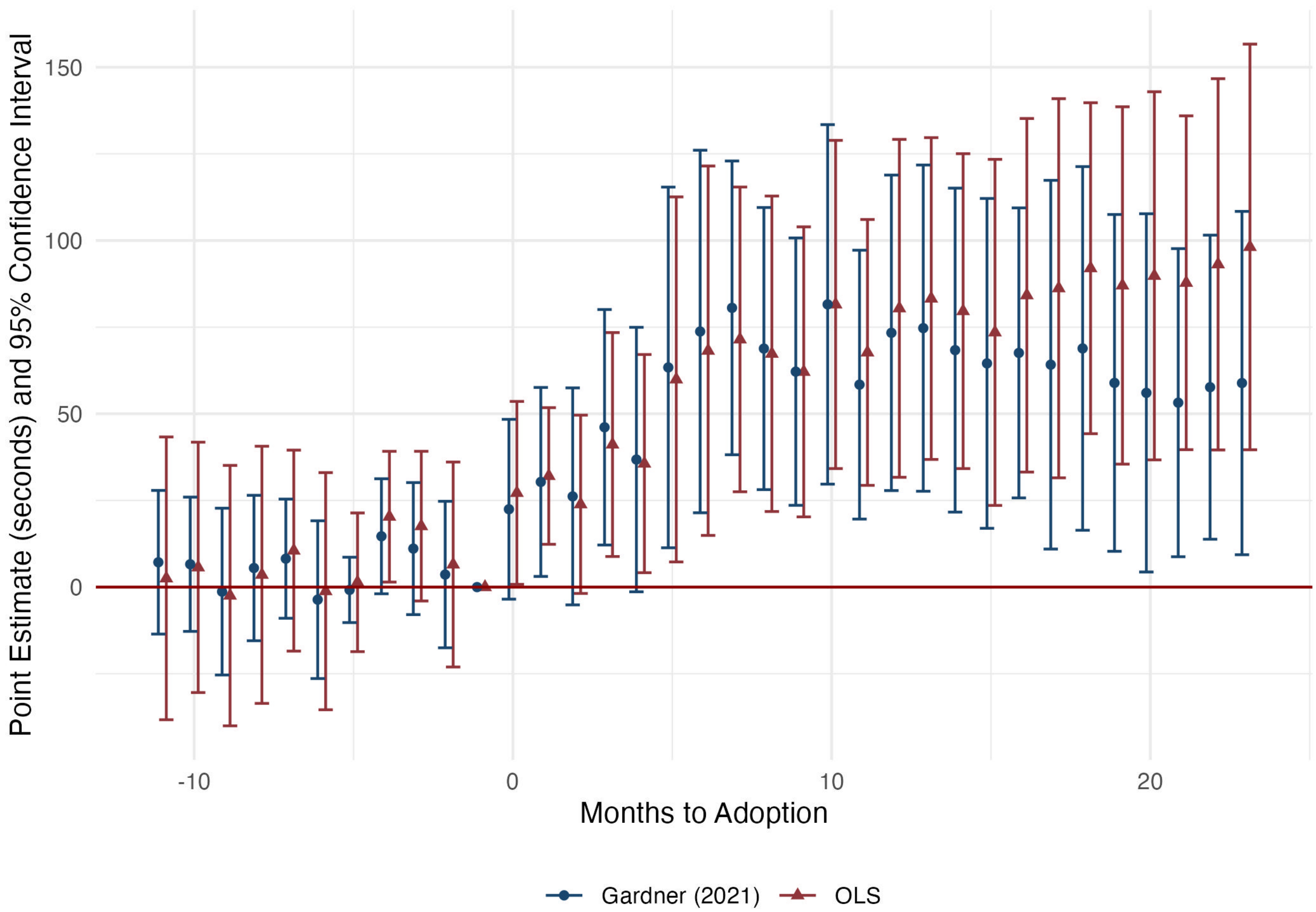


Figure 4: Event Study (Call-to-On-Scene)

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.

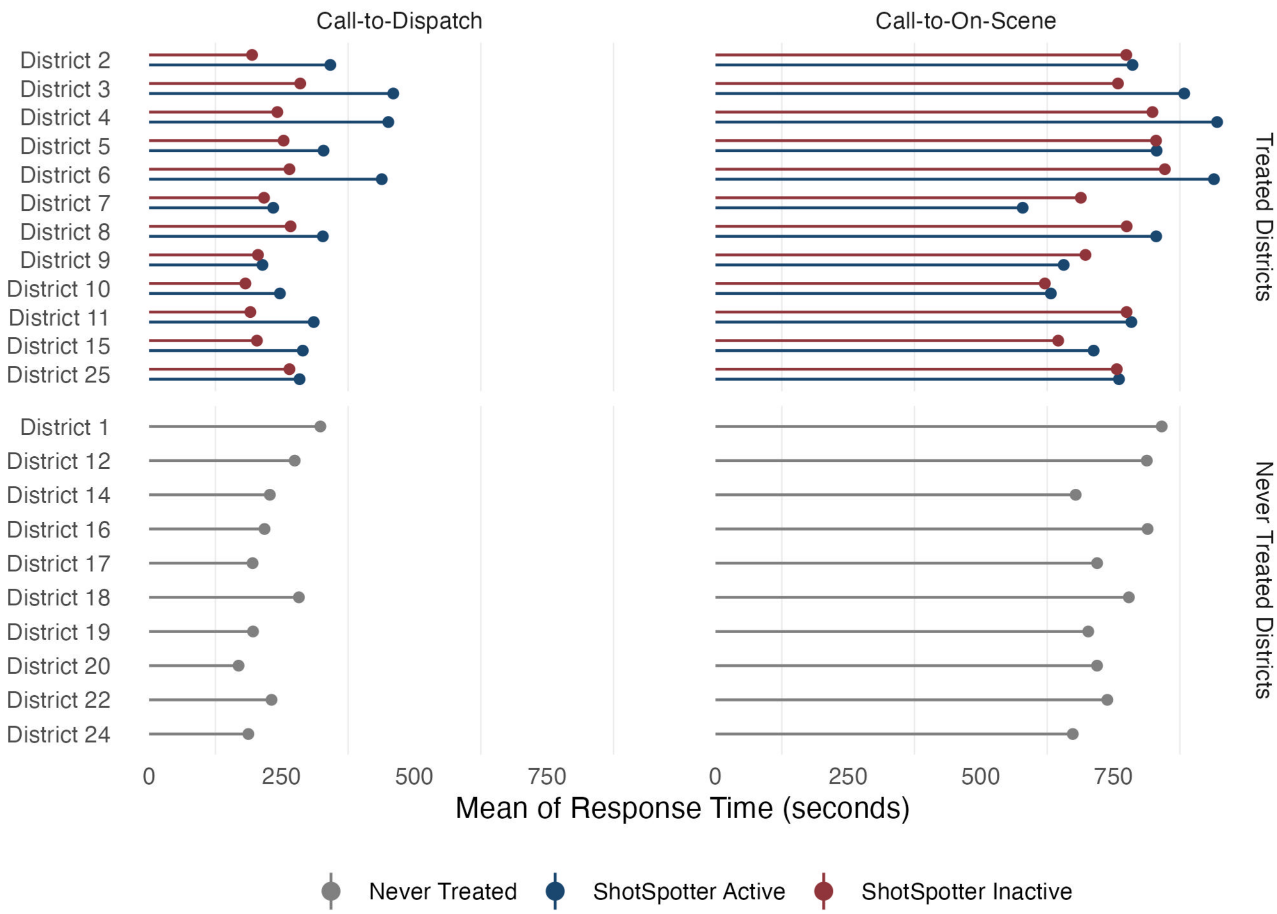
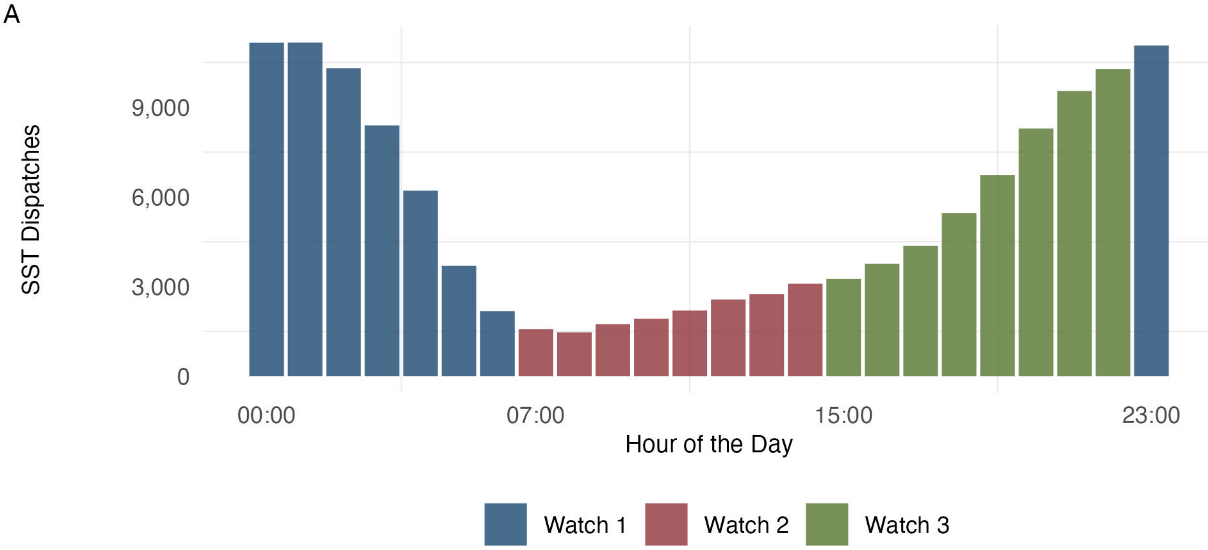


Figure 5: Average Outcomes in Police Districts

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.

Panel A



Panel B

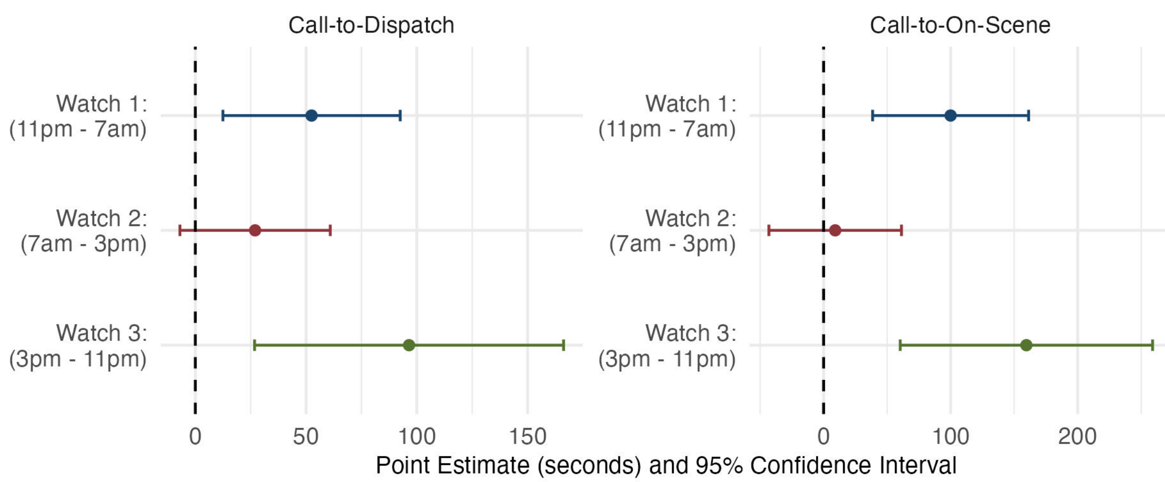


Figure 6: Effect of ShotSpotter by Officer Watch Times

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).

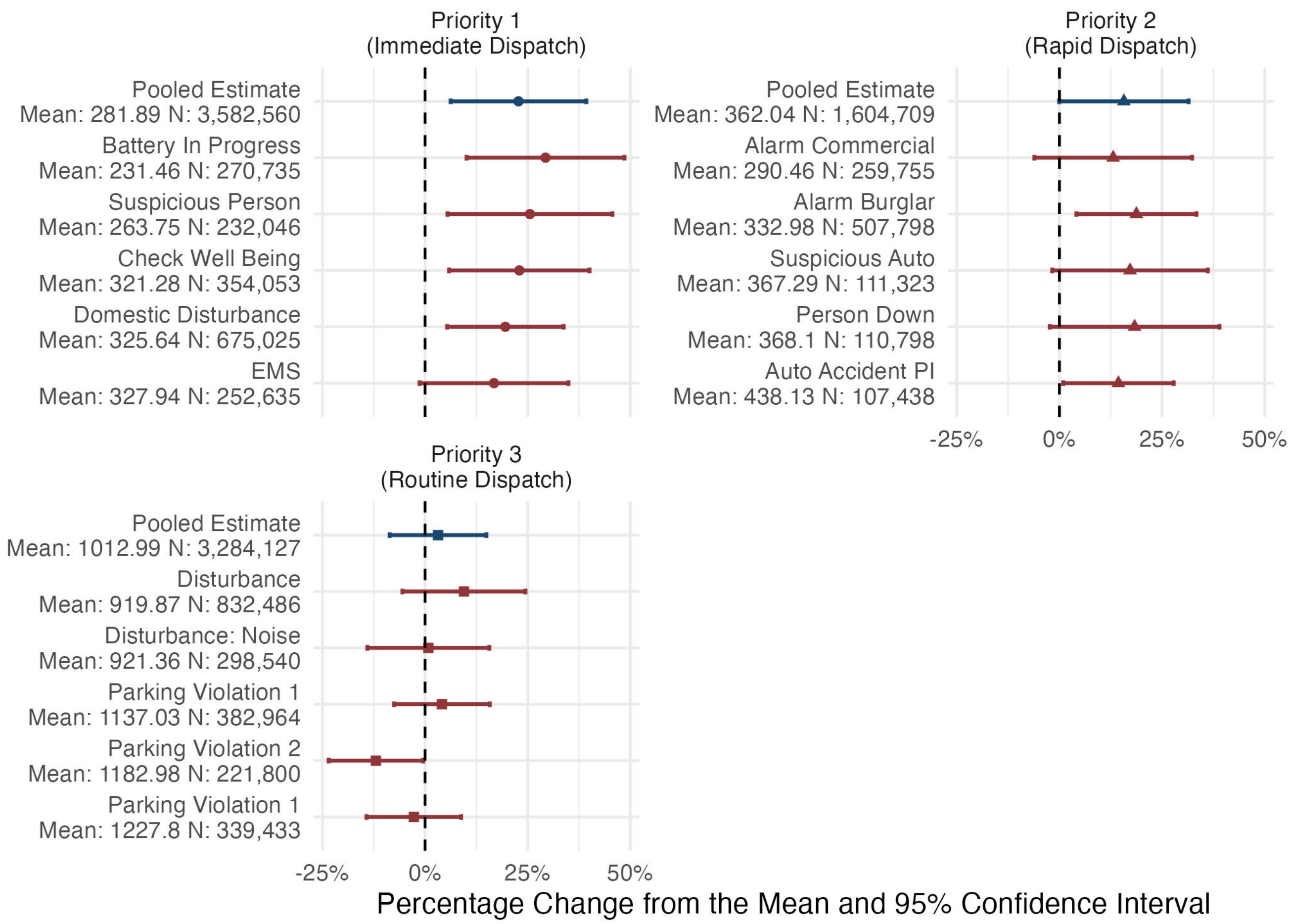


Figure 7: Effect of ShotSpotter by Priority (Call-to-Dispatch)

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.

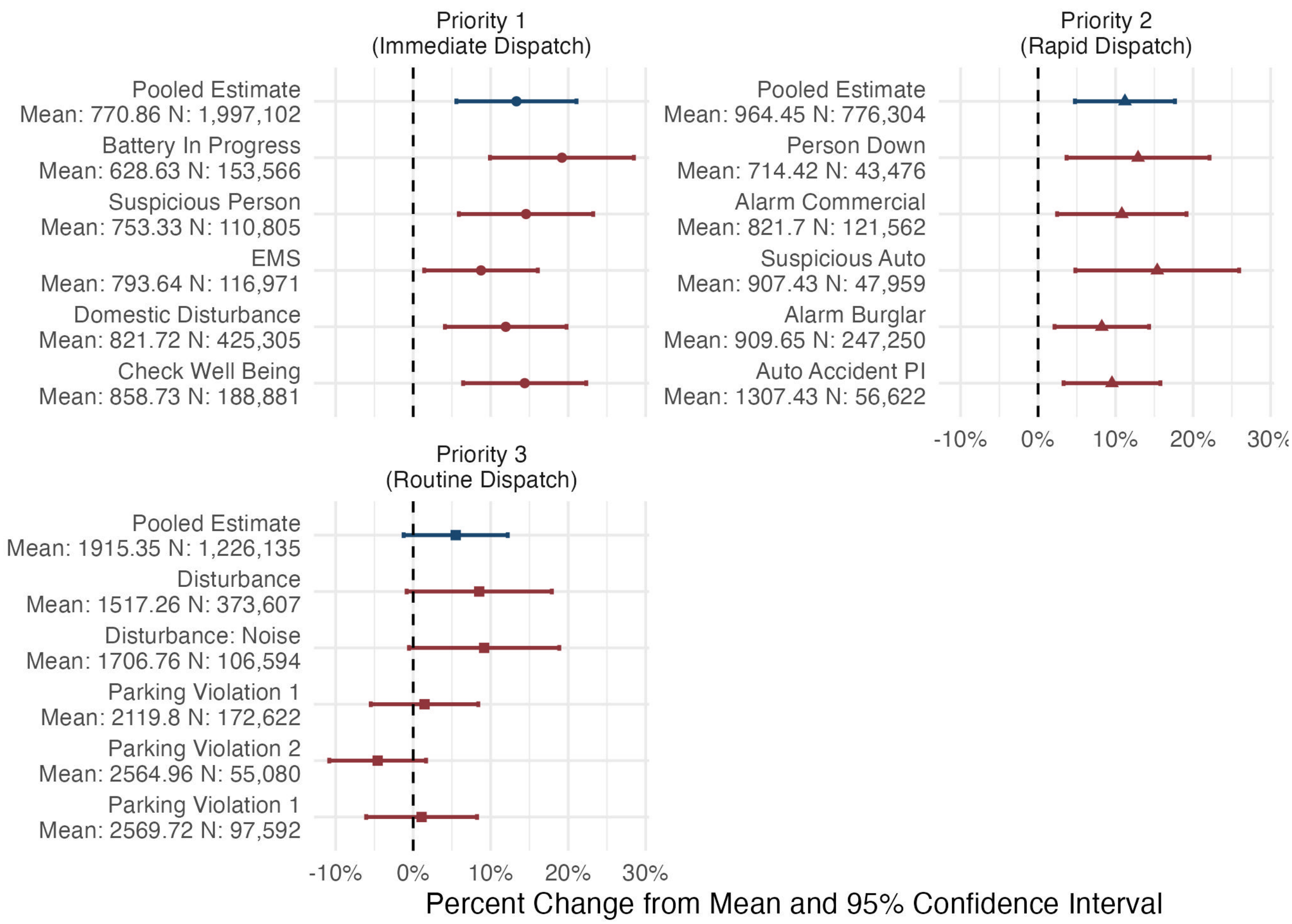


Figure 8: Effect of ShotSpotter by Priority (Call-to-On-Scene)

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).[32] 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 Call-to-On-Scene findings.

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

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.

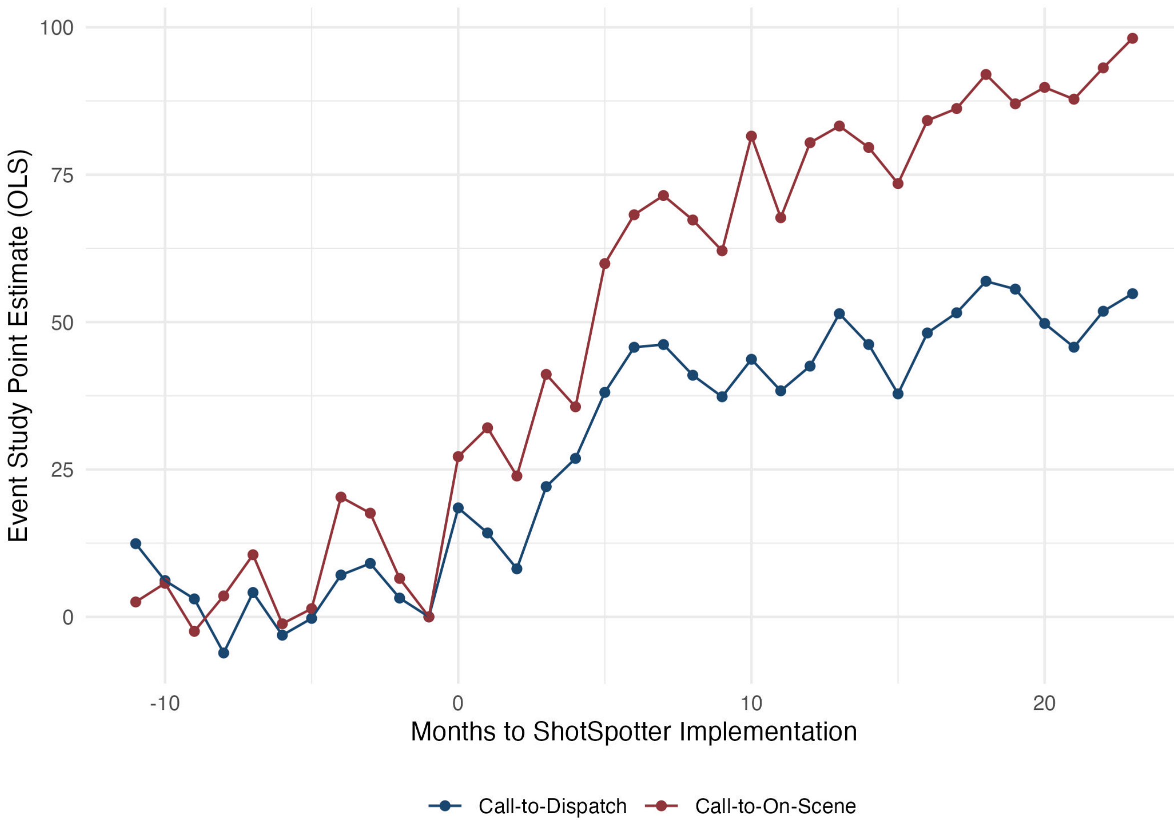


Figure A1: Event Study Point Estimates Trend

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).[33] While most of these technologies have already been in utilization by the CPD prior to SDSCs,[34] the Hunchlab software is implemented at the exact timing of an SDSC.

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

[33] 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.

[34] 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.

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. (2022b). 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., 2022b). 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, 2019b; 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.

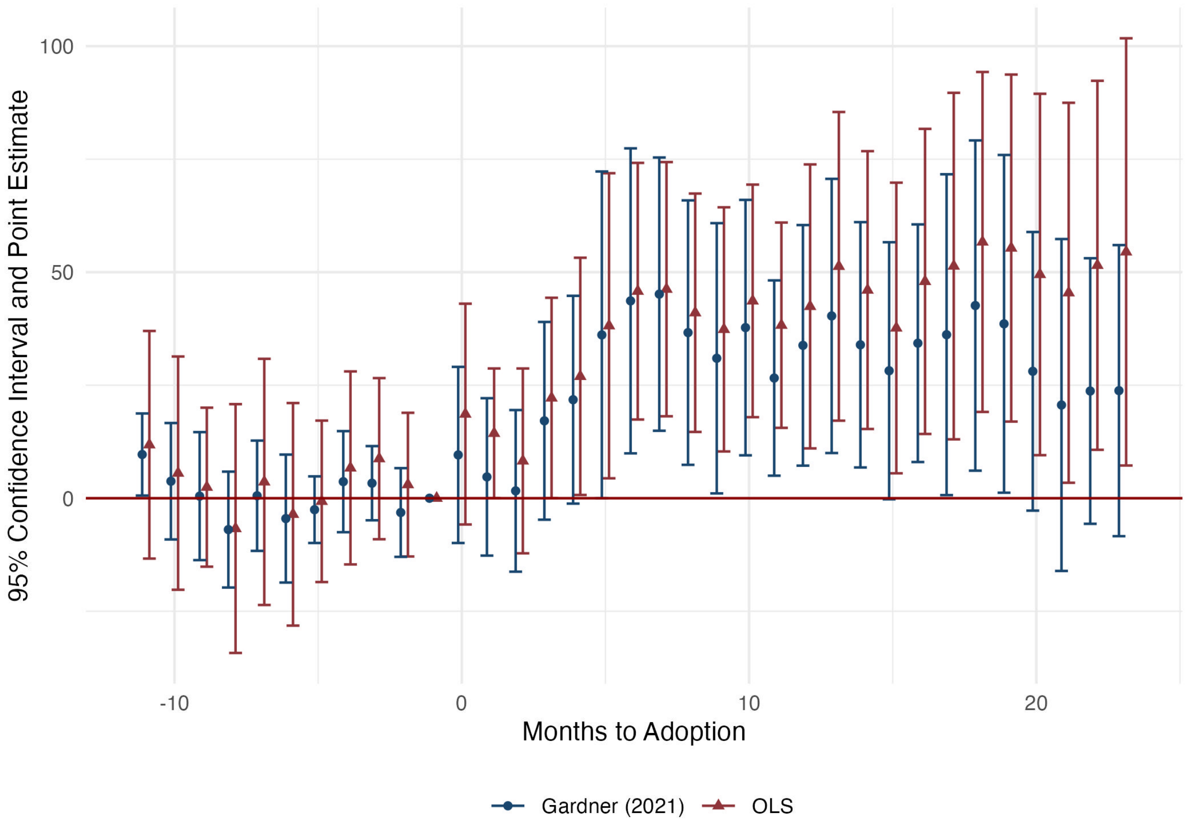


Figure B1: Event Study w/ SDSC Controls (Call-to-Dispatch)

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.

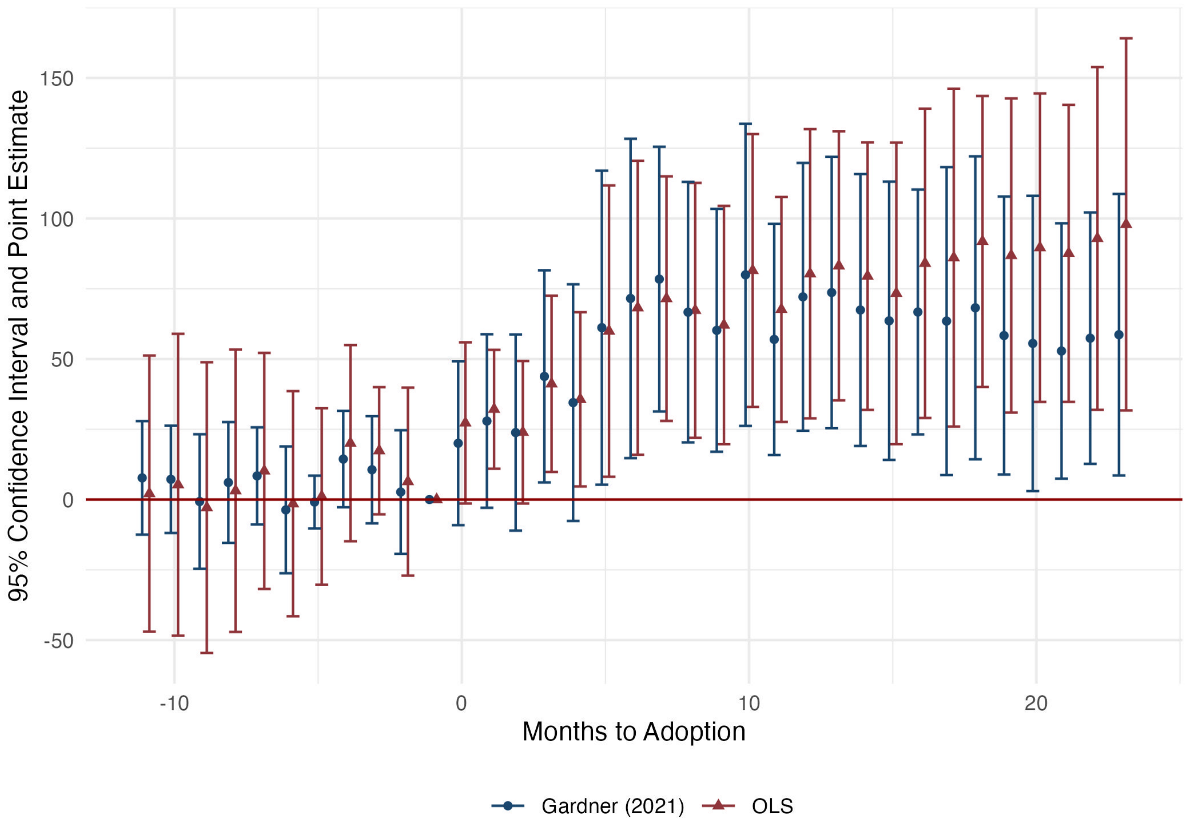


Figure B2: Event Study w/ SDSC Controls (Call-to-On-Scene)

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.

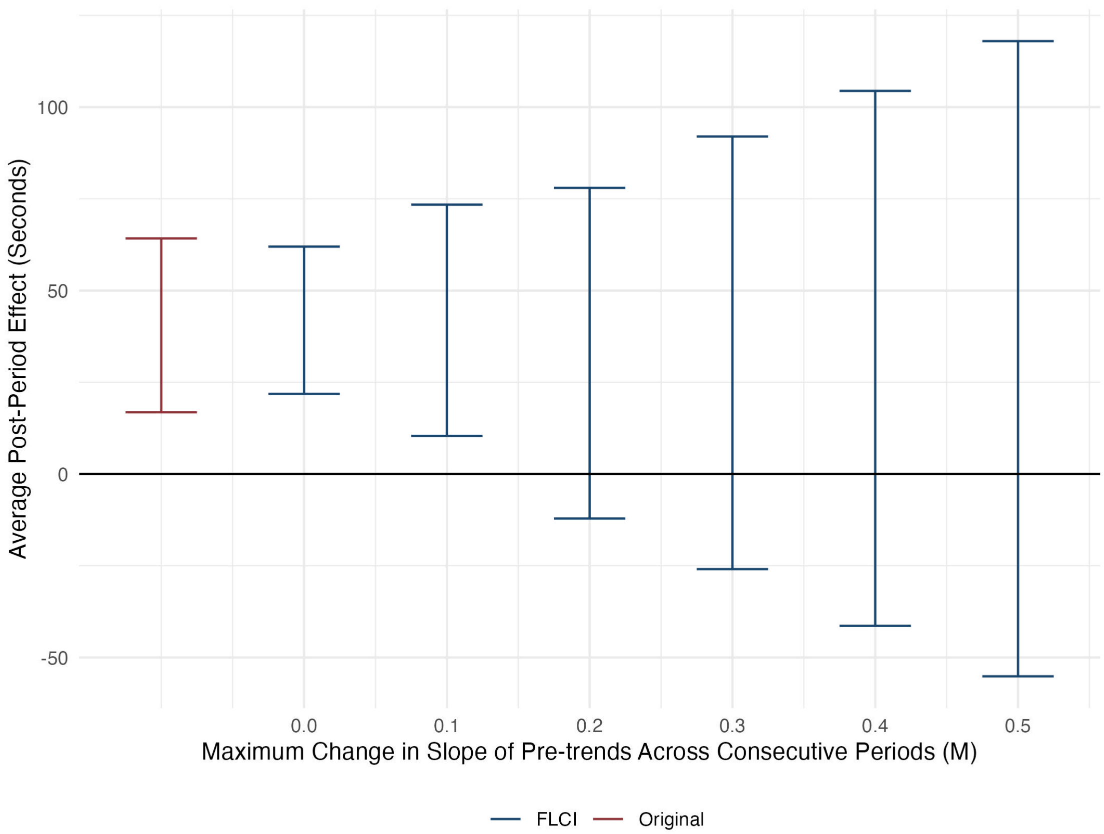


Figure C1: Sensitivity Analysis of Pre-Trends

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.

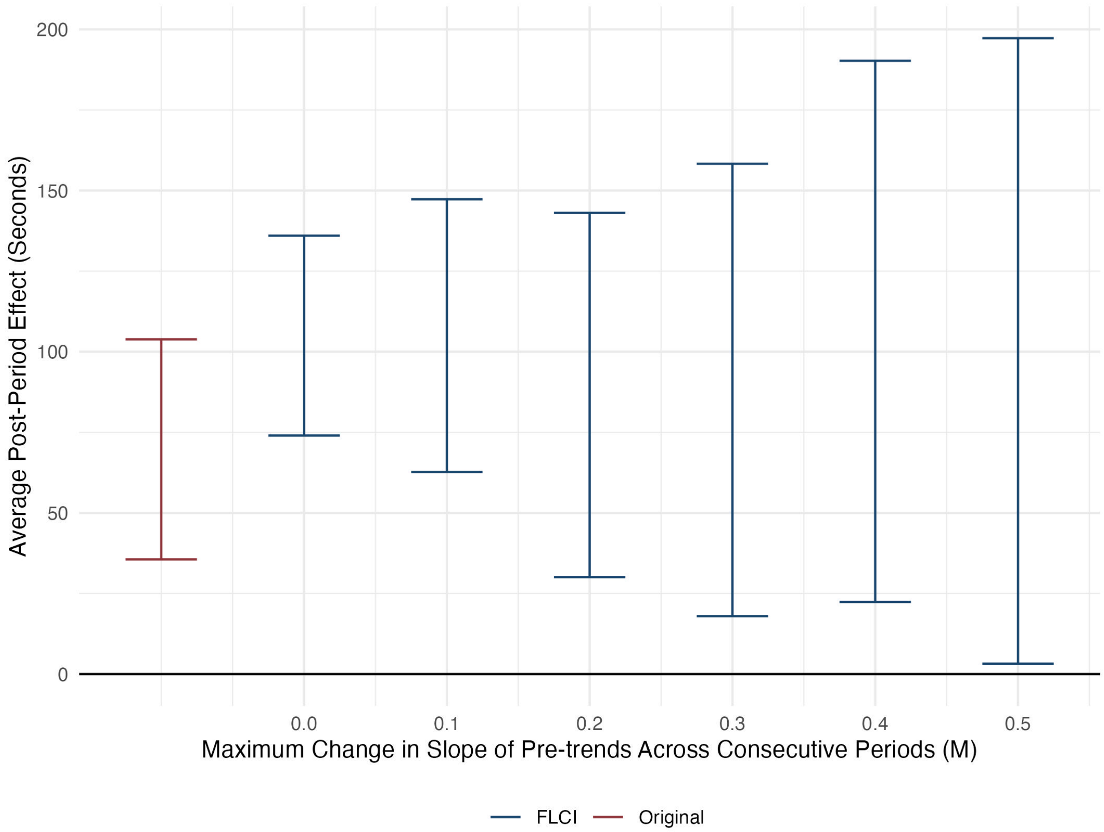


Figure C2: Sensitivity Analysis of Pre-Trends (Call-to-On-Scene)

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.

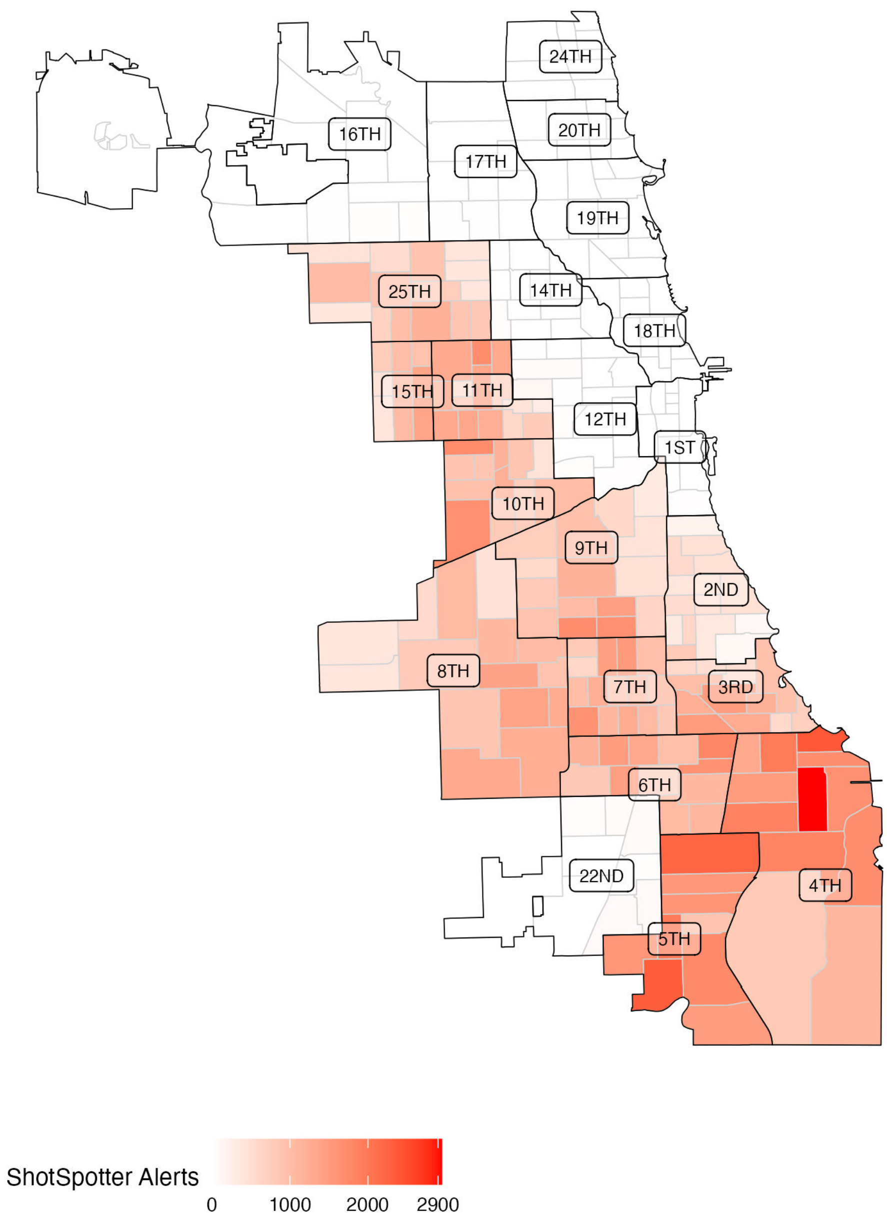


Figure D1: Map of ShotSpotter Districts in Chicago
Note: There are 22 police districts in Chicago, and 12 are equipped with ShotSpotter technology. Each district contains beats which are designated by the boxes within the district lines. ShotSpotter implementation began in January 2017 and ended in May 2018.

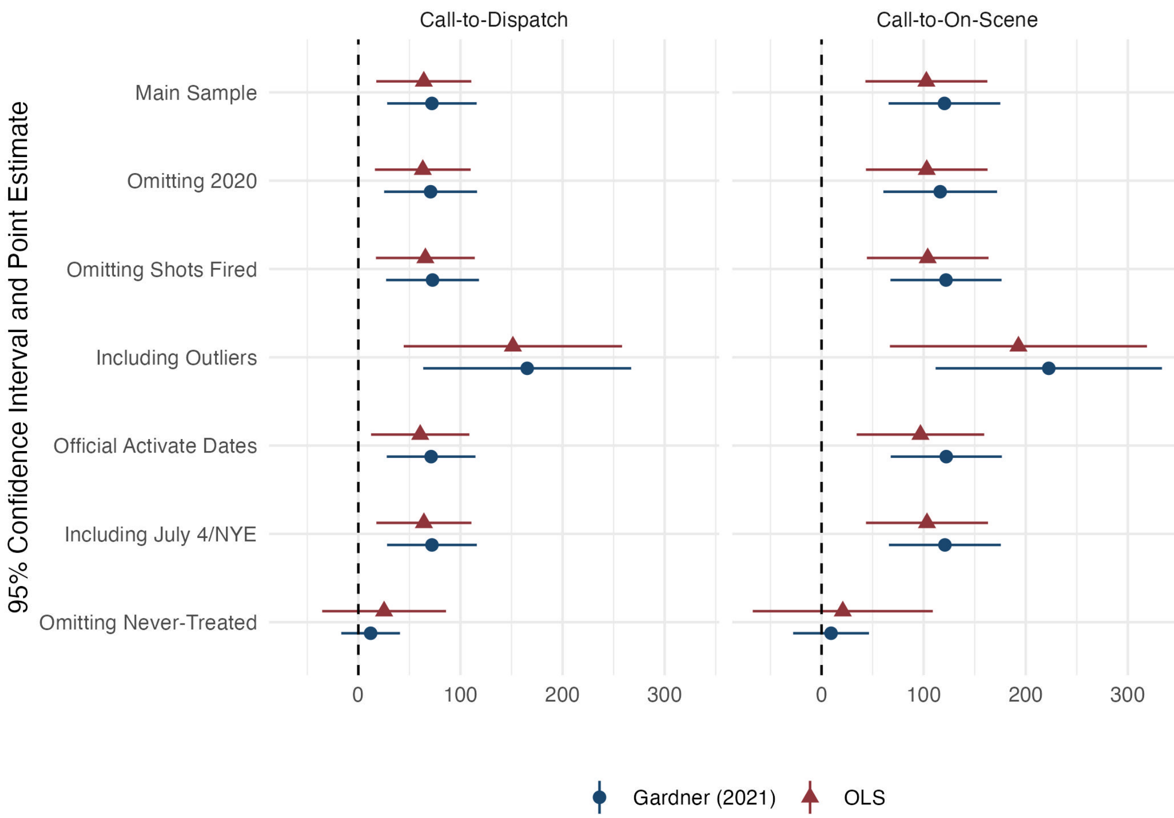


Figure D2: Robustness of Main Results

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.

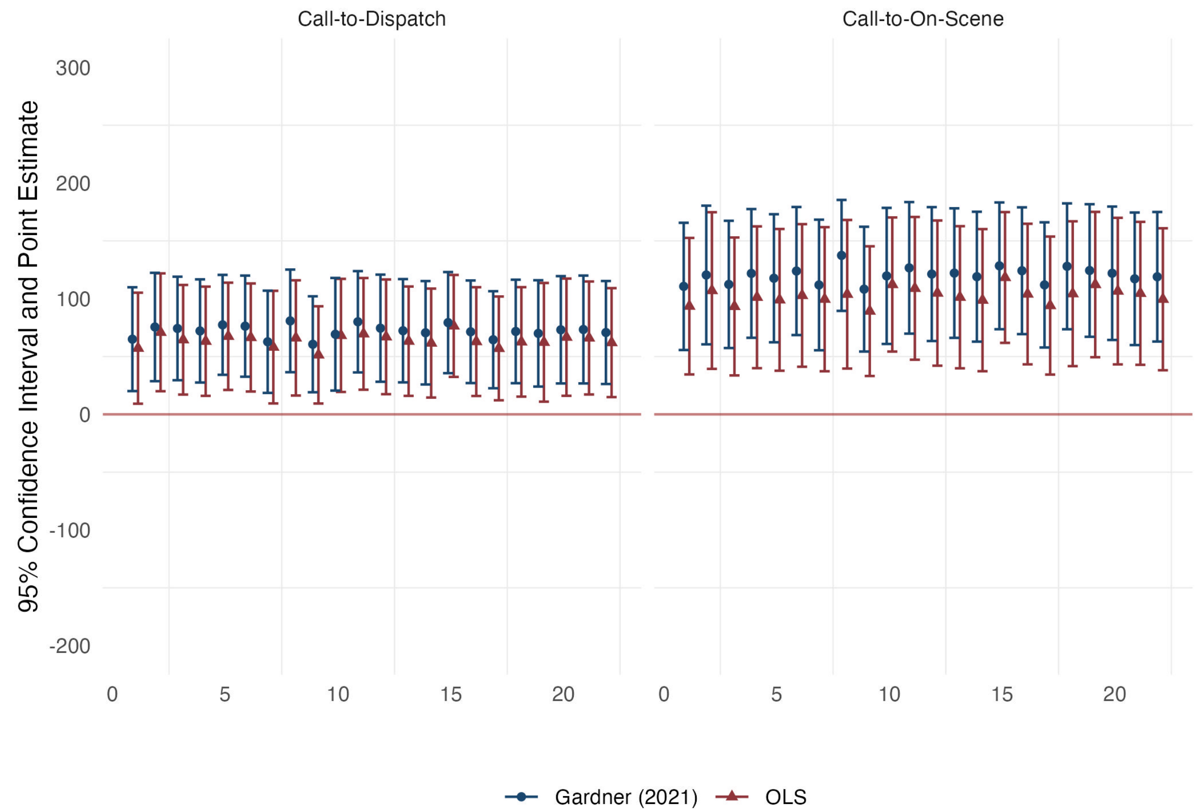


Figure D3: Leave-One-Out Analysis

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.

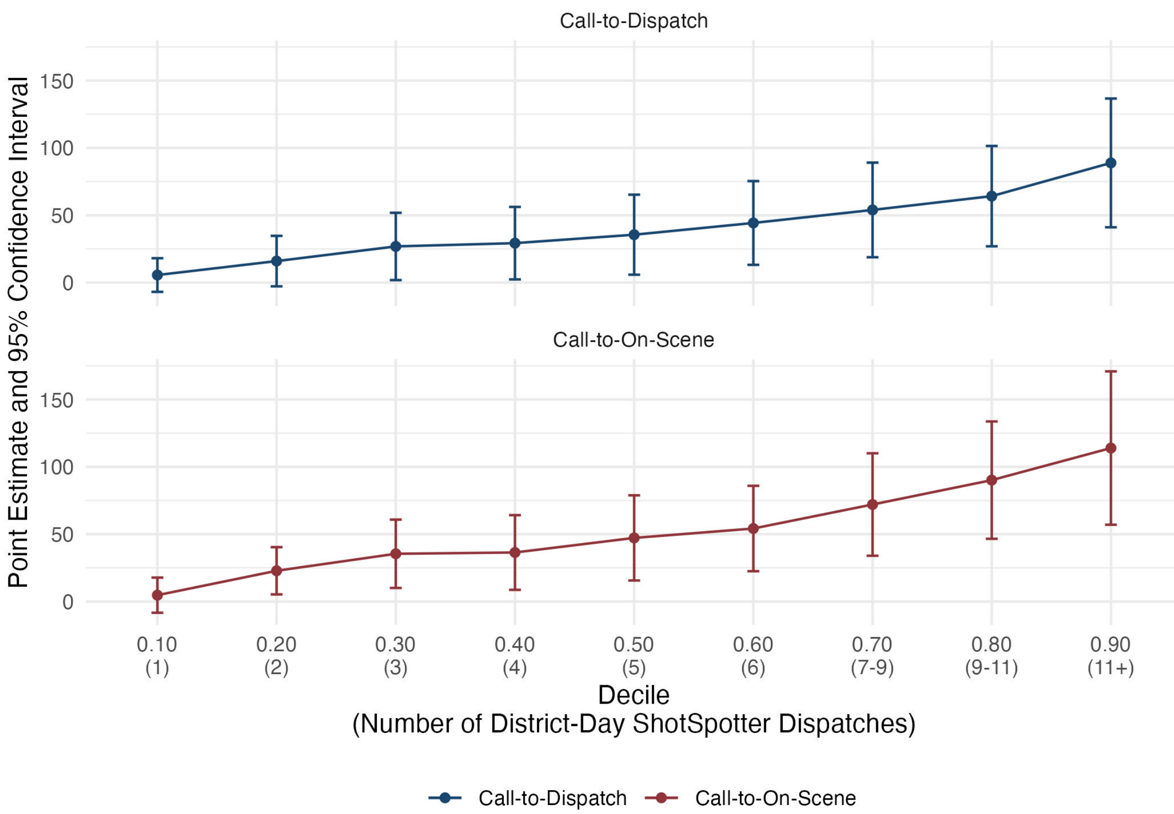


Figure D4: Marginal Effect of ShotSpotter Dispatches on Response Times (OLS)

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.

The Effect of Fraternity Moratoriums on Alcohol Offenses and Sexual Assaults

Michael Topper*

Abstract

I exploit variation in timing from 44 temporary university-wide halts on all fraternity activity with alcohol (*moratoriums*) across 37 universities over 2014-2019. I construct a novel data set, merging incident-level crime logs from university police departments to provide the first causal estimates of the effect of moratoriums on reports of alcohol offenses and sexual assaults. In particular, I find robust evidence that moratoriums decrease alcohol offenses by 26%. Additionally, I find suggestive evidence that moratoriums decrease reports of sexual assault on the weekends by 29%. However, I do not find evidence of long-term changes once the moratorium is lifted.

JEL Codes: I12, I28, K42

Please note that an Online Appendix is included.

Disclosure Statement: I, Michael Topper, declare that I have no relevant or material financial interests that relate to the research described in this paper.

Data Replication Statement: This paper uses a combination of publicly available data from multiple sources. The raw data can be obtained through Clery Act/Freedom of Information requests at each unique university. Additionally, I am willing to assist in data requests for replication (michaeltopper@ucsb.edu). All replication files, including the master data, are available at github.com/michaeltopper1/Fraternities. \

*PhD Candidate in the Economics Department at the University of California, Santa Barbara (michaeltopper@ucsb.edu). I would like to thank Heather Royer, Kevin Schnepel, Dick Startz, Kelly Bedard, Clément de Chaisemartin, Toshio Ferrazares, Anna Jaskiewicz, Elizabeth Tucker, and the members of the UCSB 290 Applied Research Group for their advice and feedback on various drafts of this paper. Special thanks to Terry Cheng, all public records officials, Fraternity and Sorority Life advisors, and police department officials who assisted me in collecting the data used in this paper. All errors are my own.

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1 Introduction

Over 800 universities in the United States have fraternities (Hechinger 2017). Existing literature has documented benefits of membership which include higher future income (Mara, Davis, and Schmidt 2018) and significantly more hours spent participating in community service and volunteering (Hayek et al. 2002; Asel, Seifert, and Pascarella 2009). Moreover, according to a Gallup survey in 2021, over 80% of fraternity alumni agreed that they would join their fraternity again if they were to redo their college experience.

Despite these benefits, fraternity membership has been associated with risky behaviors. In particular, at least one hazing-related death has occurred each year in the US between 2000 and 2019,[1] and studies have found that fraternity members binge drink and party more frequently than their non-member peers (DeSimone 2007; Routon and Walker 2014). While universities have regularly banned specific misbehaving fraternities from their campuses, the past decade popularized a new policy tool called *moratoriums*—campus-wide halts on fraternity social events with alcohol—as a way to change member behavior.

This paper is the first to estimate the causal effects of moratoriums on campus-wide police reports of alcohol offenses and sexual assaults. Between 2010 and 2019, over 50 moratoriums have been enacted across university campuses, becoming a common policy used by school administrators. However, studying this topic is challenging for several reasons; moratorium dates are difficult to find/confirm and there does not exist a centralized data source for university-specific crime with enough detail to enable casual inference. Despite the lack of research surrounding the efficacy of moratoriums, administrators continue to implement moratoriums as a disciplinary action on fraternities.

Nonetheless, how these moratoriums affect student behavior, and thus on-campus crime, is theoretically unclear. On one hand, prohibiting alcohol from fraternity social events may reduce the incidence of crime. Fraternities are a source of alcohol for underage drinking, as fraternities are typically a mix of lower and upperclassmen (Armstrong, Hamilton, and Sweeney 2006). Given that the literature has documented that alcohol causes higher prevalence of crimes such as assaults and alcohol offenses (Carpenter and Dobkin 2015), road accidents and arrests (Francesconi and James 2019), and reports of rape (Zimmerman and Benson 2007; Lindo, Siminski, and Swensen 2018), prohibiting such events could reduce the incidence of on-campus crime—especially for underage students. On the other hand, moratoriums may have the opposite effect. Without alcohol-fueled fraternity parties, students may substitute away from consuming alcohol at fraternity houses to potentially riskier places off-campus where behavior is less regulated by the university. As a result, the net effect of moratoriums remains ambiguous.

In this paper, I estimate the causal effect of 44 fraternity moratoriums across 37 universities over a six-year period (2014-2019) on university police reports of alcohol offenses and sexual assaults. I use a difference-in-differences identification strategy, leveraging the variation in timing of moratoriums. Intuitively, I compare academic-calendar days (excluding summer and winter breaks) with a moratorium to academic-calendar days without a moratorium while accounting for expected differences across days of the week and different times of the year. I construct a novel data set, merging two particularly unique data sources: university-specific Daily Crime Logs, which contain the universe of all incidents of crime reported to the university police at the incident-level, and moratorium start and end dates obtained through school newspapers and public records requests.

Using these data, I find that moratoriums significantly decrease alcohol offenses campus-wide by 26%. This effect is driven by weekends (Fridays-Sundays) when college partying is most frequent and is robust across various specifications, estimation methods, and sensitivity tests. Furthermore, I find suggestive evidence that reports of sexual assaults decrease by 29% on the weekends. Both of these declines are concentrated only when a moratorium is in place, therefore suggesting that there are no persistent effects once a moratorium is lifted. In particular, the immediate and subsequent weeks following a moratorium show little evidence that alcohol offenses or sexual assaults significantly decline and this is consistent across moratoriums of different lengths.

A key distinction of this work is that I am able to closely link changes in student behavior to a campus-wide policy that directly affects college partying. As a result, this study provides further evidence that stronger sanctions on alcohol decrease the number of alcohol-related incidents in college-aged individuals, consistent with Liang and Huang (2008) who study zero-tolerance drunk driving laws. However, unlike state or federal laws, moratoriums are unique in that university officials have the power to enact them immediately and indefinitely. This makes moratoriums an appealing policy tool as university officials can implement them at times when they see fit. Moratoriums therefore represent an understudied policy lever that university officials can readily use to reduce campus-wide partying, which in-turn, may affect alcohol and sexual assault incidence.

More broadly, this paper adds to the literature in several bodies of work, the first of which is the effect of college partying. While the literature shows that college partying increases daily reports of rape and alcohol offenses when using football game variation (Lindo, Siminski, and Swensen 2018), this study more directly focuses on a policy tool that reduces college partying. I later analyze in Section 7.A whether moratoriums have mitigating effects on college partying behavior when coinciding with college football game-days, although I find no clear evidence in support of this. Second, this paper contributes to an emerging body of economic work relating to the effectiveness of university policy, and more specifically, fraternity policy. Although university policies such as academic probation (Lindo, Swensen, and Waddell 2013) and financial aid (Dynarski 2003) have been found to be effective in improving GPA and recruiting students respectively, there are only two studies as of this writing that analyze fraternity-targeted policies—both of which study the effects of deferring fraternity recruitment from freshman to sophomore year (De Donato and Thomas 2017; Even and Smith 2020). Moratoriums, in contrast, alter a university's party culture instantly, since unaffiliated undergraduates also attend fraternity parties (Harford, Wechsler, and Seibring 2002). However, as discussed in Section 5.D, the moratorium effects diminish following the first month of implementation, making them ill-suited as a long-term solution for mitigating excessive partying. Currently, only one related study has examined the relationship between fraternities and university crime (Raghav and Diette 2022), although this study focuses on how the size of a fraternity population affects campus crime rather than the effect of a typical fraternity policy. I explore a similar idea in Online Appendix D which shows suggestive evidence that universities with higher shares of fraternity members exhibit larger moratorium effects. Last, this paper adds to the literature relating to the effects of alcohol on college-aged individuals which include health effects, such as increases in mortality (Carpenter and Dobkin 2009), emergency room visits (Francesconi and James 2019), and adolescent brain development (Silveri 2012), and behavioral effects, such as increases in crime (Carpenter and Dobkin 2015) and hindering academic performance (Carrell, Hoekstra, and West 2011; Ha and Smith 2019).

This paper proceeds as follows: Section 2 discusses the background on fraternities and moratoriums. Section 3 describes the construction of the data. Section 4 describes the empirical strategy used to estimate causal effects. Section 5 presents the main results. Section 6 explores the differences in effectiveness between different types of schools and moratoriums. Section 7 analyzes possible implications. Section 8 concludes.

2 Fraternities in the US

A Fraternity Demographics and Oversight

On average, fraternities consist of students from families of higher-than-average educational attainment and income; they are predominantly white, and prior research has linked fraternity membership to positive outcomes such as increases in graduation rates (Routon and Walker 2014), future income (Mara, Davis, and Schmidt 2018), and social capital formation (Mara, Davis, and Schmidt 2018). On the other hand, fraternity members spend approximately two more hours per-week partying than non-members (Routon and Walker 2014), binge drink on approximately two additional days per-month (DeSimone 2007), and membership has been found to decrease GPA (De Donato and Thomas 2017; Even and Smith 2020). Additionally, other research finds that membership causes students to select into easier courses and complete fewer course credits (Even and Smith 2020). While not causal, there is also survey evidence that fraternity members are more accepting of sexual violence than nonmembers (Seabrook 2019) and that sorority women, who frequently interact with fraternity men, are four times more likely to be victims of sexual assault than nonmembers (Minow and Einolf 2009).

This paper focuses on the Interfraternity Council (IFC) fraternities which are a type of social fraternity. These fraternities are the most common at universities and differ from professional, academic, or service fraternities. IFC fraternities engage in philanthropy and professional development, and according to their creed, they "exist to promote the shared interests and values of our member fraternities: leadership, service, brotherhood and scholarship" (Hechinger 2017). Importantly, IFC fraternities are the fraternities that are restricted by moratoriums in the sample.

Each IFC fraternity chapter[2] has three sources of oversight: the chapter national headquarters, the parent university, and the parent university's own IFC council—a group of student representatives from each recognized IFC fraternity chapter whom regularly meet with university staff to discuss rules/boundaries. Failure to abide by the rules outlined by these overseers' policies can result in a fraternity being unrecognized by the university which is costly—a fraternity relies on the university for new students to recruit.

B Moratoriums

A moratorium is defined as a temporary ban on social events with alcohol for IFC fraternities.[3] This can include the cancellation of new member recruitment, philanthropy activities, tailgates, or third party vendor events, although the scope of restrictions differs by university. For example, some universities may allow philanthropy events provided no alcohol is present. Importantly, moratoriums differ from individual chapter suspensions. While universities may temporarily suspend individual fraternity chapters each year, moratoriums apply to all IFC fraternities. Moreover, the timing and length of a moratorium varies substantially. Figure 1 shows the start and end dates of each moratorium over time. Moratoriums in the sample can range from as short as six calendar-days to as long as 848 calendar-days.[4] Additionally, moratoriums are generally implemented due to triggering events (see Online Appendix Table E1). These events can be a prominent sexual assault allegation, a fraternity-related death (usually due to alcohol poisoning), or an extreme behavior violation.[5] Figure 2 shows the distribution of the triggering events: 19 are triggered by behavior violations, 10 by sexual assaults, nine by a fraternity-related death, and six are unspecified. As alluded to in the introduction, moratoriums are enacted across the US. Figure 3 shows the locations of the 37 universities in the sample (see Section 3.A for further details on sample construction). Most universities are located in the Midwest and South, although there are several universities from both the West and East Coast.

Moratoriums can be implemented by two sources of jurisdiction: the university or the IFC council.[6] When a moratorium is implemented by the university, the university sets the guidelines that fraternities must abide by during the moratorium. On the other hand, an IFC-implemented moratorium is student-enforced. This means that the IFC council is responsible for producing both the guidelines and oversight of the moratorium. Figure 2 shows that IFC-implemented moratoriums are less frequent (17) than university-implemented moratoriums (27), and Section 7.B explores the potential differences in oversight.

3 Data

The main analysis uses data from a variety of sources. In particular, I construct a novel data set that links incident-level crime from university police departments to fraternity moratorium dates and university characteristics over a six-year period (2014-2019).

A Sample Construction

The 37 universities in the sample have a combined 44 moratoriums in the sample period (2014-2019). These moratoriums represent any moratorium that match the following criteria: first, the moratorium must prohibit alcohol from all fraternity social events campus-wide, and second, the moratorium must be identifiable by Google/Lexis Nexis searches. Online Appendix Table E2 lists all the universities included with their corresponding moratorium dates. While there is a possibility that the sample period contains more moratoriums from other universities, the documents provided from various fraternity associations and conversations with experts in the field suggest that the sample covers the large majority.[7] Furthermore, each moratorium's start and end dates are obtained through public records requests (20%), conversations with Fraternity and Sorority Life advisers (11%), and school newspaper articles (68%). All start and end dates are verified by at least one of these sources.[8]

Daily reports of incidents are parsed from Daily Crime Logs maintained by the 37 universities' police departments resulting in approximately 500,000 distinct reports. The Daily Crime Logs are an incident-level source of information; each crime log contains the date occurred, date reported, time occurred, time reported, a short summary of the incident, the general location of the incident, and a distinct case number (see Online Appendix Figure E1 for an example). Moreover, the Daily Crime Logs contain the universe of incidents that are reported to (or by) the university-specific police department. Hence, each of the incidents listed in these documents represents incidents that occurred on or near university property.[9]

There are two main advantages of the Daily Crimes Logs over readily available crime data sources such as the National Incidence-Based Reporting System (NIBRS), Uniform Crime Reporting System (UCR), and the Campus Safety and Security Data (CSS). First, each university police department is mandated under the Clery Act to maintain and make available a Daily Crime Log. Crime logs must be kept for seven years, and therefore, only one university is missing data from a complete calendar-year.[10] Second, the Daily Crime Logs contain all daily incidences of alcohol offenses and sexual assaults reported to or by the police—the primary outcomes used in the main analysis. This is a major advantage as the UCR does not contain alcohol offenses and the NIBRS only contains alcohol violations that end in arrests. Since not all violations of underage drinking at universities result in arrests, the NIBRS data would under-report the prevalence of alcohol misuse (Bernat et al. 2014). While the CSS data includes similar information as the Daily Crime Logs, the CSS data is aggregated to the calendar-year which makes the effect of moratoriums difficult to study given their short-lived nature. See Online Appendix Table E3 for more details on the advantages of the Daily Crime Logs.

University characteristics such as total enrollment, student demographics, and academic calendars are obtained through the Integrated Postsecondary Education Data System (IPEDS) or directly from the corresponding university. However, not all academic calendars for each year in the sample are available. Therefore, only the most current academic calendar found on a university's website is utilized. To define the start of a semester, the first day of instruction is used while the finalized grade date is used to denote the end of a semester. Since there are small changes in academic calendars year-to-year, a seven-day window is subtracted from each start date and added to each end date of every semester. For instance, if a semester begins on August 20th and ends on December 16th, the sample period will be August 13th to December 23rd.

B Matching and Harmonization

One of the challenges of using the Daily Crime Logs is their uniqueness to each university. While all crime logs contain daily reports of incidents, each university police department describes their incidents differently. As such, there is a lack of harmonization between the crime logs—incidents do not have a standardized way of being reported between university police departments. To mitigate this issue, I use regular expressions to match on typical words, phrases, and abbreviations seen in each crime log for descriptions relating to alcohol offenses and sexual assaults. For each offense, I use the following definitions for matching the incident descriptions:

- **Alcohol Offense** - Any incident description that refers to a public intoxication, underage drinking, or drinking in an unlawful manner. For instance, public drunkenness, a minor in possession, and driving while intoxicated refer to each of these definitions respectively.
- **Sexual Assault** - Any incident description that refers to a sexual assault or sex crime including rape and fondling. This corresponds to the types of sex crimes that are reported in the CSS data: rape, statutory rape, incest, and fondling. However, incest sex crimes are omitted as these are infrequent and less likely to be associated between college students.

Table 1 shows the corresponding words, phrases, and abbreviations used to match each incident description to its corresponding offense. To demonstrate the accuracy of this process, Online Appendix Table E2 shows the 15 most frequent descriptions matched to each offense.

C Descriptive Statistics

Table 2 summarizes the characteristics of the 37 universities and their corresponding distribution of offenses and fraternity moratoriums. Panel A shows descriptive statistics of the universities' demographics. On average, the universities are large with total enrollment exceeding 29,000. The majority of the student body are undergraduates and 61% of them are white. Graduation rates vary widely among schools and there is substantial variation in the selectivity of each university. For instance, graduation rates and the fraction of students admitted range between 39% to 95% and 14% to 94% respectively. Moreover, IFC fraternities, on average, represent a small fraction of the undergraduate enrollment with approximately 5% of students possessing membership to an IFC fraternity. Although IFC members represent a small number of students, the universities in the sample are representative of schools with an active Fraternity and Sorority Life as shown in Online Appendix Figure E3.

Panel B displays summary statistics of the primary outcome measures: reports of alcohol offenses and sexual assaults. Each of these outcomes are measured as per-25,000 enrolled students per-academic-calendar day. Therefore, the average amount of alcohol offenses per-25,000 enrolled students in an academic-calendar day is approximately one-half.

Last, Panel C describes characteristics of the 44 moratoriums in the sample. On average, each university experiences approximately one moratorium, although some universities experience up to three. Furthermore, the moratoriums persist for an average of 64 academic-calendar days, with significant variation in the length of the moratoriums. Specifically, the minimum length of a moratorium is only six academic-calendar days while the maximum is 541. Due to this large range, it is important to note that a median moratorium lasts for approximately 46 academic-calendar days, or approximately 1.5 months.

4 Empirical Strategy

A Empirical Approach

In order to estimate the average causal effect of fraternity moratoriums on alcohol and sexual assault offenses, I estimate the following baseline difference-in-differences specification using OLS:

$$Y_{u,t} = \beta InMoratorium_{u,t} + \gamma_u + \lambda \mathbb{X}_t + \epsilon_{u,t} \tag{1}$$

where $Y_{u,t}$ is an outcome of alcohol offenses or sexual assaults per-25000 enrolled students per academic-calendar day at university u in time t. $InMoratorium_{u,t}$ is an indicator variable equal to one when university u is experiencing a moratorium at time t, γ_u is a university-specific fixed effect, $\mathbb{X}_t$ is a vector of time-varying controls that are shared across universities, and $\epsilon_{u,t}$ is the error term. The standard errors are clustered by university to account for serial correlation within each university (Bertrand, Duflo, and Mullainathan 2004). In essence, Equation 1 is comparing moratorium days to non-moratorium days within universities that have experienced, or will experience a moratorium while accounting for expected differences across universities and time.

Including university-specific fixed effects (γ_u) in the baseline model accounts for systematic differences between a university's police department, the corresponding student demographic they are policing, and overall fixed differences in incident prevalence. For instance, university police departments may have systematic differences in the frequency of reporting due to the corresponding party-culture of their university or their own policing practices/resources, which in-turn, may lead to stronger or weaker enforcement of student drinking. Hence, including university-specific fixed effects ensures that moratorium days are compared to non-moratorium days while adjusting for these expected differences in universities. Moreover, $\mathbb{X}_t$ includes day of the week, semester type (spring/fall), holiday, and academic-year controls. Day of the week controls are included to address day-to-day fluctuations, while semester controls are included to adjust for activities that vary across the year such as fraternity recruitment. Furthermore, holiday controls[11] are included since there may be less student activity on holidays and academic year controls are included due to differences in fraternity rules and guidelines between academic-years. Lastly, while not shown in Equation 1, I also control for football game-days to account for the increases in both alcohol offenses and rapes that college football games cause (Rees and Schnepel 2009; Lindo, Siminski, and Swensen 2018).[12] Taken together, the corresponding interpretation of the parameter of interest, β, is the average difference in offense $Y_{u,t}$ on moratorium days relative to non-moratorium days, conditional on the expected differences between universities, days of the week, holidays, semesters, academic-years, and football game-days.

I expand on the baseline model in Equation 1 using several other specifications to allow for more flexibility in controlling for differences between universities' academic years. In particular, I progressively add university-by-academic-year and university-by-academic-year-by-semester fixed effects to allow for several different comparisons within the model. The inclusion of university-by-academic year fixed effects allow for comparisons within university academic years while university-by-academic-year-by-semester fixed effects allows for comparisons within a semester during a university's academic year. While university-by-academic-year-by-semester fixed effects are most flexible, a large fraction (33%) of moratoriums span across multiple academic-year-semesters which leads to a small number of comparisons within each university-academic-year-semester. Moreover, as shown later in Section 5.A, including these fixed effects produce less conservative results than the inclusion of university-by-academic-year fixed effects. Because of this, the preferred specification utilizes university-by-academic-year fixed effects, although I show that the results are similar across all empirical approaches. Hence, unless otherwise noted, all analyses in this paper utilize the preferred specification which include university-by-academic-year fixed effects.

B Identification Assumptions

To estimate Equation 1 and interpret β as a casual effect of fraternity moratoriums, there are four main assumptions that need to be satisfied: the timing of fraternity moratoriums is as-good-as-random, there are no changes in reporting/policing during a moratorium, the triggering event is not changing student behavior, and moratoriums have no lasting effects.

To address the first assumption of as-good-as-random timing, I estimate a 'multiple event' event study to identify any trends prior to a moratorium. Given that each university can experience multiple moratoriums and each moratorium can have a different length, a staggered adoption event-study design is not appropriate. Therefore, to estimate the event study, I follow the guidelines outlined in Schmidheiny and Siegloch (n.d.); I generalize a classic dummy variable event study to accommodate multiple moratoriums within a university and classify the event-time (period 0) as the entire moratorium period. Therefore, period 0 represents all moratorium days within a university.

Figures 4 and 5 show the results of the 'multiple event' event study which demonstrate that there is little suggestive evidence that crime is declining prior to a moratorium. Recall that the shaded area (period 0) represents an entire moratorium period while each lead and lag represents a 14-day period prior to, or proceeding a moratorium, normalized by the 14-day period immediately proceeding a moratorium. Fourteen-day periods are chosen instead of seven-day periods to allow for more precise point estimates. Five periods before and after are estimated, but only four are included as the fifth lead and lag are binned endpoints as described in Schmidheiny and Siegloch (n.d.). The errorbars indicate 95% confidence intervals and the number of periods before and after the moratorium are chosen to give approximately a median moratorium length of days (46). In each figure, there is little visual evidence of a downward or upward trend prior to a moratorium. This is reinforced with statistically insignificant F-test showing that the three pre-periods are jointly zero at the 10% level.[13] Moreover, the results of this analysis are intuitive; moratoriums are caused by triggering events in which typical behavior is taken "too far" and are usually enacted within three days following such event,[14] thereby giving little reason to expect anticipatory effects. In addition, according to an online repository of fraternity-related deaths from journalist Hank Nuwer, there are 19 universities that experienced a fraternity-related death but *did not* undergo a moratorium in the sample period which suggests that fraternity members may not expect a moratorium even when experiencing a particularly salient act of misconduct. Taken together, there is little evidence of a decreasing crime trend prior to a moratorium.

To test the second assumption that moratoriums do not change policing or incident reporting, I conduct an indirect test which shows no significant change between reporting behavior. In particular, I test whether there are significant differences between the time incidents occur and the time they are reported during a moratorium (*reporting lags*). This test is motivated by the notion that reporting lags may be due to factors such as police force staffing or the willingness of students to report. To perform this test, I construct the proportion of offenses that are reported with a lag on a given day for each offense.[15] An offense is defined as reported with a lag if the date the incident occurred is not equal to the date the offense was reported.

Table 3 shows that there is no significant change between the proportion of crimes reported with a lag during a moratorium. As a measure of robustness, I change the definition of a lag to reflect a difference of one, three, seven, and 14 days between the date occurred and date reported.[16] Panel A shows that roughly 0.3% of alcohol offenses are reported with a one-day lag, and the change during a moratorium is insignificant. Similarly, Panel B shows no difference in lagged reporting for sexual assaults. While sexual assaults have a higher proportion of reports that are reported with a lag (1.7%), the change during a moratorium is also insignificant.

To evaluate the third assumption that the triggering event is not changing student behavior, I perform heterogeneity analysis in Section 6.B and analyze the effect of a moratorium by each triggering event. As discussed further in Section 6.B, I find that the main results are driven by moratoriums triggered by fraternity deaths. While it is plausible that the shock of a death, rather than a moratorium, contributes to behavior changes in students, I construct a sample of 15 universities that experience a fraternity-related death but no moratorium, and apply a 64-day treatment period (the average length of a moratorium) starting with the day of the death to test whether the shock of death alone affects student behavior. In doing so, I find little evidence that alcohol offenses or sexual assaults decrease due to the shock of a death—neither outcome shows a statistically significant decrease during the 64-day period following a death.

For the final assumption that moratoriums have no lasting effects, I conduct two series of analyses which are further discussed in Section 5.C. Note that Equation 1 implicitly assumes that student behavior changes only during moratoriums and that this behavior change does not persist over time. To address this, I supplement the preferred specification with a one-week lead and one-week lag to identify whether the effects of moratoriums disappear instantly once a moratorium is lifted. Additionally, I conduct an F-test on the four post-periods in the 'multiple event' event study and show that there is no significant post-trend. These results further justify the use of already-treated universities (i.e., universities that have already experienced a moratorium) as a reasonable control group—a common critique of the difference-in-differences estimator with variation in treatment timing (Goodman-Bacon 2021). Given that moratoriums show no lasting effects, an academic-calendar day without a moratorium is a good counterfactual for an academic-calendar day with a moratorium.

C Sample Challenges and Difference-in-Differences Literature

Several recent journal articles have found that using OLS in a two-way-fixed-effects (TWFE) difference-in-differences design can cause issues with the coefficient estimates in the presence of heterogeneous treatment effects between groups over time (Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2021; Athey and Imbens 2022). In particular, the coefficient on the explanatory variable for treatment is a weighted sum of average treatment effects where some of the weights may be negative. This negative weighting occurs when the two-way-fixed-effects estimator uses treated observations as controls (Goodman-Bacon 2021; Chaisemartin and D'Haultfœuille 2020; Borusyak, Jaravel, and Spiess 2022). While this paper's research design is not a typical TWFE design since the treatment can occur multiple times, each treatment has a different length, and the preferred specification uses interacted group and time fixed effects, there remains a possibility that the negative weights issue could extend to the preferred model used in this paper due to the exclusion of never-treated units. Since the new estimators proposed by Callaway and Sant'Anna (2021) and Chaisemartin and D'Haultfœuille (2020) are not suitable for this experimental design, I conduct two different series of analyses which yield consistent results with the main findings: one that analyzes a typical TWFE design in this setting using university and day-by-month-by-year fixed effects which has no negative weights (see Online Appendix B), and another that includes 14 never-treated universities to potentially reduce the occurrence of negative weights while maintaining the preferred specification (see Section 5.A).

5 Results

In this section, the estimated causal effects of a fraternity moratorium on alcohol offenses and sexual assaults are reported using OLS. Figure 6 serves as a preview of the main results and plots the distribution of differences between the number of offenses per-25000 enrolled students on moratorium days and non-moratorium days. On average, most universities observe fewer alcohol offenses and sexual assaults on moratorium days as displayed by the dashed line.

A Main Results

Table 4 reports that fraternity moratoriums result in significantly fewer alcohol offenses on university campuses and provides suggestive evidence of decreases in sexual assaults. In Column 1, the baseline specification from Equation 1 is shown. This specification includes day of the week, holiday, semester, football game-day, and academic-year fixed effects. In Columns 2 and 3, increasingly flexible fixed effects are added, with Column 2 being the preferred specification as noted in Section 4.A. Panel A shows that alcohol offenses decrease during moratorium days compared to non-moratorium days. On average, moratorium days experience between 26% and 28% fewer alcohol offenses compared to an average academic-calendar day as indicated by the point estimates in the first three columns. These estimates are statistically significant in all three specifications, emphasizing the impact of moratoriums on reducing alcohol offenses on campus. Although the point estimates on alcohol offenses are robust, the estimates on sexual assaults do not reach statistical significance in each specification. Additionally, the magnitude varies considerably, with sexual assaults showing a 14% to 20% reduction from the mean across the different estimations.

The effects of moratoriums, as shown in Table 4, Columns 4 and 5, are driven by weekends (Friday-Sunday), which aligns with the literature that most college partying occurs on weekends (Lindo, Siminski, and Swensen 2018). Columns 4 and 5 of Table 4 show the preferred specification (Column 2) separated by weekends and weekdays. During the weekends, alcohol offenses decrease by 28% relative to an average academic-calendar weekend as shown in Panel A. However, weekdays show no statistically significant decreases. Similarly, Panel B shows that sexual assaults also decrease more on the weekends than weekdays with a 29% decrease in sexual assaults relative to an average academic-calendar weekend. This decrease in sexual assaults is significant at the 10% level.

Importantly, these findings persist across various robustness and sensitivity tests. First, given the non-negative count nature of the incident data and the sensitivity of OLS estimation to outliers, Online Appendix Table E4 reaffirms the results reported in Table 4 using Poisson estimation instead of OLS. Specifically, Poisson estimation shows a statistically significant 27% and 32% average reduction in alcohol offenses and sexual assaults on the weekends, respectively. Second, to ensure that the results are not driven by a single university, Online Appendix Figures E6 and E7 show the leave-one-out coefficient estimates for each offense. In particular, 37 unique regressions are estimated for each offense, omitting one university in each iteration—all which demonstrate similar findings to the main results. Third, due to the large variation in university size, the models in Table 4 are weighted by total enrollment in Online Appendix Table E5. The weighted estimations exhibit similar results to the unweighted models with alcohol offenses and sexual assaults decreasing by 29% and 32% on the weekends respectively, while the standard errors remain similar in magnitude. Finally, recall from Section 4.C that negative weights occur in the difference-in-difference estimator when treated units are used as control groups. Given that the sample includes only treated universities, 14 additional universities that never underwent a moratorium in the period of analysis are included to potentially mitigate the negative weighting issue. This results in 51 universities for a total of approximately 75,000 academic calendar days. Each of the additional universities are chosen from the Colleges with the Best Greek Life list on Niche.com.[17] The additional universities are selected if they are regarded as a Top 50 Greek Life school.[18] Fourteen of these universities are already included in the sample due to experiencing a fraternity moratorium, further justifying the remaining 36 Top 50 Greek Life universities as a good counterfactual. However, only 14 of these universities are included in the sample while the remaining 22 are excluded since they are unable to provide Daily Crime Logs. Online Appendix Figures E8 and E9 show the effect of moratoriums when including these never-treated universities (see *Main Sample + Never Treated* rows). Overall, the results remain similar, with weekend decreases in alcohol offenses and sexual assaults of approximately 18% and 26% respectively.

B Are There Spillovers to Nearby Areas?

One potential caveat to the main results in Table 4 is that the reported decreases in alcohol offenses and sexual assaults may be being displaced to potentially riskier areas. For instance, while campus-wide alcohol is decreasing, it may be that fraternity members and other students are substituting their behaviors on-campus to off-campus areas that are less regulated. If this is true, the net effect of a moratorium may be worse than never implementing a moratorium. Unfortunately, there does not exist a perfect data source to explore such mechanism directly; the National Incidence-Based Reporting System (NIBRS) only reliably covers 24 percent of the sample universities' neighboring police departments and includes only alcohol arrests rather than all incidents.[19] Furthermore, the Campus Safety and Security (CSS) data, while containing all incidences of crime reported on university campuses, is aggregated to the yearly level.

Despite these challenges, two sets of analyses are performed using these data. First, to identify whether crime incidence is displaced into nearby areas, I use the NIBRS data to compare the reported incidence of crimes at nearby police departments to the crimes reported at university-specific police departments using the Daily Crime Logs. Nearby police departments are defined as police departments that serve the surrounding area, but are not affiliated directly with a university.[20] This results in a comparison of nine university police departments from the Daily Crime Logs and their corresponding nearby police departments from the NIBRS. To harmonize the NIBRS data with the Daily Crime Logs, I define each offense from the NIBRS as per-25000 enrolled students at the corresponding university and limit the panel to only academic-calendar days. Both alcohol offenses and sexual assaults are restricted to incidences involving college-aged individuals (17-22), although the results are consistent when broadening the definition to include all ages. Moreover, I define sexual assaults in the NIBRS data to include fondling, rape, and sexual assault with an object to align with the definition using the Daily Crime Logs.

In both Panels A and B of Table 5, alcohol offenses and sexual assaults have an insignificant and negative point estimate at nearby police departments, thereby showing little evidence of substantial spillovers. Reassuringly, the university-specific police departments continue to show large and significant effects of the moratorium for alcohol offenses despite being a small subset of the main sample. These results give weight to the interpretation that moratoriums are decreasing the number of alcohol offenses on university campuses and students are not moving their risky behaviors to off-campus areas that are less regulated by the university.

As the second set of analysis, I analyze the CSS data to examine if students substitute partying at fraternity houses to different on-campus locations during moratoriums. The CSS data contains all disciplinary actions and arrests corresponding to liquor law violations in addition to reports of sexual assaults that occur in a calendar-year. The main advantage of using the CSS data is that it delineates between crimes that occur within a residence hall or a different on-campus location. Moreover, the CSS data includes liquor violations that may not have been reported to the university police (thus not in the Daily Crime Logs) if they were handled internally by university staff. For instance, if a liquor violation occurs in a residence hall, this citation will be absent from the Daily Crime Logs if it is handled only by the residence hall staff.[21] Therefore, on average, the Daily Crime Logs contain approximately 30% and 50% of the yearly alcohol offenses and sexual assaults reported in the CSS data respectively. However, recall that the biggest disadvantage to this data is the aggregation of all incidents to the calendar-year level. Since moratoriums can last for as few as six days and can continue through multiple calendar-years, this analysis should be taken only as speculative, not causal. See Online Appendix C for a more detailed discussion of the CSS data and the corresponding model.

Using the CSS data, there is evidence that moratoriums move drinking from fraternity houses to residence halls. Residence halls show a 0.270 *increase* in yearly alcohol offenses for each additional moratorium day in a calendar-year. Interestingly, this is accompanied by a 0.033 *decrease* in yearly residence hall sexual assaults. Although these results appear counterintuitive given the literature documents that alcohol offenses and sexual assaults tend to coincide (Lindo, Siminski, and Swensen 2018), these results point to the possibility that moratoriums cause a substitution effect of partying behavior; students substitute drinking at fraternity houses to residence halls. Residence halls, unlike fraternity houses, are far more regulated, contain university staff, and potentially have more sober bystanders to intervene if behavior appears to be escalating dangerously. Taken together, these results support the notion that *if* moratoriums displace dangerous alcohol-fueled behavior, they displace it to *less* risky areas.

C Do Moratoriums Have Long-run Effects?

Although moratoriums clearly impact student behavior when implemented, I find no evidence showing that moratoriums provide long-run impacts. In this subsection, I perform two analyses to demonstrate this: first, I conduct an F-test on the lagged coefficients in the event study specification shown in Section A, and second, I extend the preferred specification from Table 4 with an indicator for the one-week before and one-week after a moratorium.

Table 6 reports the results of the first set of analysis which fails to show significant evidence of long-run effects. Panel A includes results from the event study estimation shown in Figures 4 and 5. In addition, p-values from joint F-tests on the four lagged coefficients are reported. The p-values for both alcohol offenses and sexual assaults are above the 10% level of significance, therefore showing little evidence that the effect of the moratorium persists in the four 14-day periods (56 total days) following a moratorium.

While the sample does not collectively exhibit long-run effects, there is potential that longer moratoriums may induce more behavior change than relatively shorter ones. To study this possible heterogeneity, I supplement the analysis above by splitting the sample into quantiles based on the length of a moratorium. Each quantile represents universities with a moratorium less than 32 academic-calendar days (quantile 1), between 33 and 59 academic-calendar days (quantile 2), and more than 60 academic-calendar days (quantile 3).[22] Panel B of Table 6 shows the p-values corresponding to the F-tests on the four lagged coefficients for both alcohol offenses and sexual assaults. Similar to Panel A, there is no statistical significance across each test. Interestingly, there does appear to be evidence that moratoriums with lengths between 33 and 59 days (quantile 2) have the largest instantaneous effect, therefore showing that the length of a moratorium may be crucial to the overall effectiveness (see Section 5.D).

Last, Figure 7 reports the estimates from the second analysis which extends the specification in Column 2 of Table 4 with an indicator variable for the one-week after and one-week before a moratorium. When considering the entire sample, each offense exhibits decreases that persist only during the moratorium period and instantaneously return to previous levels in the week following a moratorium. This pattern persists when restricting the sample to weekends where the effects of the moratorium are most prominent.

D Are Moratoriums Effective Across the Entire Duration?

Although moratoriums can reduce alcohol offenses, it is likely that the reductions are not consistent throughout the enforcement period. For instance, students may find alternative ways to party or enforcement may become less strict as the moratoriums continues. Therefore, it is crucial to understand both when and how long a moratorium is most effective, as this can aid school administrators in making informed decisions about future moratorium lengths.

To understand the progression of a moratorium's effectiveness, I split the $InMoratorium_{u,t}$ treatment variable into weekly bins for the first nine weeks of a moratorium and pool the remaining weeks into one bin (Moratorium Weeks 10+) as shown in Panel A of Figures 8 and 9.[23] This amounts to 10 unique coefficients, each identifying the effect of the moratorium in the corresponding week. However, since moratorium lengths differ by university, each point estimate is identified by a different number of schools as shown in parenthesis on the x-axis. For example, the coefficient identifying the effect of a moratorium in Week 3 is identified by 33 universities that have a moratorium that reach the three-week length. Note that if a university has, for instance, a 22-day moratorium, this moratorium will contribute only one day to the identification of the Moratorium Week 4 coefficient.

Panel A of Figures 8 and 9, exhibit evidence that moratoriums are most effective in the first five weeks. In Panel A of Figure 8, alcohol offenses show statistically significant declines at the 5% level in weeks one, two, and five of a moratorium. The effectiveness appears to trend upward after the fifth week, thereby suggesting that moratorium effectiveness may diminish over time. Similarly, sexual assaults show statistically significant declines in weeks one and three in Panel A of Figure 9, while the effects appear to fade in later weeks.

Although Panel A illustrates the by-week effect, it is possible that the significant declines in the first five weeks are driven by universities that have short moratoriums. To ensure that the trends are consistent across universities, I re-estimate the coefficients using only universities that have moratoriums longer than nine weeks in Panel B of Figures 8 and 9. In each figure, Panel B shows similar trends to Panel A, although less precise due to the loss of power. The results suggest that long moratoriums exhibit the strongest effects during the initial weeks of implementation, and similarly, the effects diminish after approximately five weeks.

6 Heterogeneity

A Do Party Schools Exhibit the Strongest Effects?

Universities that have a reputation for partying may be more impacted by the restrictions of moratoriums than universities that party less. For example, past literature finds that party schools exhibit two times the increase in reports of rape on football game days than non-party schools (Lindo, Siminski, and Swensen 2018). To examine this possibility, I use Niche.com's Top Party Schools in America list.[24] The list assigns "party scene" scores based on criteria such as athletic department revenue, fraternity and sorority life statistics, access to bars, and student surveys. Using this list, a university is defined as a party school if it appears in the top 50 rankings. This amounts to 16 of the 37 universities in the sample being classified as a party school.

As shown in Table 7, universities defined as party schools have higher averages of alcohol offenses assaults relative to non-party schools. In particular, non-party schools experience approximately 52% less alcohol offenses on average. These differences are similar when excluding moratorium days (53%), although both party schools and non-party schools have relatively similar levels of sexual assault.

Table 7 also shows that party schools exhibit larger decreases in alcohol offenses than non-party schools during moratoriums. The point estimates in Panel A indicate that moratoriums decrease alcohol offenses on academic-calendar days by approximately 33% from the mean for party schools and 16% for non-party schools. Importantly, only the point estimates for party schools are statistically significant, thereby suggesting that the effects of the moratorium are driven by schools that have a stronger party culture.

Similarly, Online Appendix D explores whether universities that have relatively more *fraternity* life—defined by the fraction of undergraduate students enrolled in an IFC—display larger effects. By supplementing the preferred specification with an interaction between the $InMoratorium_{u,t}$ treatment variable and the fraction of undergraduates in an IFC fraternity, there is evidence that universities with more fraternity life show larger declines in alcohol offenses during moratoriums. Although statistically insignificant, the point estimates are negative, thereby hinting at the possibility that moratoriums are more effective when a higher fraction of students are directly affected by its guidelines.

B Does the Triggering Event for a Moratorium Matter?

As described in Section 2.B, moratoriums can be the result of a fraternity-related death, a prominent sexual assault, or a behavior violation. Given the differing salience of these events, it is possible that a triggering event affects a moratorium's effectiveness. As an illustration, a death may be more salient than a behavior violation, resulting in a behavior shock to the college campus. Moreover, both deaths and sexual assaults are exceptionally undesirable results of risky behavior—a moratorium may seem more justified under these outcomes rather than following an instance of hazing.

Figure 10 reports that moratoriums have a stronger impact when triggered by a death or sexual assault, rather than a behavior violation. Specifically, alcohol offenses decrease notably when a fraternity-related death is the trigger. To confirm that this effect is caused by the moratorium rather than the triggering death, I analyze data from 15 additional universities that had a fraternity-related death in the sample period, but did not have a moratorium.[25] Hence, these supplemental universities serve as a control group to observe the effect of a fraternity-related death without the influence of a moratorium. I exclusively analyze data from these 15 universities that did not have a moratorium by creating a 64-day binary treatment variable (i.e., the average length of a moratorium) beginning with the date of the death. Next, I estimate the preferred specification using the 64-day period after the death instead of the $InMoratorium_{u,t}$ treatment variable. Panel C of Online Appendix Figures E8 and E9 show that there is little evidence of declines in alcohol offenses or sexual assaults following a fraternity-related death without a moratorium. The point estimates for alcohol offenses are consistently positive, while both offenses exhibit statistically insignificant estimates at the 10% level. To increase precision, I supplement this analysis in Panel D of Online Appendix Figures E8 and E9 by including the 14 never-treated schools that are used in Section 5.3 as never-treated controls. This amounts to 29 universities, 15 of which undergo the effect of a death, and 14 of which receive no such treatment. As shown, the point estimates remain consistent across both of these analyses and the statistical significance does not change. Taken together, there is little evidence suggesting that a fraternity-related death contributes to the decreases shown in alcohol and sexual assault offenses during a moratorium. Instead, this points to the possibility that students may more seriously abide by the moratorium guidelines when the triggering event is a death.

Additionally, Figure 10 shows significant decreases in sexual assaults when a triggering event involves either a sexual assault or behavior violation. However, the persistent shortcomings of estimating effects on sexual assaults, such as the under-reporting issue, may be exacerbated in this analysis since these estimates are based on a small subset of universities (19 universities for behavior violations and 10 for sexual assaults).[26] Consequently, although the results indicate evidence of decreases in sexual assaults, this evidence is mostly speculative under the data limitations.

7 Discussion

A Do Moratoriums Mitigate the Effects of Football Games?

It is well-documented in the literature that college football games cause higher rates of alcohol offenses and rape (Rees and Schnepel 2009; Lindo, Siminski, and Swensen 2018). While football games cause negative outcomes, universities are reluctant to suspend football games—college football is popular among students and alumni in addition to being a major source of revenue. Therefore, finding an effective policy that can mitigate the detrimental effects of football games while maintaining the benefits is important for university administrators. This subsection analyzes whether moratoriums are the policy tool that can accomplish this.

Figure 11 shows that football game-days cause a significant increase in the number of alcohol offenses and sexual assaults. These effects are identified by 34 of 37 universities that have football teams in the sample, resulting in over 2000 football games. Each of these effects is larger on home games rather than away games which is consistent with Lindo, Siminski, and Swensen (2018) and Rees and Schnepel (2009). Furthermore, Figure 11 also shows the combined effect of a game day and a moratorium. In each of these estimations, the point estimates are similar to the effect of game-days only, although less precise. This may be caused by a lack of identifying variation—the estimates are identified by 89 occurrences of game days that coincide with moratoriums. As a robustness check, I broaden the definition of game-days to game-weekends in Online Appendix Figure E10. Although this nearly triples the amount of identifying variation, the results are consistent.[27] Considering these results, it is uncertain whether moratoriums mitigate the effects of game-days. On one hand, these results offer the possibility that fraternities are not an integral component to college partying on game-days—students can substitute away from fraternity parties to other alternatives such as tailgates. On the other hand, it may be that moratoriums restrict the amount of dangerous partying that occurs during football games and produce a safer environment. Since the estimates are imprecise, it is unclear whether moratoriums can act as an effective policy tool to mitigate the undesirable effects of football game-days.

B Who Should Enforce Moratoriums?

Recall from Section 2.B that there are two sources of enactment/oversight for campus-wide moratoriums—the university itself and the university-specific IFC council. In the sample, 27 of the 44 (61%) moratoriums are enacted by a university. There is reason to suspect differences between these two sources of jurisdiction since IFC moratoriums may lack the incentive structure that university moratoriums have. For instance, a university can permanently suspend a fraternity chapter from its campus for failure to abide by moratorium guidelines which may damage the fraternity chapter's membership and reputation. On the other hand, IFC councils have little incentive to permanently suspend or impose additional sanctions as fraternity chapters rely on each other to maintain their community life. As such, further disciplinary measures by the IFC-council directly affect the council members themselves, thus creating a system that may incentivize IFC council members to look away from the moratorium guidelines.

In Table 8, the coefficient estimates on alcohol offenses show suggestive evidence of a decline when a university imposes the moratorium as shown in Panel A. Consistent with the main results, the largest effects are on weekends rather than weekdays. However, in Panel B, the coefficient estimates for sexual assaults are insignificant across both university-imposed and IFC-enacted moratoriums, likely due to the infrequent reporting of sexual assaults. While there is no definitive evidence for differences in enforcement for sexual assaults, the significant declines in the number of alcohol offenses point to the university administration as the more effective enforcement body rather than the fraternity members themselves.

8 Conclusion

In this paper, I estimate the causal effect of temporary restrictions of fraternity social events with alcohol (*moratoriums*) on campus-wide reports of alcohol offenses and sexual assaults across 37 universities in the US. I construct a novel dataset which includes daily-level incident reports from each university police department. Using these data, I compare academic-calendar days with a moratorium to academic-calendar days without a moratorium while controlling for expected differences in the days of the week, holidays, semesters, academic years, football game-days, and universities. I find that moratoriums decrease the average reports of alcohol offenses on a given academic calendar day by approximately 26%. This result is most prominent on the weekends when partying is most frequent (28% reduction) while nonexistent on the weekdays. Importantly, there is not substantial evidence that moratoriums displace crime to nearby areas. Moreover, I find suggestive evidence of decreases in reports of sexual assaults on the weekends by 29%, although only significant at the 10% level. Notably, moratoriums show no lasting effects, and this result is consistent across moratoriums of shorter and longer lengths. Taken together, these results support the notion that moratoriums are only effective in temporarily reducing campus-wide crime.

Given that moratoriums are unable to create permanent changes in student behavior, it is unclear whether they are a welfare-improving policy. On one hand, moratoriums cause decreases in alcohol offenses. If these decreases are the result of a displacement effect, these offenses may be occurring in relatively safer areas (residence halls) as speculated in Section 5.B. Furthermore, moratoriums may help alleviate the detrimental health effects that alcohol causes in college students such as hindering academic performance and costly emergency room visits. On the other hand, moratoriums do not permanently change student behavior; while moratoriums are effective during the first month of enforcement, moratoriums are an unproductive policy to systematically reduce college partying behavior. Hence, school administrators should understand that moratoriums are a transient solution and should therefore seek other methods to promote long-term change. One understudied possibility is the suspension of specific misbehaving fraternity chapters from universities rather than IFC moratoriums. Although this policy alleviates the criticism that moratoriums are punishing even well-behaving fraternities, more research is needed to understand the benefits and downfalls of this practice. Specifically, it is unclear whether this truly propagates behavior change—members of a poor behaving fraternity may choose to substitute to a new fraternity and thereby negatively influence its members.

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Notes

[1]This is based on the online repository of hazing deaths from journalist Hank Nuwer. See here: https://www.hanknuwer.com/hazing-destroying-young-lives/

[2]A chapter, otherwise known as a *house*, is a unique fraternity. A fraternity can have many chapters across the US, with usually one per-school.

[3]This is the minimum requirement for a moratorium in this paper. Some universities ban alcohol at social events for all IFC fraternities in addition to the rest of their Fraternity and Sorority Life. However, IFC fraternities are generally the main focus.

[4]Note the distinction between calendar-days and academic-calendar days. A calendar-day represents the entire calendar, whereas an academic-calendar day represents only the fall/spring semesters of the university school year.

[5]A behavior violation refers to hazing, rule violations, offensive behavior, and other disorderly conduct that results in a moratorium.

[6]Note that the fraternity's chapter headquarters cannot impose a moratorium. Since chapter headquarters are unique to a fraternity chapter, they only have jurisdiction over one specific fraternity.

[7]See Online Appendix A for a discussion of known universities that experienced moratoriums but are excluded.

[8]There is one exception to this which is the first moratorium at San Diego State University. While the start date has been verified by a newspaper article, the exact end date is ambiguous. However, evidence shows that the moratorium ended before the start of the 2015 spring semester, and hence, this is the date used in the analysis. The newspaper article showing this evidence can be seen here: https://newscenter.sdsu.edu/sdsu_newscenter/news_story.aspx?sid=75357.

[9]Sometimes, university police may respond to calls slightly outside of university property. Based on conversations with university police, this is usually when a student is involved.

[10]See Online Appendix Section B for more details.

[11]Holiday controls include indicators for Veterans Day, Thanksgiving, Labor Day, Halloween, and MLK Day. Christmas/New Years/July 4th are not included since no university's academic-calendar contains them.

[12]Information on football game dates and locations are found using sports-reference.com and espn.com. In total, 34 of the 37 universities in the sample that have football teams resulting in over 2000 football games, 89 of which coincide with a moratorium.

[13]As a measure of robustness, an alternative event-study is estimated using 46-day periods before and after a moratorium in Figures E4 and E5. Each of these figures fails to show evidence of a decreasing or increasing pre-period trend.

[14]This statistic is based on 13 of the 15 universities in which I have data on date of the triggering event.

[15]Only 32 of the 37 universities had data for the date occurred of their incidents. Hence, this test only reflects a subset of the sample.

[16]Literature such as Sahay (2021) use a 3-day lag when applying this test.

[17]I use Niche.com since it is the top search result on Google when searching for the "best fraternity colleges". The Princeton Review, notable for its annual list of party schools, does not a list regarding fraternity life.

[18]Notably, it is known that at least one university (Chico State) had a moratorium outside of the sample period (2013). This, however, only further validates the selection of the never-treated universities.

[19]In this case, I consider a data source to be reliable if reporting of crime is consistent in the sample period. NIBRS features only nine schools that continually report data without large missing periods.

[20]The neighboring police departments were identified using Lindo, Siminski, and Swensen (2018) public access data files in addition to Jacob Kaplan's NIBRS data tool available here: https://jacobdkaplan.com/nibrs.html#state=Colorado&agency=Denver%20Police%20Department&category=murder_nonnegligent_manslaughter&rate=false

[21]Similarly, if a student tells a school counselor of a sexual assault, that sexual assault may not necessarily be reported to the university police and thus not appear in the Daily Crime Logs. However, this is mandated to be included in the CSS data.

[22]Note that six universities have more than one moratorium and can therefore be included in multiple quantiles. This occurs for five of the six universities. However, this represents a small fraction within each quantile: quantile 1 (20%), quantile 2 (23%), and quantile 3 (26%)

[23]Note that nine weeks is approximately the average length of a moratorium.

[24]I use Niche.com over the Princeton Review since the Princeton Review no longer posts their party school rankings. For more details on the methodology see: https://www.niche.com/about/methodology/top-party-schools/.

[25]These universities were found using Hank Nuwer's repository of hazing-related deaths in the US: https://www.hanknuwer.com/hazing-deaths/.

[26]Survey evidence shows that nearly 80% of sexual assaults go unreported. This is based on statistics from the AAU Campus Climate Survey on Sexual Assault and Sexual Misconduct. See here: https://ira.virginia.edu/sites/ias.virginia.edu/files/University%20of%20Virginia_2015_climate_final_report.pdf

[27]Not all game-days occur on a weekend, so the expanding the definition to a game-day weekend does not quite triple the number.

10 Tables

Table 1: Words and Phrases used to Pattern Match on Offenses of Interest

Outcome	Words to Match
Alcohol Offense	alcohol, dwi, intox, drink, dui, drunk, liquor, driving under the influence, dip, abcc, underage, dwi, underage, pula, owi, mip, under age, beer, wine, booze, minor in possession, ovi
Sexual Assault	sex, rape, fondling, fondle

Note:

The second column represents a portion of an incident's description to pattern match on. Words for alcohol violations and sexual assaults are found by reading each university's dataset for common words within incident descriptions. For example, the word 'sex' will match on 'sexual assault' and 'sex offense' since 'sex' appears in each of these descriptions. Notably, this method likely undercounts the true number of violations in each police department's Daily Crime Log due to spelling errors. As a demonstration, the word 'alcohol' may be written as 'aclohol' which this matching process will not include. Some notable abbreviations include the following:

'dwi' is an abbreviation for 'driving while intoxicated'.

'dip' is an abbreviation for 'drunk in public'.

'abcc' is an abbreviation for 'alcohol beverage control comission'.

'pula' is an abbreviation for 'possession under legal age'.

'owi' is an abbreviation for 'operating while intoxicated'.

'mip' is an abbreviation for 'minor in possesion'.

'ovi' is an abbreviation for 'operating vehicle intoxicated'.

Table 2: Summary Statistics of the Universities in the Sample

	Mean	SD	Median	Min	Max
Panel A: University Characteristics					
Total Enrollment	29,074	14,423	28,718	3,127	69,402
Total Undergraduate Enrollment	22,417	11,878	22,309	2,571	59,371
Fraction Asian	0.07	0.08	0.04	0.01	0.36
Fraction Black	0.07	0.04	0.06	0.01	0.20
Fraction Hispanic	0.13	0.14	0.07	0.02	0.68
Fraction White	0.61	0.18	0.67	0.08	0.83
Graduation Rate	70.33	13.78	70.00	39.00	95.00
SAT Math 75th Percentile	655.79	69.11	650.00	480.00	790.00
SAT Reading 75th Percentile	641.26	54.25	640.00	490.00	760.00
Fraction Admitted	0.60	0.21	0.61	0.14	0.94
Fraction Private	0.13	0.34	0.00	0.00	1.00
Fraction IFC Fraternity[a]	0.052	0.025	0.049	0.011	0.113
Panel B: Daily Crime Log Offenses					
Alcohol Offense	0.46	1.23	0.00	0.00	31.68
Sexual Assault	0.05	0.30	0.00	0.00	15.99
Panel C: Moratorium Characteristics					
Number of Moratoriums per-University	1.36	0.61	1.00	1.00	3.00
Length of Moratoriums	64.07	80.90	45.50	6.00	541.00
Total Number of Universities	*37*				

Note:
Offenses are per-25000 students enrolled per-academic calendar day. Length of moratorium statistics are in academic-calendar days. Number of moratoriums refers to number of moratoriums only within the 2014-2019 time period. Some schools may or may not have had moratoriums in periods before or after the time period of analysis. Only a subset of races are chosen, and hence, the fractions do not sum to 1 in the table. SAT Math 75th Percentile and SAT Reading 75th Percentile correspond to the 75th percentile SAT score for an admitted student. A perfect score is 800, while an average score is approximately 500. Fraction Private refers to the fraction of universities that are private universities.

[a] This is defined as the number of IFC members divided by the total undergraduate enrollment. However, in the case of four universities, counts had to be obtained from year 2022 due to lack of data availability within departments. Note that IFC fraternity populations do not change substantially year-to-year.

Table 3: Effect of Moratoriums on Changes in Reporting (OLS)

	Reporting Lag			
	More than 1-Day Lag	More than 3-Day Lag	More than 7-Day Lag	More than 14-day Lag
	(1)	(2)	(3)	(4)
Panel A: Proportion of Alcohol Offenses Reported with Lag				
In Moratorium	0.002	0.000	0.000	0.000
	(0.002)	(0.001)	(0.001)	(0.001)
Observations	48026	48026	48026	48026
Mean of Dependent Variable	0.003	0.002	0.001	0.001
Panel B: Proportion of Sexual Assaults Reported with Lag				
In Moratorium	-0.001	-0.003	-0.001	0.000
	(0.004)	(0.004)	(0.004)	(0.003)
Observations	48026	48026	48026	48026
Mean of Dependent Variable	0.017	0.014	0.011	0.001

Note:
Standard errors are clustered by university. Panels A and B are OLS regressions of proportions of alcohol offenses and sexual assaults reported with a reporting lag. A reporting lag is defined as an offense that was reported more than one (Column 1), three (Column 2), seven (Column 3), or 14 (Column 4) days after it occurred. 32 of the 37 universities have information on date occurred. Specification is the preferred specification which includes day of week, holiday, football game-day, semester, and university-by-academic-year fixed effects. See Table 4 Column 2 for more details on the preferred specification.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 4: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults (OLS)

				Specification (2)	
				Weekends	Weekdays
	(1)	(2)	(3)	(4)	(5)
Panel A: Alcohol Offenses					
In Moratorium	-0.125**	-0.123**	-0.131***	-0.238**	-0.038
	(0.047)	(0.051)	(0.046)	(0.106)	(0.026)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.464	0.464	0.464	0.828	0.190
Wild Bootstrap P-Value	0.004	0.010	0.006	0.012	0.179
Panel B: Sexual Assaults					
In Moratorium	-0.009**	-0.010	-0.007	-0.017*	-0.004
	(0.004)	(0.006)	(0.006)	(0.010)	(0.006)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.049	0.049	0.049	0.058	0.042
Wild Bootstrap P-Value	0.014	0.149	0.246	0.094	0.518
FE: Day of Week	X	X	X	X	X
FE: Holiday	X	X	X	X	X
FE: Game Day	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X		X	X
FE: University	X				
FE: Academic Year	X				
FE: University by Academic Year		X		X	X
FE: University by Academic Year by Semester			X		

Note:
Estimates are obtained using OLS. Standard errors shown in parenthesis are clustered by university (37 clusters) and each offense is defined as per-25000 enrolled students. P-values from 1000 wild cluster bootstrap iterations are shown for the In Moratorium coefficient as suggested by Cameron, Gelbach, and Miller (2008) in cases with a small number of clusters (typically lower than 30). This analysis is near, but not below this threshold. Game Day controls consist of university football games within each university. Weekends include Friday-Sunday while Weekdays include Monday-Thursday. Column 2 is the preferred specification due to the flexibility of the fixed effects and the conservativeness of the estimates. Significance stars correspond to clustered standard errors.
* p < 0.1, ** p < 0.05, *** p < 0.01

Table 5: Effect of Moratoriums in Local Police Departments Compared to University Police Departments (OLS)

	Nearby Police Departments			University Police Departments		
	All Days (1)	Weekends (2)	Weekdays (3)	All Days (4)	Weekends (5)	Weekdays (6)
Panel A: Alcohol Offenses						
In Moratorium	-0.156	-0.201	-0.126	-0.320*	-0.714**	-0.029
	(0.130)	(0.206)	(0.114)	(0.141)	(0.290)	(0.040)
Observations	13764	5898	7866	13743	5889	7854
Mean of Dependent Variable	1.225	1.930	0.696	0.754	1.403	0.267
Panel B: Sexual Assaults						
In Moratorium	-0.025	-0.011	-0.035	-0.003	-0.013	0.004
	(0.016)	(0.017)	(0.021)	(0.017)	(0.029)	(0.013)
Observations	13764	5898	7866	13743	5889	7854
Mean of Dependent Variable	0.478	0.522	0.446	0.055	0.071	0.043
FE: Day of Week	X	X	X	X	X	X
FE: Holiday	X	X	X	X	X	X
FE: Game Day	X	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X	X	X	X	X
FE: Agency by Academic Year	X	X	X			
FE: University by Academic Year				X	X	X

Note:
The columns under Nearby Police Departments use the NIBRS data which pertains to police departments that are closest to the university. University Police Departments uses the Daily Crime Log data set which contains only university-specific police departments. Only 9 local police departments in the NIBRS data consistently report in the sample period. This table represents the comparison of alcohol offenses and sexual assaults per-25000 enrolled students at the nine local police departments and the corresponding nine universities. Standard errors are clustered by agency for NIBRS data and by university for Daily Crime Log data.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Absence of Long-Run Effects of Moratoriums Split by Moratorium Length (OLS)

	Dependent Variable	
	Alcohol Offenses	Sexual Assaults
	(1)	(2)
Panel A: Full Sample		
Estimates from Figures 4 and 5		
In Moratorium	-0.137**	-0.015
	(0.059)	(0.010)
Observations	55115	55115
F-test P-value of Lags	0.158	0.102
Panel B: Quantiles by Moratorium Length		
Moratorium Length: 1st Quantile		
In Moratorium	0.062	-0.015
	(0.036)	(0.021)
Observations	22503	22503
F-test P-value of Lags	0.459	0.070
Moratorium Length: 2nd Quantile		
In Moratorium	-0.238**	-0.021
	(0.097)	(0.012)
Observations	19241	19241
F-test P-value of Lags	0.552	0.408
Moratorium Length: 3rd Quantile		
In Moratorium	-0.128	-0.007
	(0.087)	(0.015)
Observations	22653	22653
F-test P-value of Lags	0.203	0.128

Note:
Point estimates of In Moratorium reflect the time 0 for the 'multiple event' event studies similar to Figures 4 and 5 with four leads and four lags of 14-day bins. Each offense is defined as per-25,000 enrolled students. Standard errors are clustered at the university level. All periods are normalized by the 14-day period before the moratorium. Panel A represents the same coefficient estimates as Figures 4 and 5, while Panels B, C, and D represent subsets of the sample split by three quantiles. The three quantiles represent the 33rd, 66th, and 100th percentile of a moratorium length which correspond to [0-32], [33-59], and [60-541] academic calendar days of a moratorium respectively. Hence, if a university has a moratorium that lasts 30 academic calendar days, then it is included in Panel A. P-values are reported from joint F-test of the four lags. Fixed effects include day of the week, holiday, semester number, football game-day, and university-by-academic-year.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Effect of Moratoriums on Alcohol Offenses and Sexual Assault by Party School (OLS)

	School Type		
	All Schools (1)	Party Schools (2)	Non-Party Schools (3)
Panel A: Alcohol Offenses			
In Moratorium	-0.123**	-0.223**	-0.053
	(0.051)	(0.101)	(0.034)
Observations	55115	23980	31135
Mean of Dependent Variable	0.464	0.658	0.314
Non-Moratorium Mean	0.461	0.661	0.312
Panel B: Sexual Assaults			
In Moratorium	-0.010	-0.008	-0.011
	(0.006)	(0.007)	(0.010)
Observations	55115	23980	31135
Mean of Dependent Variable	0.049	0.045	0.052
Non-Moratorium Mean	0.049	0.045	0.052

Note:
Standard errors are clustered by university and each offense is defined as per-25000 enrolled students. The column All Schools represents the preferred specification (i.e., Column 2) from the main results table which includes day of the week, football game-day, semester number, and university-by-academic-year fixed effects. A party school classification is determined from Niche.com's list of top partying schools. A university in the top 50 is considered a party school which amounts to 16 of the 37 universities.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Effect of Moratoriums Imposed by the University vs. the IFC (OLS)

	Days of the Week		
	All Days (1)	Weekends (2)	Weekdays (3)
Panel A: University-Enacted Moratoriums			
Alcohol Offense			
In Moratorium	-0.132*	-0.252*	-0.041
	(0.065)	(0.136)	(0.035)
Observations	55115	23643	31472
Sexual Assault			
In Moratorium	-0.010	-0.019	-0.003
	(0.008)	(0.013)	(0.007)
Observations	55115	23643	31472
Panel B: IFC-Enacted Moratoriums			
Alcohol Offense			
In Moratorium	-0.101	-0.197	-0.030
	(0.082)	(0.166)	(0.026)
Observations	55115	23643	31472
Sexual Assault			
In Moratorium	-0.010	-0.014	-0.007
	(0.010)	(0.010)	(0.012)
Observations	55115	23643	31472

Note:
Standard errors clustered by university. In Panel A, the In Moratorium is interacted with an indicator variable equal to one if the moratorium was enacted by a university. In Panel B, In Moratorium is interacted with an indicator variable equal to one if the moratorium was enacted by the IFC. Controls follow the preferred specification from Column 2 in the main results table with day of week, holiday, semester, football game-day, and university by academic year fixed effects. Panel A shows the effects of a moratorium when a moratorium is imposed by the university. University-imposed moratoriums represent 27/44 (61%) of the moratoriums. Panel B shows the effects of a moratorium when the IFC council imposes the moratorium. This is a student-lead initiative. IFC-imposed moratoriums represent 17/44 (39%) of the moratoriums in the sample. Weekends represent Fridays through Sundays while Weekdays represent Mondays through Thursdays.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

11 Figures

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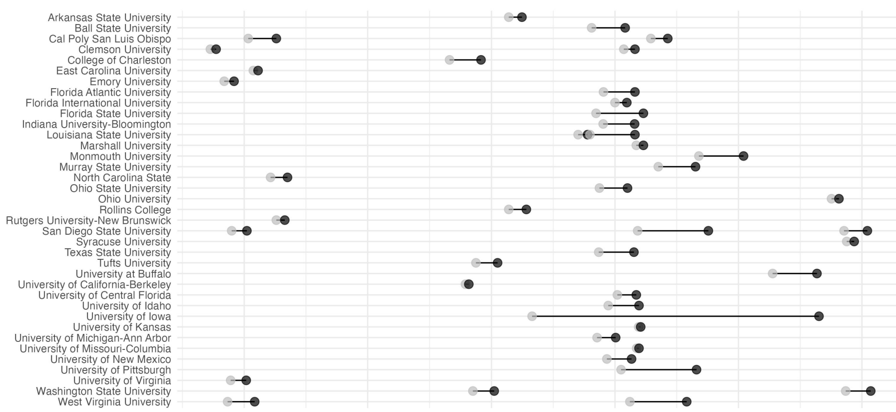

Figure 1: Distribution of Moratoriums Across the Sample Period for all Universities

Note: The sample period starts in 2014 and ends on the last day of 2019. The lengths of the moratoriums in this graph represent calendar-day lengths, not academic-calendar day lengths. Universities experience one to three moratoriums in the sample period. Note that the two moratoriums that end in the year 2020 end in January at the beginning of the semester. This short period in 2020 is not included in the sample.

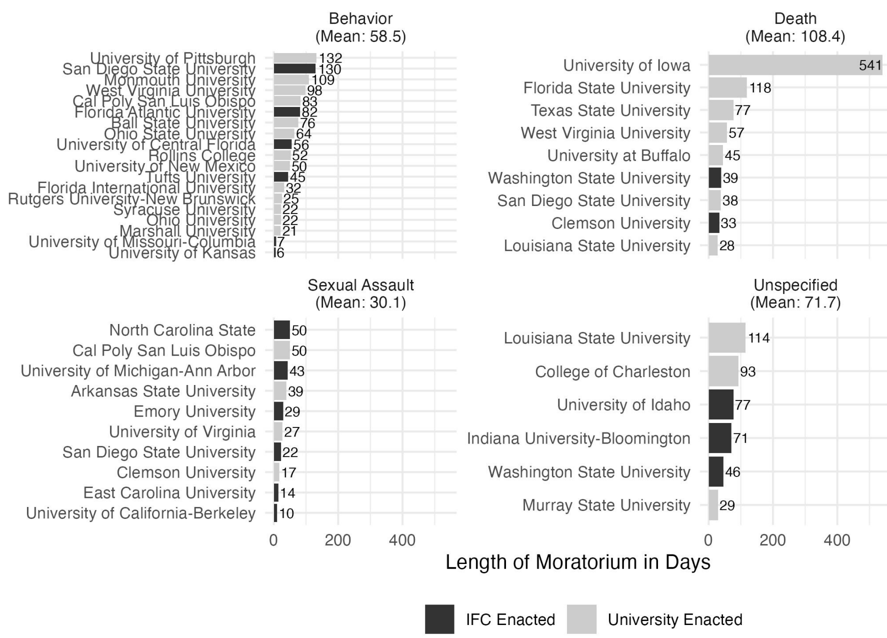

Figure 2: Number of Academic Calendar Days in Each Moratorium by Triggering Event
Note: Lengths of moratoriums represent academic calendar days. Therefore, the lengths of moratoriums differ from Figure 1. Grey shaded regions represent a moratorium that was imposed by the university, while black shaded regions represent moratoriums that were imposed by the IFC. Each of the four categories represents the event that triggered a moratorium. Behavior violations is a catchall term for hazing, rule violations, offensive behavior, and other disorderly conduct. Death relates to a fraternity-related death that triggered a moratorium. Sexual assaults relate to a sexual assault case that triggered a moratorium. Lastly, the Unspecified category represents all moratoriums in which the moratorium triggering event is unknown or unclearly defined.

Figure 3: Locations of the Universities Included in the Sample
Note: There are a total of 37 universities in the sample, five of which are private universities. Data on both geographic location and private/public entity are obtained from the Integrated Postsecondary Education Data System (IPEDS).

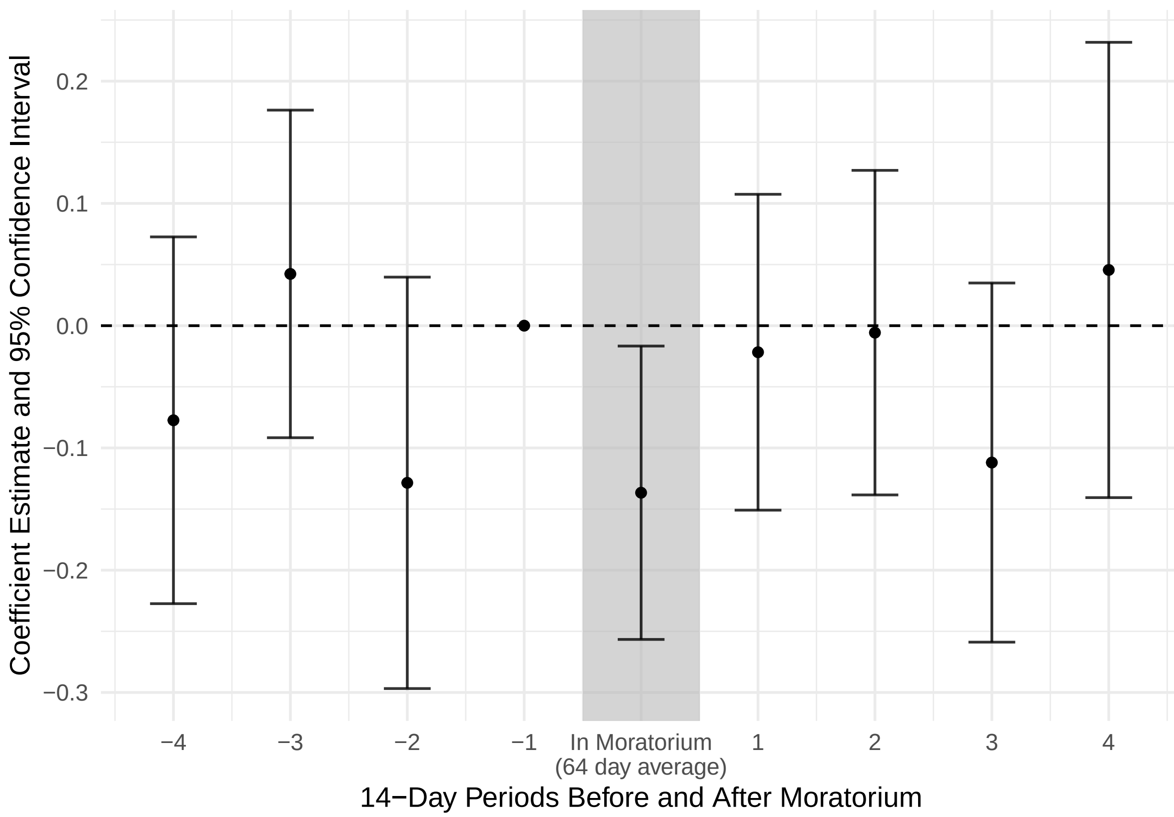

Figure 4: Event Study for Alcohol Offenses

Note: The shaded area point estimate represents an entire moratorium period for each university. Hence, the shaded area point estimate has varying amounts of days within based on the university. For instance, Arkansas State University had a 39-day moratorium and therefore their shaded area point estimate would be identified by the 39 moratorium days. Point estimates not within the shaded region are 14-day periods. Number of days within a period are chosen to give approximately a median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 14-day period before the moratorium. Alcohol offenses are defined as alcohol offenses per-25000 enrolled students. Controls include holiday, spring semester, day of the week, football game-days, and university-by-academic-year. Standard errors clustered by university. All errorbars represent 95% confidence intervals. A joint-hypothesis F-test that each of the leading periods are zero shows that the p-value is 0.27 which is statistically insignificant.

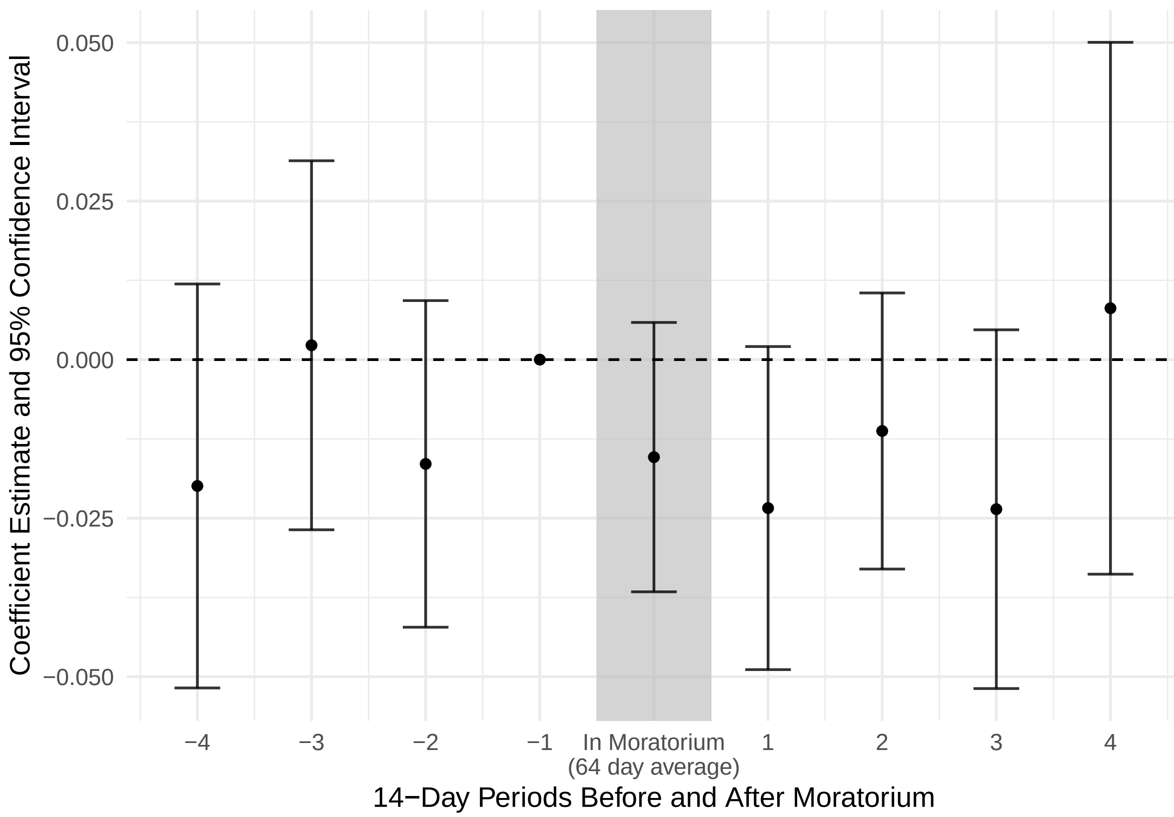

Figure 5: Event Study for Sexual Assault Offenses

Note: The shaded area point estimate represents an entire moratorium period for each university. Hence, the shaded area point estimate has varying amounts of days within based on the university. For instance, Arkansas State University had a 39-day moratorium and therefore their shaded area point estimate would be identified by the 39 moratorium days. Point estimates not within the shaded region are 14-day periods. Number of days within a period are chosen to give approximately a median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 14-day period before the moratorium. Sexual assault offenses are defined as sexual assaults per-25000 enrolled students. Controls include holiday, spring semester, day of the week, football game-day, and university-by-academic-year. Standard errors clustered by university. All errorbars represent 95% confidence intervals. A joint-hypothesis F-test that each of the leading periods are zero shows that the p-value is 0.54 which is statistically insignificant.

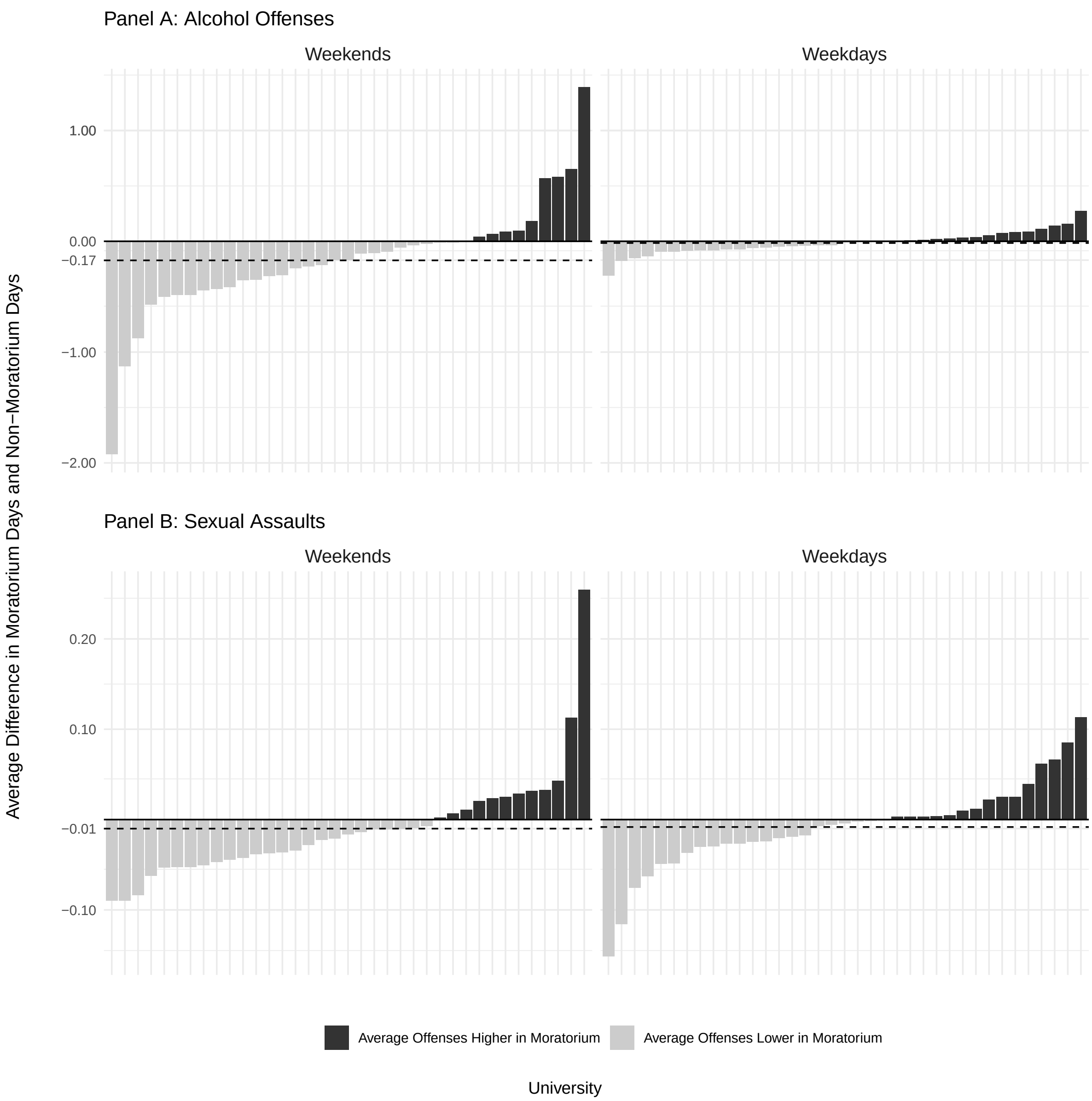

Figure 6: Difference in Average Offenses on Moratorium Days and Non-Moratorium Days
Note: The y-axis represents the average difference in offenses per-25000 enrolled students on moratorium days and non-moratorium days for each university. Negative y-axis values indicate that average offenses were lower on moratorium days than non-moratorium days. The x-axis denotes a unique university. The solid black line on the y-axis is 0, while the dashed black line denotes the average of the entire distribution.

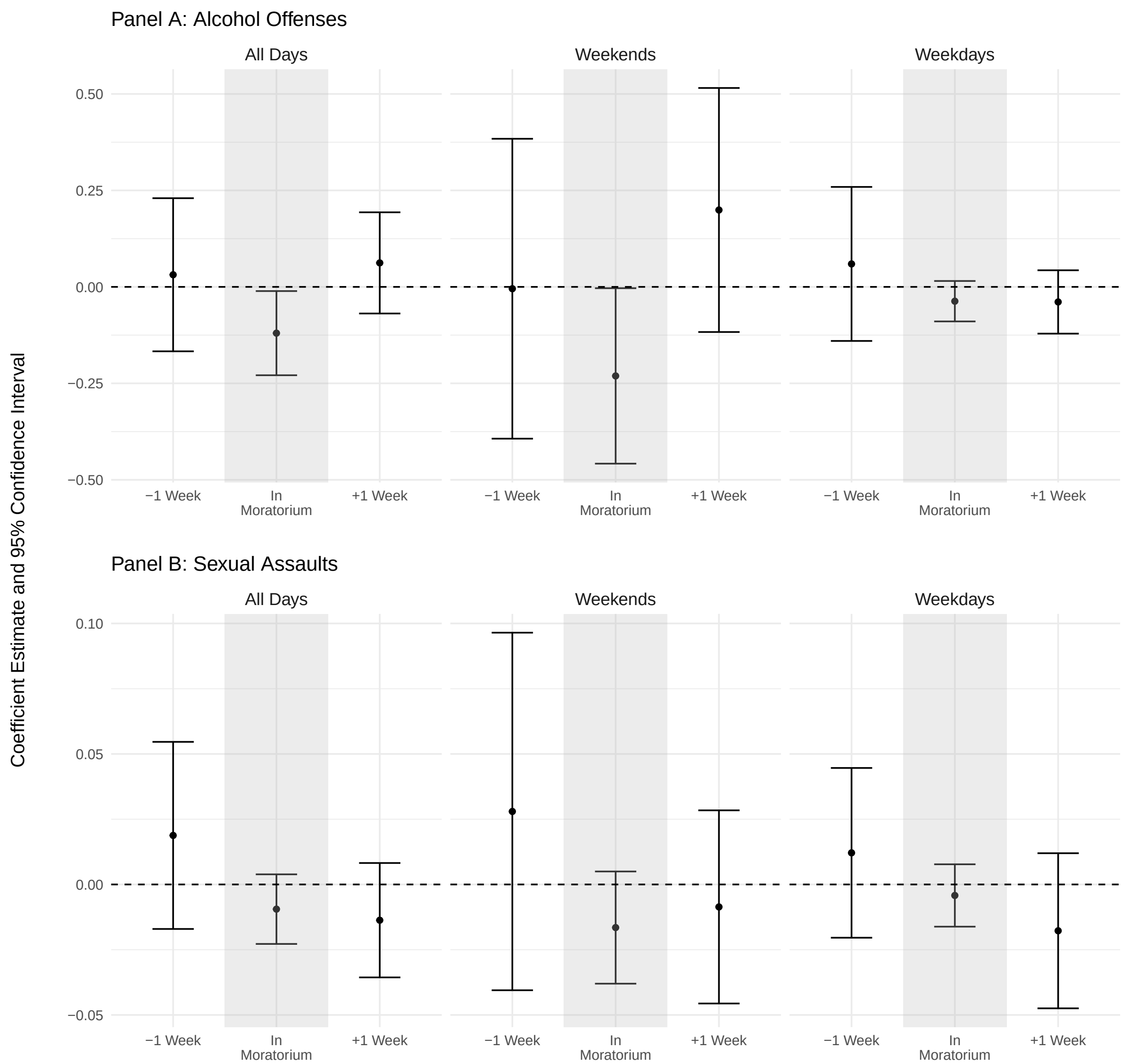

Figure 7: Coefficient Estimates Including a Week Before and Week After Indicator

Note: The x-axis represents three periods: the week before a moratorium, the moratorium itself, and the week after the moratorium. Indicators for week before and week after are added to Specification 2 from Table 4. Controls include holiday, spring semester, day of the week, football game-days, and university-by-academic-year. Standard errors are clustered by university. Weekends represent Fridays, Saturdays, and Sundays. Weekdays represent Mondays-Thursdays. Errorbars represent 95% confidence intervals.

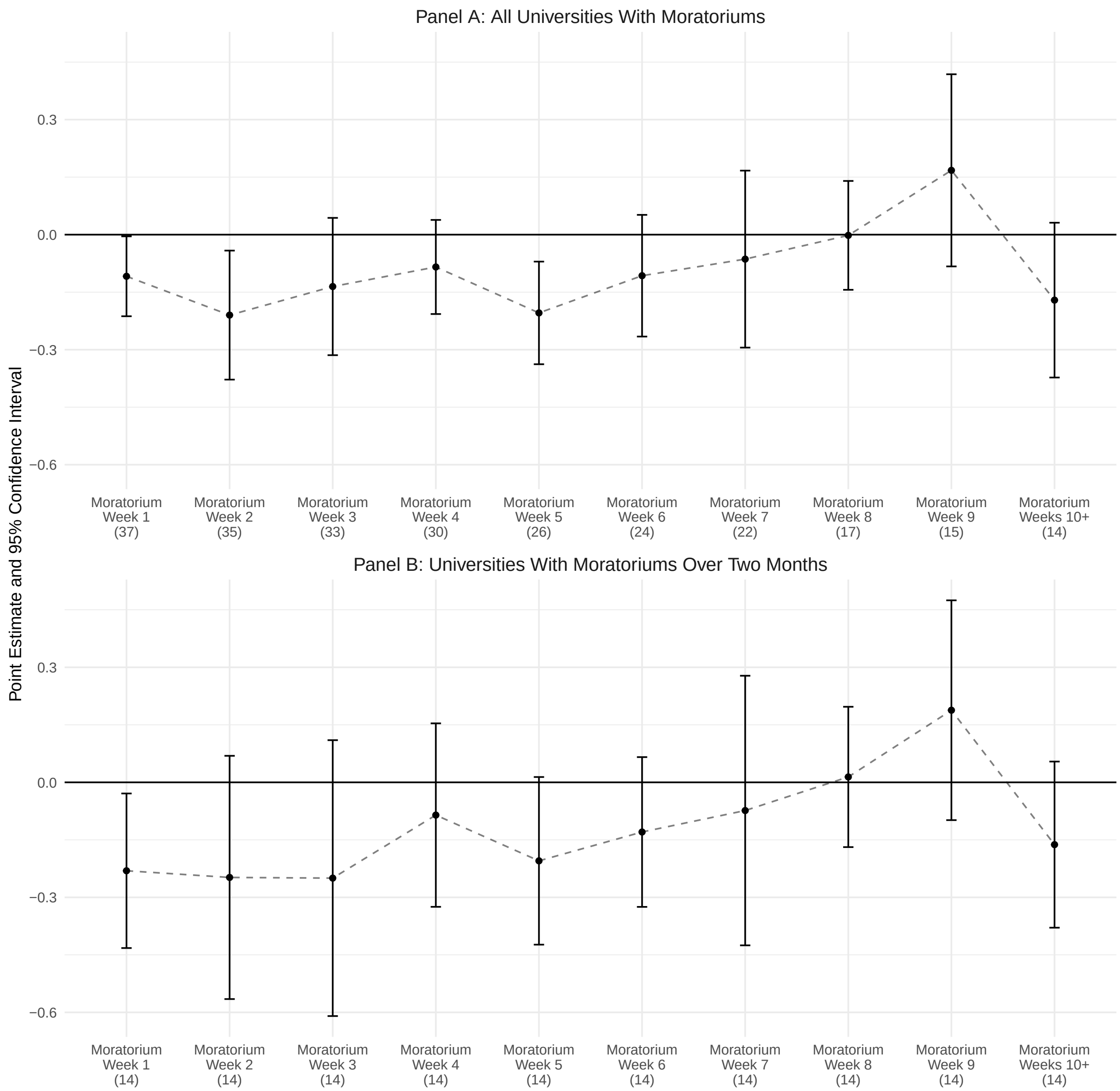

Figure 8: The Dynamics of a Moratorium (Alcohol Offenses)

Note: This figure shows how the effect of a moratorium progresses over time. Each point estimate represents a week within a moratorium, except Moratorium Weeks 10+, which pools moratoriums weeks 10 and above. The x-axis represents the week number the moratorium is currently in, while the parenthesis represents the number of universities that identify the point estimate. Recall that moratorium lengths differ across universities, and therefore some universities may not identify each weekly estimate. The y-axis represents the point estimates and 95% confidence intervals. Panel A estimates include all universities in the sample using the preferred specification, while Panel B estimates include only universities that have moratoriums over two-months long (approximately the average length of a moratorium). Standard errors are clustered by university, and controls include holiday, spring semester, day of the week, and university-by-academic-year.

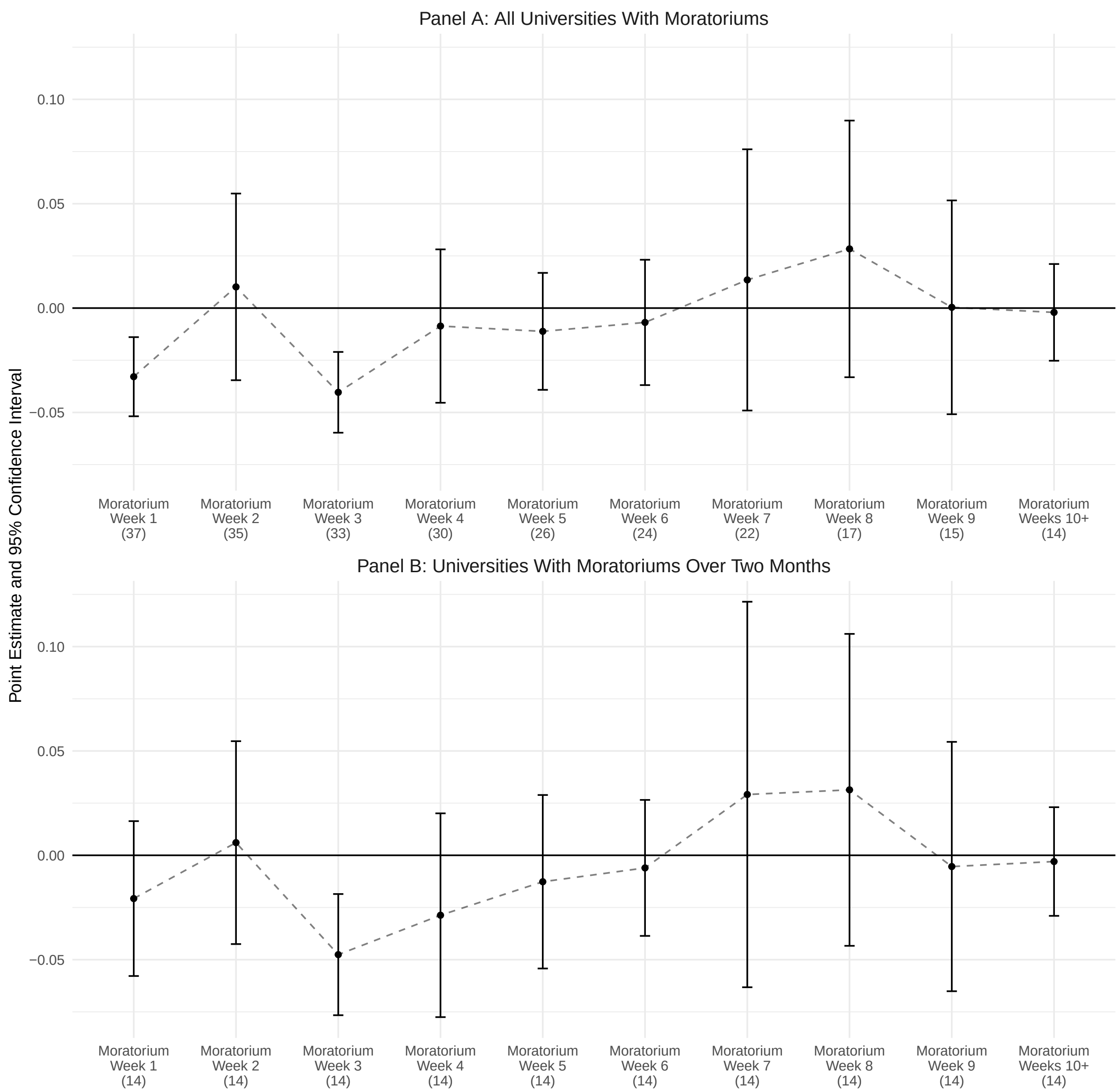

Figure 9: The Dynamics of a Moratorium (Sexual Assaults)

Note: This figure shows how the effect of a moratorium progresses over time. Each point estimate represents a week within a moratorium, except Moratorium Weeks 10+, which pools moratoriums weeks 10 and above. The x-axis represents the week number the moratorium is currently in, while the parenthesis represents the number of universities that identify the point estimate. Recall that moratorium lengths differ across universities, and therefore some universities may not identify each weekly estimate. The y-axis represents the point estimates and 95% confidence intervals. Panel A estimates include all universities in the sample using the preferred specification, while Panel B estimates include only universities that have moratoriums over two-months long (approximately the average length of a moratorium). Standard errors are clustered by university, and controls include holiday, spring semester, day of the week, and university-by-academic-year.

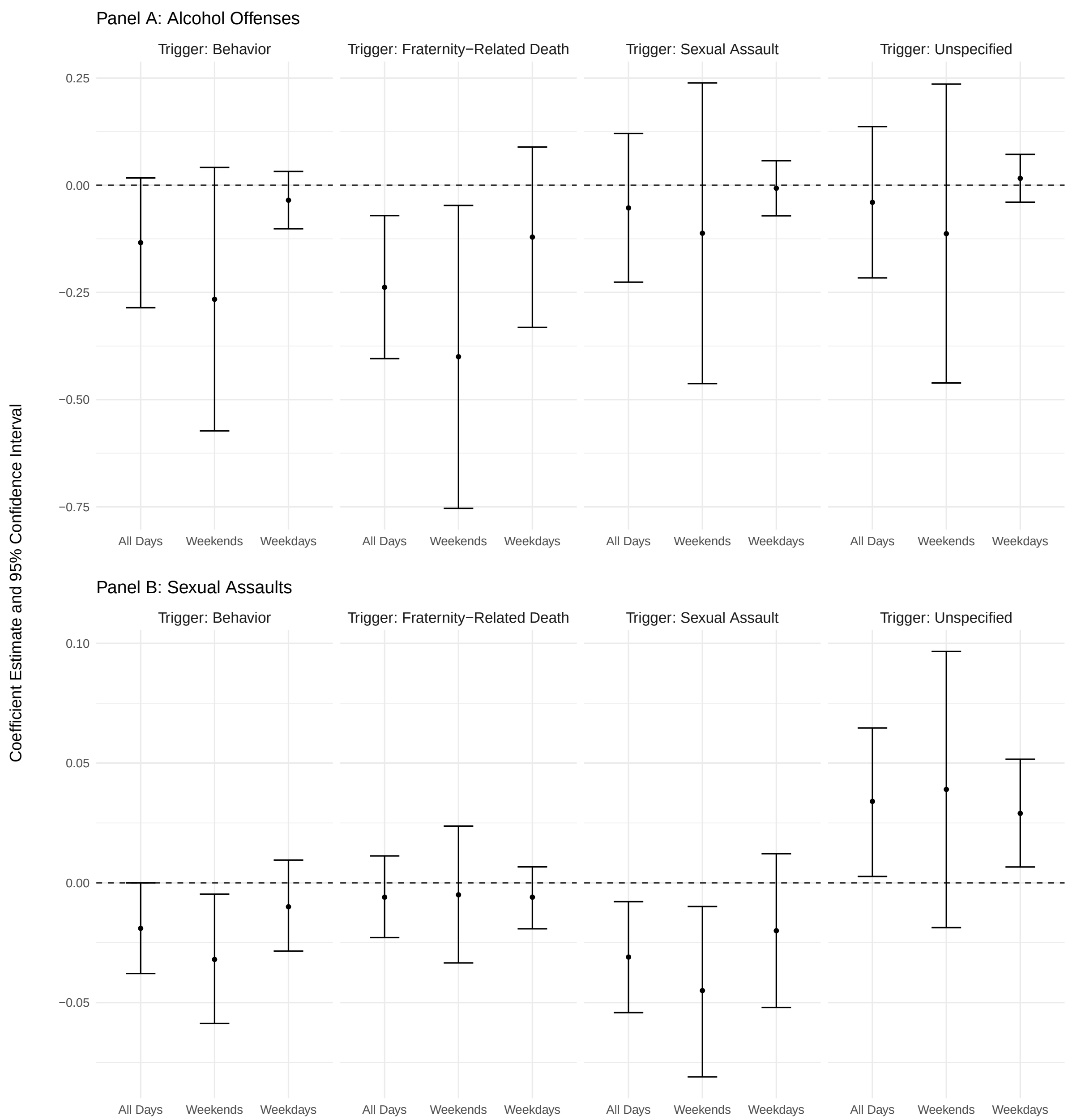

Figure 10: Heterogeneous Effects of Moratoriums by Triggering Event

Note: The x-axis represents three periods: the entire sample (All Days), weekends only, and weekdays only. Specification 2 (the preferred specification) from Table 4 is used in estimation. Each of the four categories represent the event that triggered a moratorium. A behavior violation refers to hazing, rule violations, offensive behavior, and other disorderly conduct. Death relates to a fraternity-related death that triggered a moratorium. Sexual assaults relate to a sexual assault case that triggered a moratorium. Lastly, the Unspecified category represents all moratoriums in which the moratorium triggering event is unknown or unclear. Errorbars represent 95% confidence intervals. Weekends represent Friday-Sunday, while Weekdays represent Monday-Thursday.

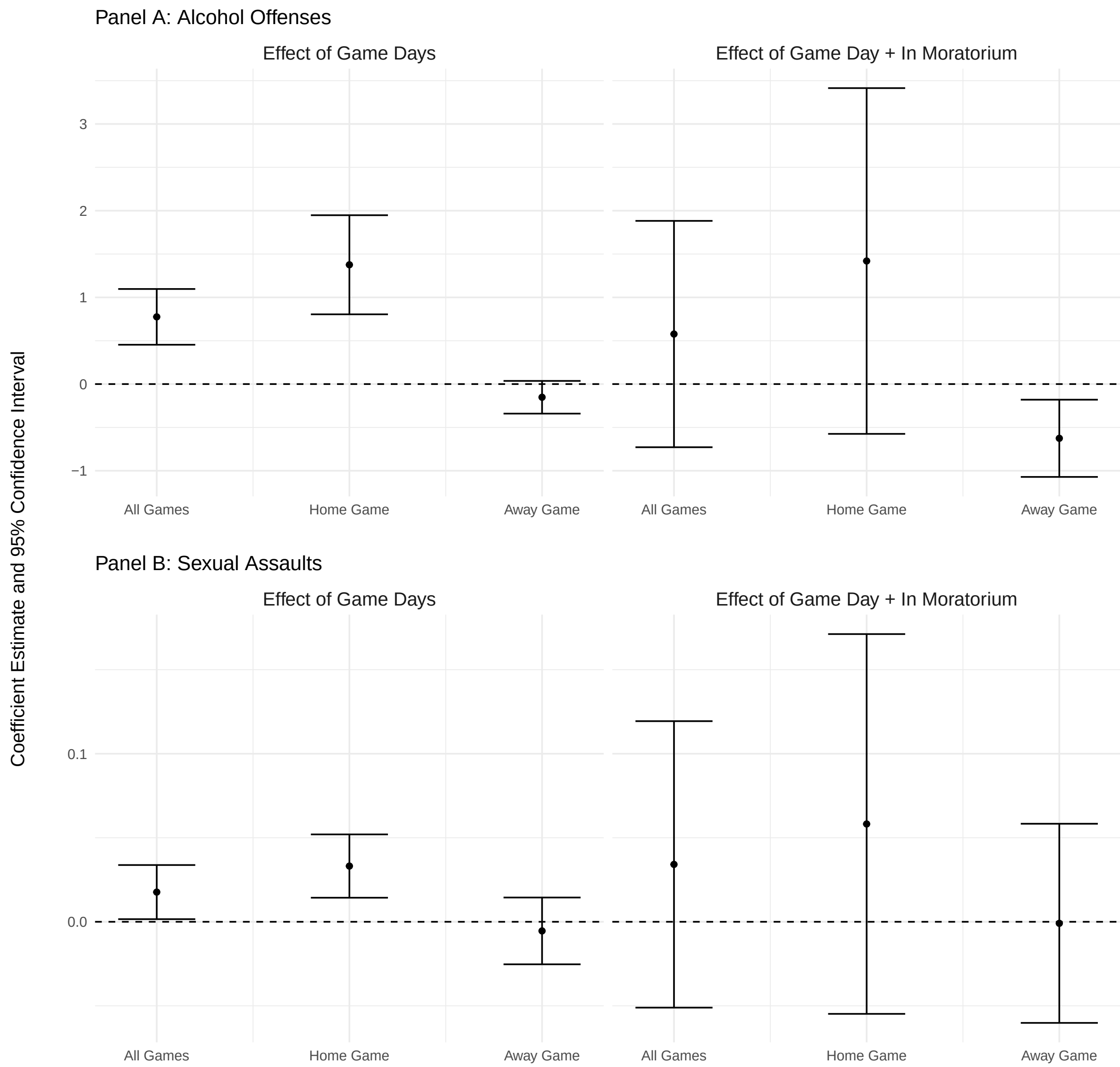

Figure 11: The Effect of Football Game-days With and Without Moratoriums

Note: Game days include all football games occurring in the sample period. 34 of the 37 universities have football teams and corresponding game days. The y-axis represents coefficient estimates. Errorbars represent 95% confidence intervals. Each panel is split into two effects: the first effect being the effect of only football game days on the outcome per-25000 enrolled students, and the second being the effect of a football game that occurs within a moratorium. The All Games category includes both home and away games. The effects of game days + moratorium is identified by 89 football games that coincide with moratoriums. Controls include holiday, spring semester, day of the week, and university-by-academic-year. Standard errors are clustered by university.

The Effect of Fraternity Moratoriums on Alcohol Offenses and Sexual Assaults

Michael Topper

Online Appendix

A Details on Data Collection

In this appendix, I cover minute details of the data collection. First, a list of universities that underwent moratoriums but are not included in the sample are explained. Second, the small portion of missing data in the Daily Crime Logs is discussed.

A Sample Selection Details

Recall from Section 3.A that the main sample includes 37 universities which experienced moratoriums between 2014 and 2019. However, these do not represent the universe of fraternity moratoriums that occur in this time period. In particular, there are six schools that are known to have experienced a moratorium in this time frame, but are excluded due to data issues or their definition of a moratorium. First, Miami University is excluded since the end-date of their moratorium could not be verified. Second, Pennsylvania State University is excluded because they did not digitally release their Daily Crime Logs. Third, the University of Texas at Arlington is excluded because the crime logs are scanned images that cannot be read reliably by any computer software. Fourth, Cal State Northridge is excluded because it is unclear whether the moratorium includes a ban on alcohol. Fifth, the University of North Florida is excluded because of a discrepancy between public records information and newspaper articles—newspaper articles claim there is a moratorium beginning 12/4/17, but the public records department claims this is untrue. Last, the University of Vermont is excluded due to issues with the reliably of the data—crimes often are reported to have occurred in large intervals of days (or months) for nearly 40% of the data provided which is not suitable for the daily-level analysis in this paper. There may exist other universities that experienced a moratorium without news coverage—these are also excluded from the sample.

B Daily Crime Log Details

As outlined in Section 3.A, the Daily Crime Logs are mandated under the Clery Act to include a set of characteristics for each crime and to be maintained for seven years. Despite these mandates, there are exceptions to each of these. First, while the date occurred is mandated to be included in the Daily Crime Logs, only 32 of the 37 universities' crime logs contain the date occurred. However, these five schools contain the date reported, and therefore, I use the date reported in lieu of the date occurred when the date occurred is missing. Second, the seven-year record mandate is not interpreted uniformly across universities. In particular, if Daily Crime Logs from 2014 are requested in year 2021, the police departments of Rollins College and North Carolina State University consider seven-years to be inclusive of their current year, and hence, only retain records from 2015-2021 or have only partially completed records in 2014 respectively.

B Robustness Under TWFE

In this appendix, I estimate a model that contains no negative weights to acknowledge the potential issues with the difference-in-differences estimator as discussed in Section 4.C. These weights are calculated using the TwoWayFEWeights package (Chaisemartin, D'Haultfoeuille, and Deeb 2020). The estimated model is the following two-way fixed effects (TWFE) specification:

$$Y_{ut} = \beta Moratorium_{ut} + \gamma_u + \alpha_t + \epsilon_{ut}$$

where Y_{ut} is the outcome for university u at time t measured by per-25000 enrolled students per academic-calendar day, $Moratorium_{ut}$ is an indicator equal to one if university u is in a moratorium at time t, γ_u are university fixed effects, α_t are day by month by year fixed effects, and ϵ_{ut} is the error term. Hence, this model compares academic-calendar days within a moratorium to the same calendar days without a moratorium while controlling for systematic differences between universities. As mentioned above, there are no negative weights in this specification and therefore sign reversal is impossible. With this advantage, I re-estimate the results in Columns 2, 3, and 5 in Table 4.

Table B1 shows that the results of the TWFE specification with no negative weights are mostly consistent with the results in Table 4. In Panel A, alcohol offenses exhibit a 19% decrease from the mean during a moratorium, with a 25% decrease on the weekends. Although sexual assaults do not exhibit statistically significant decreases on the weekends, this is potentially due to the loss of identifying variation from the data-intensive controls. However, it is important to note that the coefficient sign remains the same on all of the estimates. Hence, under the identifying assumptions of the model, moratoriums decrease alcohol offenses.

Table B1: Effect of Moratoriums on Alcohol Offenses and Sexual Assault by Weekend/Weekdays (No Negative Weights-OLS)

	Days of the Week		
	All Days (1)	Weekends (2)	Weekdays (3)
Panel A: Alcohol Offenses			
In Moratorium	-0.091*	-0.211**	-0.004
	(0.045)	(0.097)	(0.017)
Observations	55115	23643	31472
Mean of Dependent Variable	0.464	0.828	0.190
Panel B: Sexual Assaults			
In Moratorium	-0.006	-0.008	-0.004
	(0.005)	(0.007)	(0.007)
Observations	55115	23643	31472
Mean of Dependent Variable	0.049	0.058	0.042
FE: University	X	X	X
FE: Day by Month by Year	X	X	X

Note:
Standard errors are clustered by university and each offense is defined as per-25000 enrolled students. Column 1 represents the preferred specification from the main results table, Column 2. Weekends consist of Fridays, Saturdays, and Sundays. Weekdays consist of Monday through Thursday. The specification used in this table has no negative weights and thus, sign reversal is ruled out.
* p < 0.1, ** p < 0.05, *** p < 0.01

C Spillover Analysis Using CSS Data

In this appendix, I use the Campus Safety and Security (CSS) data to indirectly analyze whether alcohol offenses and sexual assaults are being displaced to riskier areas during a moratorium. I compare the yearly aggregation of the Daily Crime Logs to the CSS data using a model that is less suited for a causal analysis due to the yearly aggregation of the CSS data. Therefore, the estimates in this appendix should be taken as speculative only.

A CSS Data and Empirical Strategy

The CSS data is maintained by the US Department of Education. This data is mandated by the federal government to be updated each calendar year with the yearly totals of liquor law disciplinary actions and arrests, and sexual assault violations that are reported *to any entity* at a university. Hence, this data will not match one-to-one with the Daily Crime Logs as the Daily Crime Logs contain only incidents *reported to or by the university police*. For instance, if a residence hall administrator issues a liquor violation to an underage student, but handles the issue internally without involving the police, then this would be included in the CSS data as a liquor law disciplinary action, but not the Daily Crime Logs. However, the advantage of the CSS data is that it contains counts of offenses that occur on-campus, not-on-campus, and on public property.[1] Most importantly, I am able to delineate whether incidents occur in student residence halls.

Since the CSS data is aggregated by calendar-year, the CSS data is not a preferred data source for causal analysis. In spite of this shortcoming, I estimate the following difference-in-differences specification:

[1]Not-on-campus is defined by the Department of Education as "(1) Any building or property owned or controlled by a student organization that is officially recognized by the institution; or (2) Any building or property owned or controlled by an institution that is used in direct support of, or in relation to, the institution's educational purposes, is frequently used by students, and is not within the same reasonably contiguous geographic area of the institution." Furthermore, public property is defined as "All public property, including thoroughfares, streets, sidewalks, and parking facilities, that is within the campus, or immediately adjacent to and accessible from the campus."

$$Y_{u,t} = \beta Moratorium_{u,t} + \gamma_u + \lambda_t + \epsilon_{u,t} \tag{C0}$$

where $Y_{u,t}$ is the offense of interest defined as per-25000 enrolled students per-calendar-year, $Moratorium_{u,t}$ is the ***number*** of calendar-days with a moratorium within a year, γ_u are university fixed effects, λ_t are calendar-year fixed effects, and $\epsilon_{u,t}$ is the error term. Standard errors are clustered at the university level to account for serial correlation within universities.

B Results

Table C1 shows the comparison of estimating Equation C0 with the Daily Crime Logs aggregated to the calendar-year level with the CSS data.[2] The Daily Crime Logs show relatively consistent results with those found in Table 4; yearly averages of alcohol offenses per-25,000 enrolled students decrease by approximately 0.134 per additional calendar day with a moratorium and sexual assaults decrease by approximately 0.013.

Although the results using aggregated Daily Crime Logs are consistent with the findings in Table 4, the CSS data shows that residence halls experience a 0.270 *increase* in yearly liquor law disciplinary violations per-25,000 enrolled students and a 0.033 *decrease* in reports of sexual assault for each additional calendar-year-day with a moratorium (Column 3). Each of these estimates are significant at the 5% level. However, there is little evidence of an effect on liquor law arrests as shown in Columns 4 and 5—consistent with the literature that campus police do not typically arrest students for alcohol violations (Bernat et al. 2014). As discussed in Section 5.B, this supports the notion that if moratoriums displace alcohol-fueled behavior, they displace it to *less* risky areas whereby behavior can more easily be intervened before it becomes dangerous.

[2] This aggregation includes all calendar-year days rather than only academic-calendar days that were used in the main analysis.

Table C1: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults: Comparison of Daily Crime Logs and Campus Safety and Security (OLS)

		Campus Safety and Security			
	Daily Crime Logs	Displinary Actions/Reported Crime		Arrests	
	All Reports	All Reports	Residence Halls	All Reports	Residence Halls
	(1)	(2)	(3)	(4)	(5)
Panel A: Alcohol Offenses					
In Moratorium	-0.134*	0.297**	0.270**	-0.022	-0.025
	(0.077)	(0.118)	(0.125)	(0.056)	(0.040)
Mean of Dependent Variable	131.861	362.978	343.616	55.961	24.280
Observations	220	222	222	222	222
FE: Year	X	X	X	X	X
FE: University	X	X	X	X	X
Panel B: Sexual Assaults					
In Moratorium	-0.013	-0.046	-0.033**		
	(0.011)	(0.039)	(0.014)		
Mean of Dependent Variable	14.099	28.732	14.444		
Observations	220	222	222		
FE: Year	X	X	X		
FE: University	X	X	X		

Note:
Standard errors are clustered by university and each offense is defined as offense per-25000 enrolled students per-calendar year. Recall that Daily Crime Logs are the primary source of data used in prior analysis. In this model, the In Moratorium treatment variable is defined as the number of calendar-days that experienced a moratorium in a calendar-year. All Reports columns include the entire Daily Crime Logs/Campus Safety and Security Data (CSS), while Residence Halls is a subset of the CSS. All Reports in the CSS data contains both off-campus and on-campus reports. CSS data does not necessarily need to be reported to the university police and hence, may not show up in the Daily Crime Logs. Columns 2 and 3 refer to disciplinary actions for liquor law violations and reported crime for sexual assaults. Columns 4 and 5 refer to arrests for liquor law violations. Fixed effects include university and year fixed effects.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D Is the Share of Students in a Fraternity Important for Effectiveness?

In this appendix, I analyze whether universities with a higher fraction of undergraduates belonging to IFC fraternities exhibit larger effects during a moratorium. Each university in the sample has a different share of its student population belonging to IFC fraternities. Recall from Table 2 that the fraction of undergraduate students with IFC membership can range from 1% to as high as 11%. Presumably, a moratorium has a greater effect on student behavior when the restrictions apply to a greater share of students.

To conduct this analysis, I supplement the preferred specification with an interaction of $InMoratorium_{u,t}$ and $FractionIFC_u$, where $FractionIFC_u$ is the earliest recorded count of IFC fraternity members over 2014-2019 at university u, divided by the undergraduate enrollment, and centered at its mean. I use the earliest count of IFC members for two reasons; first, to avoid the potential issue of declines in IFC membership after a moratorium due to permanent suspensions of specific IFC chapters, and second, many universities do not maintain records of IFC numbers for every year in the sample period. However, in the universities that do supply complete records, I do not find substantial semester-to-semester changes in IFC populations.[3] Therefore, an early one-year measure of the IFC population is a good approximation for the other corresponding years. In effect, the interaction of $InMoratorium_{u,t}$ and $FractionIFC_u$ creates a measure of moratorium intensity—universities with a higher fraction of IFC members receive a more intense treatment than universities with lower shares.

Table D1 provides suggestive evidence that moratoriums with a higher fraction of student enrollment belonging to an IFC fraternity result in larger decreases in alcohol offenses and sexual assaults during a moratorium period. In Panel A, the point estimates for the interaction term show patterns consistent with the main findings in the paper—the effects are negative with the strongest effects are observed on the weekends when partying is more frequent. Similarly,

[3] West Virginia University is an exception to this. Their official IFC count decreased by over 60 percent in years following the moratorium.

in Column 1 of Panel B, the interaction term coefficient shows suggestive evidence that moratoriums in universities with a higher share of IFC members exhibit larger decreases of sexual assaults. However, none of the interaction coefficients presented in either panel are significant, indicating only a suggestive relationship between the share of IFC members and the impact of moratoriums.

The results of Table D1 may appear surprisingly inconclusive given the expectation that universities with a higher share of fraternity members exhibit larger effects. One possible reason for these inconclusive results is that the share of fraternity members is a noisy indicator for a fraternity-related activity—schools with a small share of fraternity life may have chapters that are particularly active, or vice-versa. To demonstrate this, I plot each university's undergraduate IFC fraction against its Niche.com Colleges with the Best Greek Life ranking. The ranking, based on survey responses from Niche.com users, ranges from 1-300, and 32 out of the 37 universities in the sample are ranked in the top 300. For the remaining five schools, I assign a ranking between 301-305. Figure D1 shows the inverse relationship between these two measures: as the Greek Life ranking increases, the fraction of undergraduates in an IFC fraternity generally decreases. This likely contributes to the negative point estimates in the previous analysis. However, this relationship is noisy, and the slope is not statistically different from zero at the 5% level. This may explain why the previous analysis only provided suggestive rather than clear evidence.

Table D1: The Effect of Moratoriums Interacted with the Centered IFC Share (OLS)

	All Days	Weekends	Weekdays
	(1)	(2)	(3)
Panel A: Alcohol Offenses			
In Moratorium	-0.124**	-0.239**	-0.038
	(0.051)	(0.107)	(0.026)
In Moratorium x Fraction IFC	-0.231	-0.729	-0.209
	(1.402)	(2.629)	(0.733)
Mean of Dependent Variable	0.464	0.828	0.190
Observations	55115	23643	31472
Panel B: Sexual Assaults			
In Moratorium	-0.010	-0.017	-0.004
	(0.007)	(0.010)	(0.006)
In Moratorium x Fraction IFC	-0.068	0.164	-0.242
	(0.235)	(0.304)	(0.234)
Mean of Dependent Variable	0.049	0.058	0.042
Observations	55115	23643	31472
FE: Day of Week	X	X	X
FE: Holiday	X	X	X
FE: Game Day	X	X	X
FE: Semester (Spring/Fall)	X	X	X
FE: University by Academic Year	X	X	X

Note:
Fraction IFC is the average share of undergraduates that are in an IFC fraternity, centered at the mean. Note that not every university keeps record of their IFC numbers over time, and therefore, the most recent number of IFC members is used in this calculation when sample-period data is missing. However, based on the few universities that provided year-to-year data on their IFC populations, the total number does not substantially change over time. Standard errors shown in parenthesis are clustered by university (37 clusters) and each offense is defined as per-25000 enrolled students. The interaction of In Moratorium and Fraction IFC gives a measure of moratorium intensity based on the fraction of IFC members. The regression specification is the preferred specification which includes day of week, holiday, football game-day, semester, and university-by-acacdemic-year fixed effects.
* p < 0.1, ** p < 0.05, *** p < 0.01

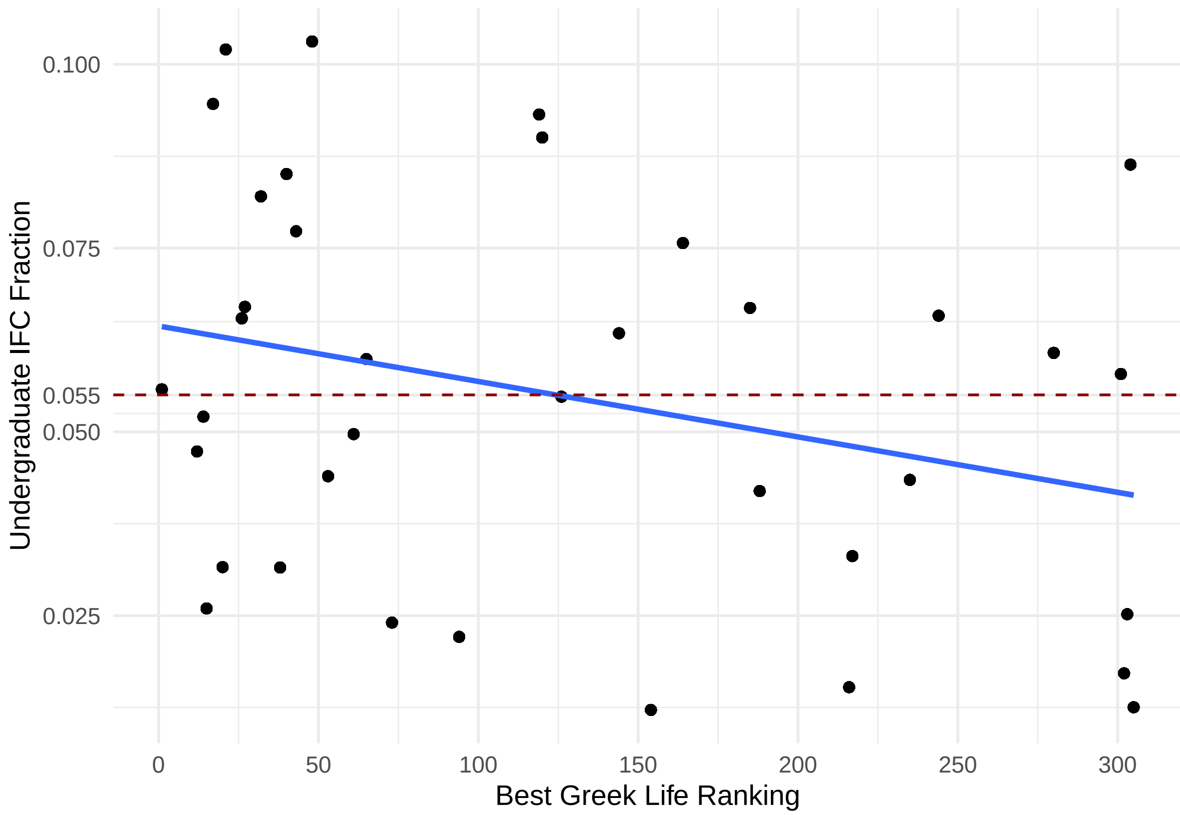

Figure D1: Scatterplot of Best Greek Life Ranking and IFC Fraction

Note: The x-axis represents the ranking from Niche.com's Colleges with the Best Greek Life list. There are 300 rankings within this list. Of the four universities that are not ranked, a ranking between 301 and 305 is assigned. The y-axis represents the share of undergraduate students that are a member of an IFC fraternity. The dashed red line denotes the average share of undergraduate students that are in an IFC fraternity, while the blue line represents the regression estimation of the share of undergraduate students on the Colleges with the Best Greek Life ranking. Note that, at the five percent level, the slope of the regression line is not statistically different from zero.

E Appendix Figures and Tables

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Indiana University, Bloomington
Police Department
Student Right To Know CAD Daily Log
From Jan 20, 2014 to Jan 20, 2014.

Date Reported: 01/20/14 - MON at 12:22 **Location :** EIGENMANN HALL **Event #:** 14-01-20-001434
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 17:03 **Location :** ALL OTHER ROADWAYS/INTERS **Event #:** 14-01-20-001446
Date and Time Occurred From - Occurred To 01/20/14 - MON at 17:02 - 01/20/14 - MON at 17:03
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:** 140154
Disposition: CLOSED BY ARREST

Date Reported: 01/20/14 - MON at 19:30 **Location :** EIGENMANN HALL **Event #:** 14-01-20-001464
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 20:22 **Location :** EIGENMANN HALL **Event #:** 14-01-20-001466
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 20:45 **Location :** FOSTER HARPER HALL **Event #:** 14-01-20-001468
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 21:38 **Location :** ALL OTHER NON-UNIVERSITY **Event #:** 14-01-20-001476
Date and Time Occurred From - Occurred To
Incident : ALL OTHER OFFENSES - HARASSMENT/INTIMIDATION **Report #:**
Disposition: NO CASE REPORT

Date Reported: 01/20/14 - MON at 21:53 **Location :** ROSE AVE RESIDENCE HALL **Event #:** 14-01-20-001479
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 22:30 **Location :** COLLINS COMMON AREA **Event #:** 14-01-20-001486
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 23:02 **Location :** FOREST QUAD **Event #:** 14-01-20-001487
Date and Time Occurred From - Occurred To 01/20/14 - MON at 22:45 - 01/20/14 - MON at 23:02
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:** 140157
Disposition: CLOSED NO ARREST.

Date Reported: 01/20/14 - MON at 23:07 **Location :** FOSTER JENKINSON HALL **Event #:** 14-01-20-001491
Date and Time Occurred From - Occurred To
Incident : NARCOTIC/DRUG LAWS - POSSESSION - MARIJUANA **Report #:**
Disposition: FAILED TO LOCATE

Date Reported: 01/20/14 - MON at 23:35 **Location :** ALL OTHER OPEN AREAS **Event #:** 14-01-20-001494
Date and Time Occurred From - Occurred To 01/20/14 - MON at 23:35 - 01/20/14 - MON at 23:41
Incident : ASSAULT - OTHER ASSAULTS - SIMPLE, NOT AGGRAVATED **Report #:** 140159
Disposition: CLOSED BY ARREST.

11 Incidents Listed.

Print Date and Time 1/21/2014 12:23:52PM at Page No. 1

Figure E1: An Example of a Daily Crime Log

Note: The main analysis uses data from 37 universities' Daily Crime Logs—each unique in their own respect. All Daily Crime Logs are collected from each university and harmonized using the pattern matching technique outlined in Section 3.B.

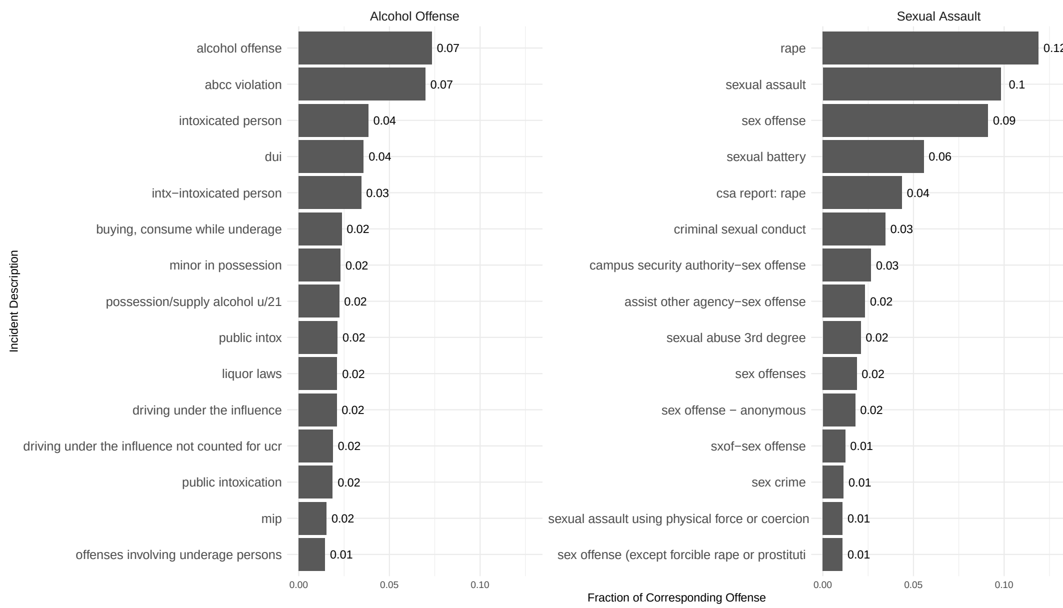

Figure E2: Top 15 Most Frequent Offense Matches

Note: The top 15 most frequent offense matches represent the 15 most frequent incidents after the pattern matching process. The x-axis represents the fraction of the total number of offenses in each category.

Table E1: Description of the Triggering Events that lead to a Moratorium

University	Description of Triggering Event	Triggering Event Date	Moratorium Start Date	Classification
Arkansas State University-Main Campus	Arrest of a man suspected of raping a 19-year old woman at a party in a fraternity house.	2017-02-10	2017-02-21	Sexual Assault
Ball State University	Concerns regarding the behavior and actions of members of IFC fraternities.		2017-10-24	Behavior
Cal Poly San Luis Obispo	A report of a sexual assault that allegedly took place at a social event hosted by a Greek group.		2015-01-13	Sexual Assault
Cal Poly San Luis Obispo	Racially insensistive photos surfacing on social media featuring fraternity members in both blackface and gang-related images.	2018-04-08	2018-04-17	Behavior
Clemson University	Alleged sexual assault.	2018-01-27	2018-01-29	Sexual Assault
College of Charleston	Decision was made after consulting with student leaders within the community.		2016-08-30	Unspecified
East Carolina University	An alleged sexual assault on Jan. 25 that provoked an ongoing investigation with the Greenville Police Department.	2015-01-25	2015-01-28	Sexual Assault
Emory University	Report of a sexual assault in a fraternity house.	2014-11-02	2014-11-03	Sexual Assault
Florida Atlantic University	Tailgating issues involving alcohol.		2017-11-28	Behavior
Florida International University	Growing concerns about the state of fraternity and sorority life at FIU as well as around the nation.		2018-01-01	Unspecified
Florida State University	Death of Andrew Coffey.	2017-11-03	2017-11-06	Death
Indiana University-Bloomington	A university spokesperson said the decision came in light of the ongoing national conversation about Greek life and its place on college campuses, as well as challenges on IU's Bloomington campus. The decision is not attributable to one particular incident.		2017-11-27	Unspecified
Louisiana State University	Death of Maxwell Gruver.	2017-09-14	2017-09-14	Death
Louisiana State University	Unclear.		2017-10-19	Unspecified
Marshall University	High-risk behavior in the fraternity community.		2018-03-05	Behavior
Monmouth University	Troubles within the fraternity system.		2018-09-06	Behavior

Murray State University	The letter implementing the suspension indicates that “national trends, and our own review…".		2018-08-27	Unspecified
North Carolina State University at Raleigh	Surfaced newstory of a pledge book that featured racially insensitive remarks and rape jokes.	2018-03-20	2018-03-20	Sexual Assault
Ohio State University-Main Campus	Proactive step based on the significantly high number of investigations this semester, not on the nature of any specific case or cases.		2017-11-16	Behavior
Ohio University-Main Campus	Allegations within the past week of hazing at seven of the fraternities.		2019-10-03	Behavior
Rollins College	The temporary suspension was issued after reviewing a ‘series of student conduct concerns.’		2017-02-21	Behavior
Rutgers University-New Brunswick	Several incidents with alcohol .		2015-04-06	Behavior
San Diego State University	Sexual assault allegations.		2014-11-25	Sexual Assault
San Diego State University	Ongoing concerns related to alcohol.		2018-03-09	Behavior
San Diego State University	Death of Dylan Hernandez.	2019-11-07	2019-11-09	Death
Syracuse University	A string of racist and anti-Semitic incidents.		2019-11-17	Behavior
Texas State University	Death of Matthew Ellis.	2017-11-13	2017-11-14	Death
Tufts University	Accusations of hazing and discrimination.		2016-11-16	Behavior
University at Buffalo	Death of Sebastian Serafin-Bazaan.		2019-04-12	Death
University of California-Berkeley	Reports of sexual assault at off-campus fraternity functions.		2016-10-16	Sexual Assault
University of Central Florida	Decision was made in light of drinking-related controversies.		2018-01-08	Behavior
University of Idaho	A response to the growing national crisis surrounding personal violence like hazing and sexual assault.		2017-12-12	Unspecified
University of Iowa	Death of Kamil Jackowski.	2017-04-30	2017-05-01	Death
University of Kansas	Poor behavior among some Greek groups at the University of Kansas.		2018-03-12	Behavior

University of Michigan-Ann Arbor	Claims of sexual misconduct cases involving fraternity brothers, six incidents of reported hazing, more than 30 hospital transports for students during the weekend of the football game against Michigan State.		2017-11-09	Sexual Assault
University of Missouri-Columbia	Hazing allegations.		2018-03-06	Behavior
University of New Mexico-Main Campus	With three UNM fraternities already in “emergency suspension” following allegations of hazing or alcohol policy violations, administrators have ordered a two-month halt to most social events within the university’s larger Greek system.		2017-12-08	Behavior
University of Pittsburgh	A serious alcohol incident involving members and non-members of one of the fraternities.	2018-01-18	2018-01-19	Behavior
University of Virginia-Main Campus	Rolling Stone article describing the fraternity culture at the school.	2014-11-19	2014-11-22	Sexual Assault
Washington State University	Due to the current negative reputation of the community.		2016-11-07	Unspecified
Washington State University	Death of Samuel Martinez.	2019-11-12	2019-11-14	Death
West Virginia University	Death of Nolan Burch	2014-11-12	2014-11-13	Death
West Virginia University	The result of a Theta Chi brother published a Snapchat video on social media using a racial slur directed at a bartender in a downtown Morgantown club.		2018-02-14	Behavior

Note:

Description of the triggering event is summarized based on newsarticles or conversations with Fraternity and Sorority Life staff. The date of the triggering event is shown if provided. The classification of each event is based off of the description and aligns with Figure 2.

Table E2: Moratorium Dates of Each University in the Sample

University	Start 1	End 1	Start 2	End 2	Start 3	End 3
Arkansas State University-Main Campus	2017-02-21	2017-04-01				
Ball State University	2017-10-24	2018-01-31				
California Polytechnic State University-San Luis Obispo	2015-01-13	2015-04-06	2018-04-17	2018-06-06		
Clemson University	2014-09-23	2014-10-10	2018-01-27	2018-03-01		
College of Charleston	2016-08-30	2016-12-01				
East Carolina University	2015-01-28	2015-02-11				
Emory University	2014-11-03	2014-12-02				
Florida Atlantic University	2017-11-28	2018-03-01				
Florida International University	2018-01-01	2018-02-05				
Florida State University	2017-11-06	2018-03-26				
Indiana University-Bloomington	2017-11-27	2018-02-28				
Louisiana State University and Agricultural & Mechanical College	2017-09-14	2017-10-12	2017-10-19	2018-03-01		
Marshall University	2018-03-05	2018-03-26				
Monmouth University	2018-09-06	2019-01-16				
Murray State University	2018-05-09	2018-08-27				
North Carolina State University at Raleigh	2015-03-20	2015-05-09				
Ohio State University-Main Campus	2017-11-16	2018-02-07				
Ohio University-Main Campus	2019-10-03	2019-10-25				
Rollins College	2017-02-21	2017-04-14				
Rutgers University-New Brunswick	2015-04-06	2015-05-01				
San Diego State University	2014-11-25	2015-01-09	2018-03-09	2018-10-04	2019-11-09	2020-01-17
Syracuse University	2019-11-17	2019-12-09				
Texas State University	2017-11-14	2018-02-26				
Tufts University	2016-11-16	2017-01-19				
University at Buffalo	2019-04-12	2019-08-21				
University of California-Berkeley	2016-10-16	2016-10-26				
University of Central Florida	2018-01-08	2018-03-05				
University of Idaho	2017-12-12	2018-03-13				
University of Iowa	2017-05-01	2019-08-27				
University of Kansas	2018-03-12	2018-03-18				
University of Michigan-Ann Arbor	2017-11-09	2018-01-03				
University of Missouri-Columbia	2018-03-06	2018-03-13				
University of New Mexico-Main Campus	2017-12-08	2018-02-19				
University of Pittsburgh-Pittsburgh Campus	2018-01-19	2018-08-30				
University of Virginia-Main Campus	2014-11-22	2015-01-07				
Washington State University	2016-11-07	2017-01-09	2019-11-14	2020-01-27		
West Virginia University	2014-11-13	2015-02-01	2018-02-14	2018-08-01		

Note:
Universities can have multiple moratoriums in the sample period. Each moratorium date was verified by either a Fraternity and Sorority Life advisor, a news article, or a public records request. However, the first San Diego State University moratorium end date could not be directly verified by either a fraternity or sorority advisor, news article, or public record request. Based on the following news article link, I am confident that the moratorium ended before the start of the 2015 semester. Link: https://newscenter.sdsu.edu/sdsu_newscenter/news_story.aspx?sid=75357

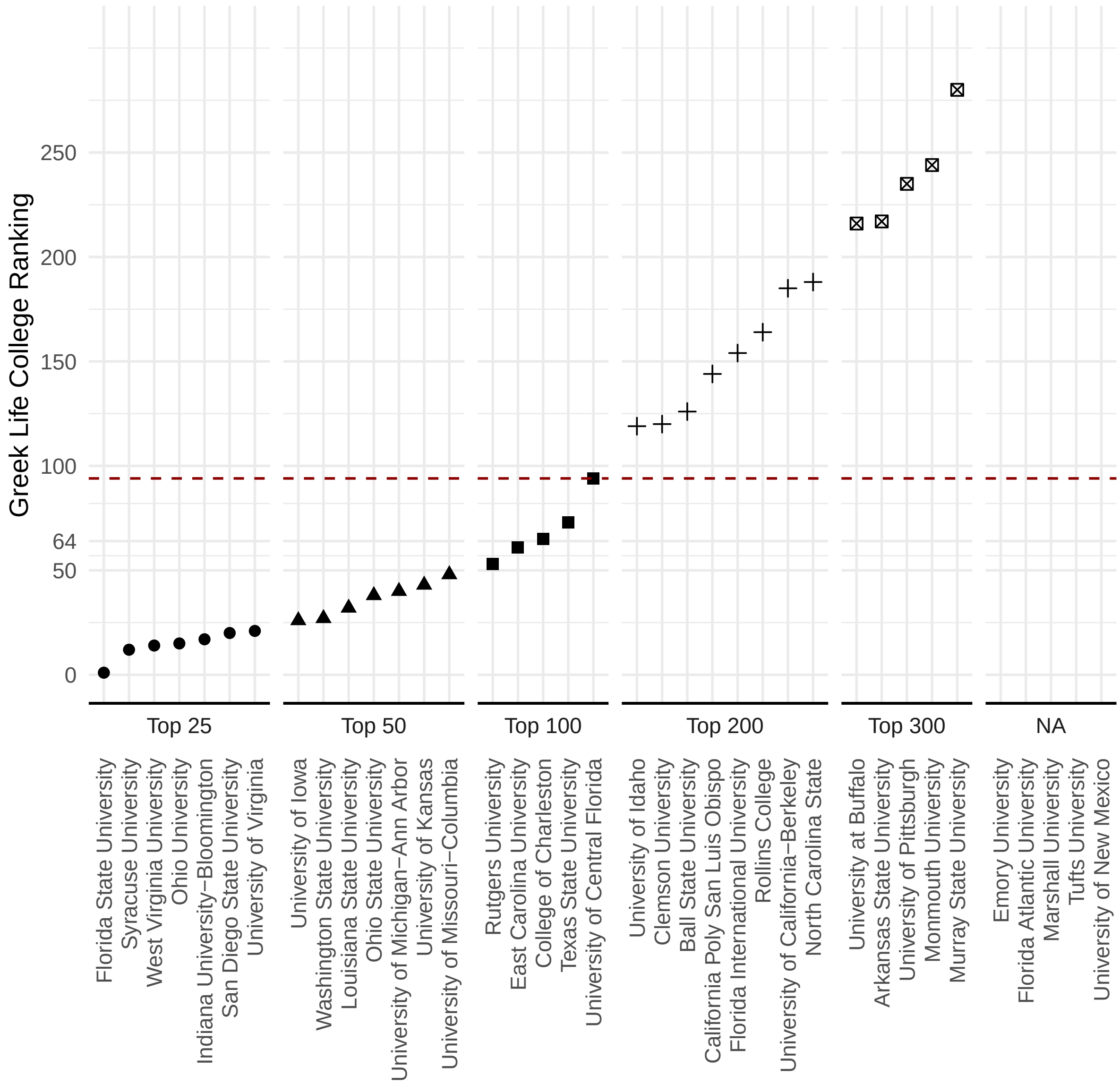

Figure E3: Greek-life Rankings of Universities Included in the Sample

Note: Greek-life rankings are based on Niche.com's 2023 list of Colleges with the Best Greek Life. Rankings are based on survey responses from Niche.com users on the quality of Greek Life at their school. The dashed red line represents the median ranking of the 37 universities in the sample. Three-hundred universities are ranked in the list. Of the universities in the sample, 14 of the 37 universities (38%) are ranked in the top 50, while only 5 of 37 (13%) are not ranked.

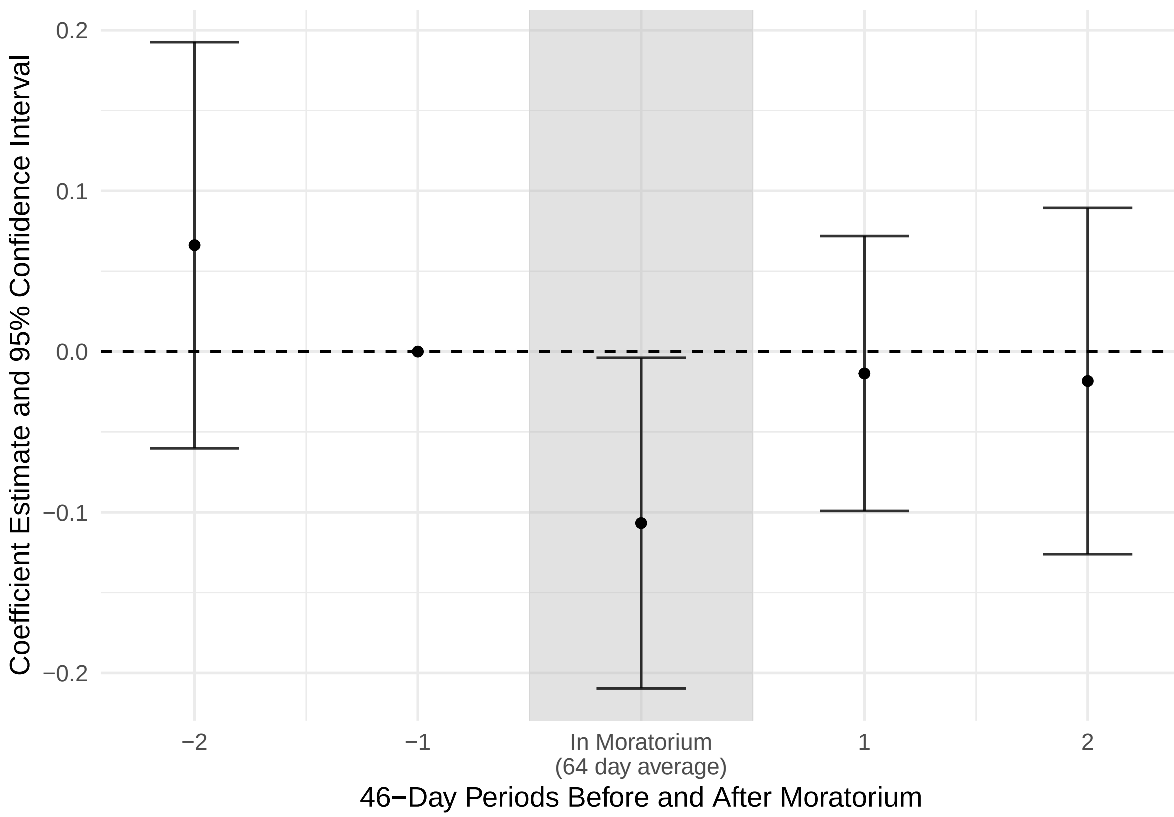

Figure E4: Event Study for Alcohol Offenses

Note: The shaded area point estimate represents an entire moratorium period for each university. Hence, the shaded area point estimate has varying amounts of days within based on the university. For instance, Arkansas State University had a 39 day moratorium and therefore their shaded area point estimate would be identified by the 39 moratorium days. Point estimates not within the shaded region are 46 day periods. Number of days within a period was chosen to give approximately two median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 46-day period before the moratorium. Alcohol offenses are defined as alcohol offenses per-25000 enrolled students. Controls include holiday, spring semester, day of the week, football game-days, and university by academic year. Standard errors clustered by university. All errorbars represent 95% confidence intervals.

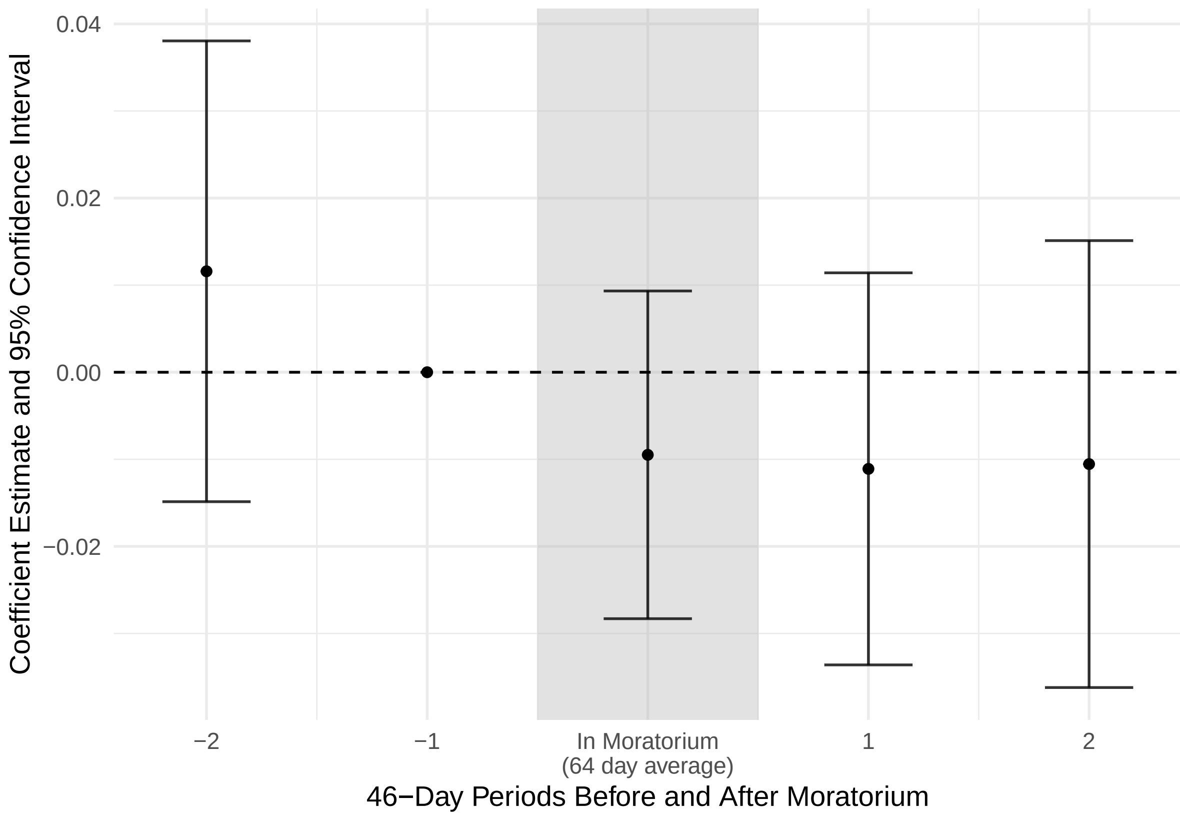

Figure E5: Event Study for Sexual Assault Offenses

Note: The shaded area point estimate represents an entire moratorium period for each university. Hence, the shaded area point estimate has varying amounts of days within based on the university. For instance, Arkansas State University had a 39 day moratorium and therefore their shaded area point estimate would be identified by the 39 moratorium days. Point estimates not within the shaded region are 46 day periods. Number of days within a period was chosen to give approximately two median-length (46 days) moratorium on each side of the shaded area. All periods are normalized by the 46-day period before the moratorium. Sexual assault offenses are defined as sexual assault offenses per-25000 enrolled students. Controls include holiday, spring semester, day of the week, football game-days, and university by academic year. Standard errors clustered by university. All errorbars represent 95% confidence intervals.

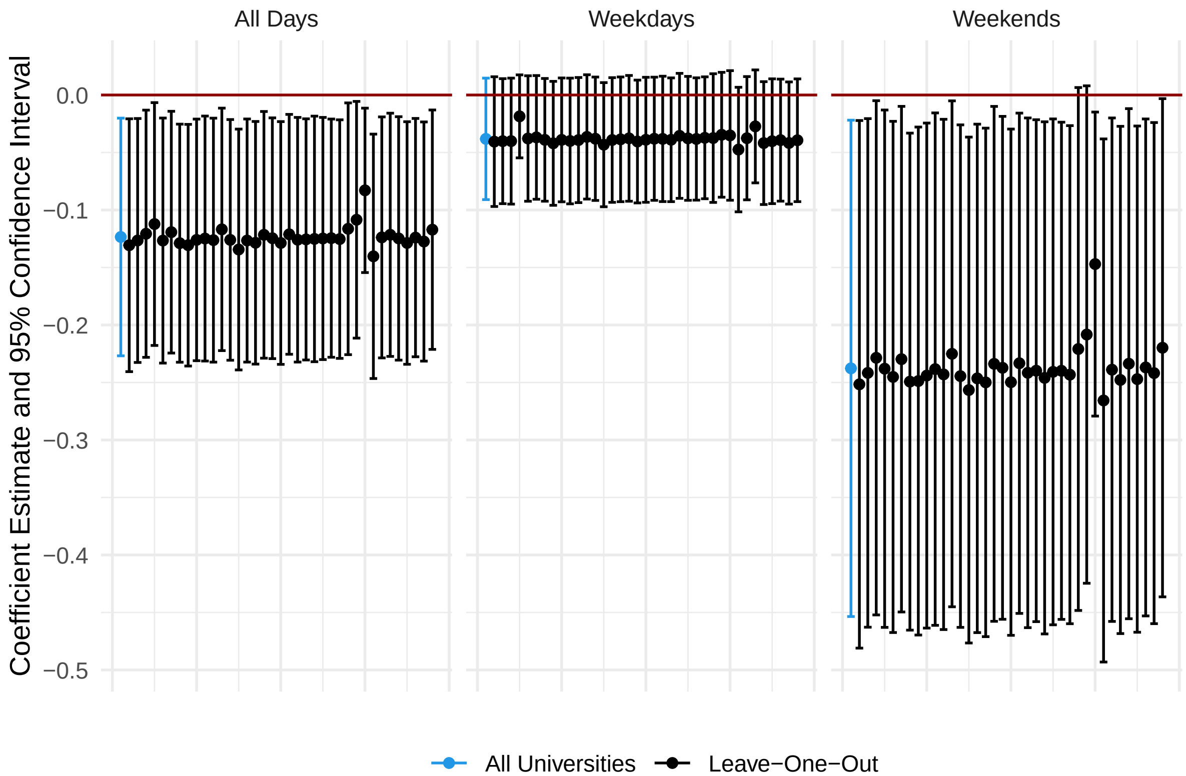

Figure E6: Leave-one-out OLS Regressions of Alcohol Offenses

Note: Each blue point represents the preferred specification (2) from Table 4. Each black point represents specification (2) from Table 4 with one university omitted from the sample. Offenses are per-25000 enrolled students. Errorbars represent 95% confidence intervals. Weekends includes only Friday, Saturday, Sunday, while weekdays includes Monday through Thursday.

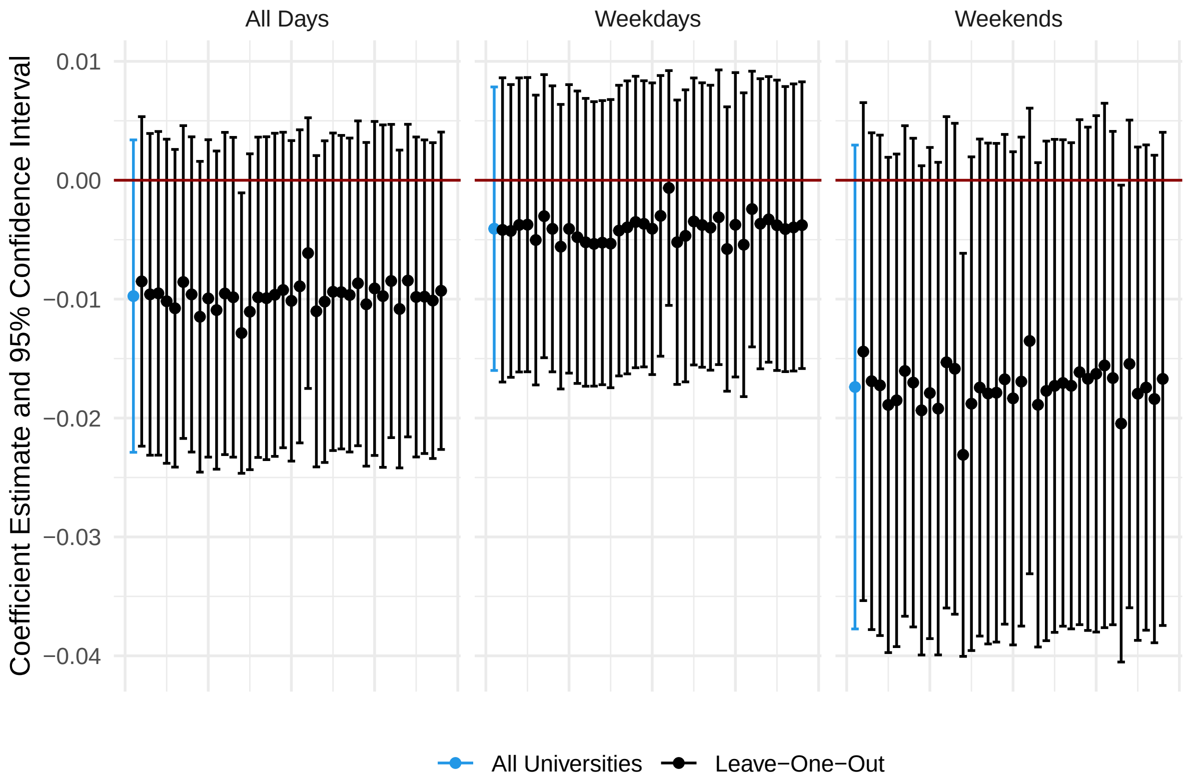

Figure E7: Leave-one-out OLS Regressions of Sexual Assaults

Note: Each blue point represents the preferred specification (2) from Table 4. Each black point represents specification (2) from Table 4 with one university omitted from the sample. Offenses are per-25000 enrolled students. Errorbars represent 95% confidence intervals. Weekends includes only Friday, Saturday, Sunday, while weekdays includes Monday through Thursday.

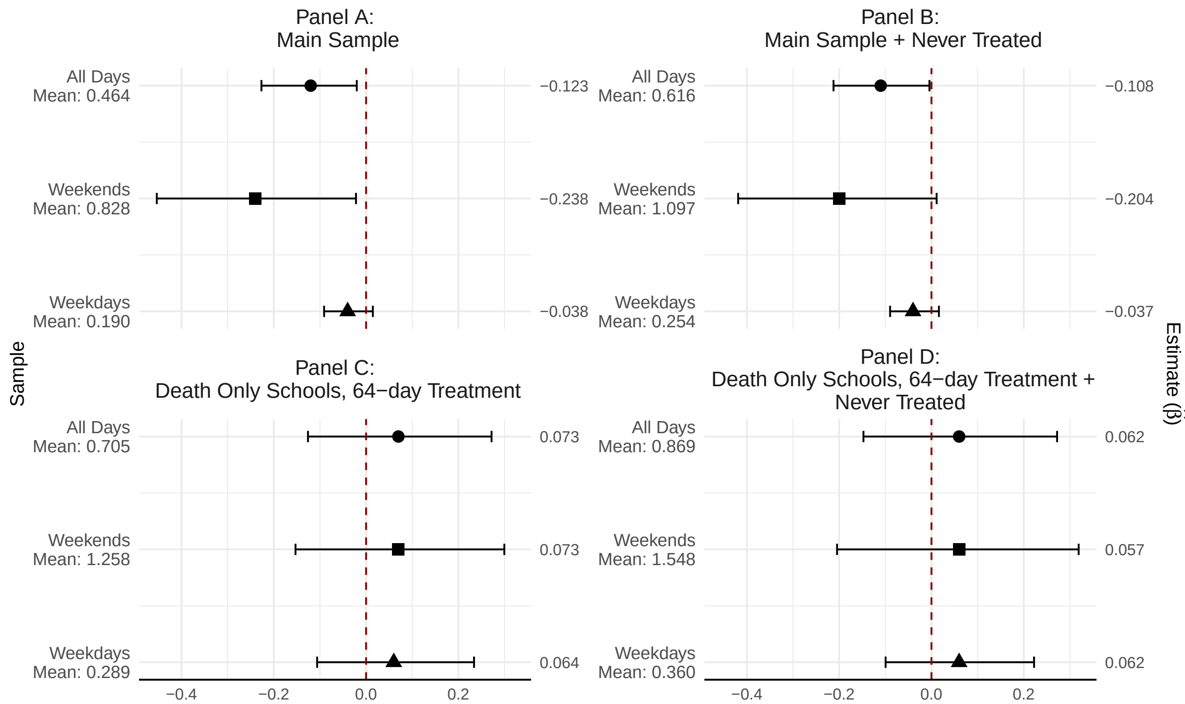

Figure E8: Robustness Across Samples (Alcohol Offenses)

Note: This graph depicts the coefficient estimates and 95% confidence intervals for different subsets of the sample. The y-axis on the left is the sample selection used, while the y-axis on the right is the point estimate. All estimates use the preferred specification from Table 4 Column 2, and all outcomes are in terms of per-25000 enrolled students. Standard errors are clustered at the university level. Panel A uses the main sample as shown in the main results, while Panel B uses the main sample in addition to 14 never-treated schools (see Section 5.A for more details). Panel C analyzes 15 universities which undergo a fraternity death, but do not undergo a moratorium. A 64-day binary treatment period is given to each of these universities, beginning on the date of the death. Panel D extends the analysis in Panel C by adding in the 15 never-treated universities as controls, analogous to Panel B in reference to Panel A. See Section 6.B for more details.

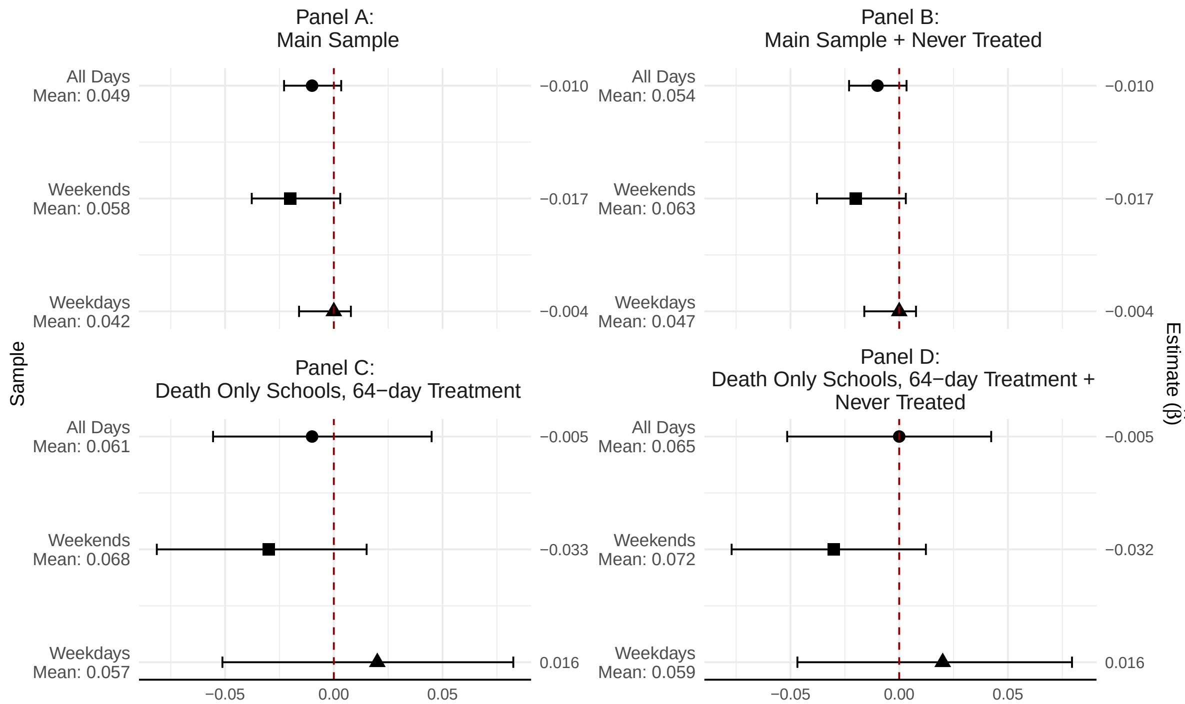

Figure E9: Robustness Across Samples (Sexual Assaults)

Note: This graph depicts the coefficient estimates and 95% confidence intervals for different subsets of the sample. The y-axis on the left is the sample selection used, while the y-axis on the right is the point estimate. All estimates use the preferred specification from Table 4 Column 2, and all outcomes are in terms of per-25000 enrolled students. Standard errors are clustered at the university level. Panel A uses the main sample as shown in the main results, while Panel B uses the main sample in addition to 14 never-treated schools (see Section 5.A for more details). Panel C analyzes 15 universities which undergo a fraternity death, but do not undergo a moratorium. A 64-day binary treatment period is given to each of these universities, beginning on the date of the death. Panel D extends the analysis in Panel C by adding in the 15 never-treated universities as controls, analogous to Panel B in reference to Panel A. See Section 6.B for more details.

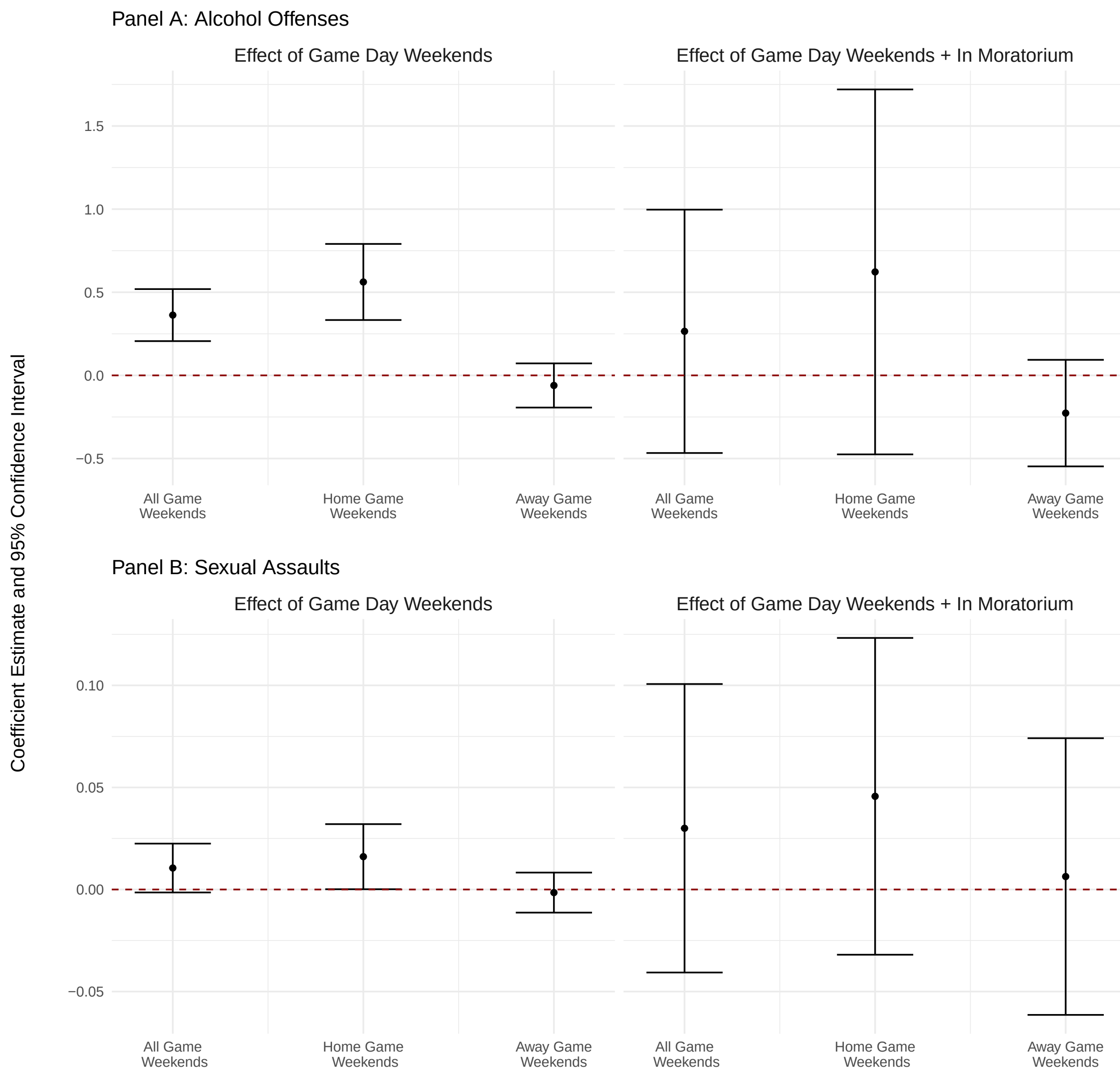

Figure E10: The Effect of Football Game-day Weekends With/Without Moratoriums

Note: Game weekends include all football games occurring in the sample period. 34 of the 37 universities have football teams and corresponding game days. The y-axis represents coefficient estimates. Errorbars represent 95% confidence intervals. Each panel is split into two effects: the first effect being the effect of only football game-day weekends on the outcome per-25000 enrolled students, and the second being the effect of a football game-day weekend that occurs within a moratorium. A game-day weekend is defined as a weekend in which a football game occurs. For example, if a game occurs on a Friday, then Saturday and Sunday will be included in the game weekend. Note that weekends are defined as Friday/Saturday/Sunday. "All Game Weekends" includes both home and away games. The effects of game-day weekends + moratorium is identified by 245 football game days that coincide with moratoriums. Controls include holiday, spring semester, day of the week, and university by academic year. Standard errors are clustered by university.

Table E3: Comparison of All Relevant Data Sources

	Data Source			
	Daily Crime Logs	CSS	NIBRS	UCR
Source and Requirement:				
Source of Data:	University police departments	US Department of Education	FBI	FBI
Reporting Mandate:	By-law	By-law	Voluntary	Voluntary
Aggregation and Consistency:				
Level of Aggregation:	Incident-level	Yearly	Incident-level	Monthly
Fraction Reporting Consistently:	1.00	1.00	0.24	0.78
Offenses Reported and Location:				
Alcohol Violations:	All incidences reported to or by the university police.	All incidences reported to or by any university entity.	Arrests only	None
Sexual Assaults:	All incidences reported	All incidences reported	All incidences reported	Hierarchy rule
Residence Hall Information:	No	Yes	No	No
Analysis in Paper:	Main analysis	Substitution of partying	Spillovers of partying	Not used

Note:

Appreviations of the data sources are as follows: Campus Safety Security (CSS), National Incidence-based Reporting System (NIBRS), Uniform Crime Report (UCR). The Daily Crime Logs are used for the main analysis due to the advantages it has over the other sources. The fraction reporting consistently refers row corresponds to the fraction of the sample university police departments. For the NIBRS however, the fraction reported consistently refers to the number of university-specific and corresponding nearby police departments that report consistently. The hierarchy rule is a classification rule by the UCR where only the most serious crime in an incident is reported. While over 50 percent of UCR data is recorded to be reported consistently, the true percentage is difficult to know since NAs and 0s are treated as equivalent in the data.

Table E4: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults (Poisson)

				Specification (2)	
				Weekends	Weekdays
	(1)	(2)	(3)	(4)	(5)
Panel A: Alcohol Offenses					
In Moratorium	-0.216**	-0.305***	-0.328***	-0.328***	-0.247
	(0.093)	(0.087)	(0.104)	(0.092)	(0.161)
Observations	55115	54151	52541	22578	29823
Mean of Dependent Variable	0.524	0.524	0.524	0.939	0.211
Panel B: Sexual Assaults					
In Moratorium	-0.164**	-0.199*	-0.187	-0.388***	-0.016
	(0.076)	(0.110)	(0.117)	(0.147)	(0.141)
Observations	55115	52905	50077	21775	28003
Mean of Dependent Variable	0.051	0.051	0.051	0.062	0.043
FE: Day of Week	X	X	X	X	X
FE: Holiday	X	X	X	X	X
FE: Game Day	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X	X	X	X
FE: University	X				
FE: Academic Year	X				
FE: University by Academic Year		X		X	X
FE: University by Academic Year by Semester			X		

Note:
Standard errors are clustered by university and each offense is defined as a count. Observation values may vary between specifications due to no variation with particular fixed effects. Specification (2) is the preferred specification due to the flexibility of the fixed effects and the conservativeness of the estimates in the main results. A weekend is defined as Friday-Sunday while a weekday is defined as Monday-Thursday.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table E5: Effect of Moratoriums on Alcohol Offenses and Sexual Assaults (WLS)

				Specification (2)	
				Weekends	Weekdays
	(1)	(2)	(3)	(4)	(5)
Panel A: Alcohol Offenses					
In Moratorium	-0.128***	-0.129**	-0.131**	-0.243**	-0.042
	(0.046)	(0.050)	(0.049)	(0.103)	(0.030)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.464	0.464	0.464	0.828	0.190
Wild Bootstrap P-Value	0.005	0.006	0.010	0.006	0.170
Panel B: Sexual Assaults					
In Moratorium	-0.007*	-0.008*	-0.008	-0.019**	0.000
	(0.004)	(0.005)	(0.005)	(0.008)	(0.005)
Observations	55115	55115	55115	23643	31472
Mean of Dependent Variable	0.049	0.049	0.049	0.058	0.042
Wild Bootstrap P-Value	0.062	0.095	0.121	0.030	0.989
FE: Day of Week	X	X	X	X	X
FE: Holiday	X	X	X	X	X
FE: Game Day	X	X	X	X	X
FE: Semester (Spring/Fall)	X	X		X	X
FE: University	X				
FE: Academic Year	X				
FE: University by Academic Year		X		X	X
FE: University by Academic Year by Semester			X		

Note:
Estimates are obtained using WLS. All regressions are weighted by total enrollment. Standard errors shown in parenthesis are clustered by university (37 clusters) and each offense is defined as per-25000 enrolled students. P-values from 1000 wild cluster bootstrap iterations are shown for the In Moratorium coefficient as suggested by Cameron, Gelbach, and Miller (2008) in cases with a small number of clusters (typically lower than 30). This analysis is near, but not below this threshold. Game Day controls consist of university football games within each university. Weekends include Friday-Sunday while Weekdays include Monday-Thursday. Column 2 is the preferred specification due to the flexibility of the fixed effects and the conservativeness of the estimates. Significance stars correspond to clustered standard errors.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Gunshot Noise and Birth Outcomes

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Abstract

Gun violence is ubiquitous across the United States, with gun-related deaths reaching an all-time high in 2021. The prevalence of gunfire results in loud and potentially stress-inducing sounds, which may adversely affect critical stages of in utero development. However, gunfire is largely unreported, creating a unique challenge for researchers to understand its consequences. In this paper, we mitigate this shortcoming by leveraging data from ShotSpotter—an acoustic gunshot technology which uses an array of sensors placed on city structures to detect the sound of gunfire. We combine this unique data source with the universe of births from nine California cities, each matched to a mother's residence. Using the variation in gunfire detections from ShotSpotter at the census-block level, we employ a difference-in-differences methodology and find that gunshot noise creates substantial increases in very low birth weight ($<$ 1,500 grams) and very pre-term births ($<$ 32 weeks). These effects are driven by times of the day when mothers are likely to be at-home, and are particularly concentrated among mothers with low levels of education. These results suggest that gunshot noise is a major factor contributing to the income inequities in pregnancy outcomes.

JEL Codes: I14, I18, K42

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1 Introduction

In 2021, the United States experienced nearly 700 mass shootings, an increase of roughly 150% relative to 2014 (Gun Violence Archive, 2023). Despite the growing prevalence in gun-related violence, interest groups and lobbyists alike continue to halt any groundbreaking legislation that would prohibit gun ownership. In effect, gun violence is likely to continue indefinitely.

Although a growing body of scholarly work documents the adverse impact of large mass shooting events on birth outcomes (Dursun, 2019; Banerjee and Bharati, 2020; Currie et al., 2023), substantially less research exists demonstrating the effect of typical, every-day, gun violence on an infant's health. The mere sound of gunshots can profoundly shape a pregnant individual's perception of safety and future victimization risk, thus influencing their psychological well-being and altering their daily routines. Consequently, this added stress during in utero can significantly affect a mother's health, and in-turn, the health of their child. However, as gunshot noise remains largely under-reported (Carr and Doleac, 2016), there exists little empirical evidence demonstrating its effects on birth health.

In this study, we overcome the challenge of under-reported gunshots by leveraging novel data obtained through ShotSpotter, an automated gunshot detection technology which utilizes sensors placed on buildings and streetlamps, thereby circumventing the need for civilian reporting. We geographically map these gunshot detections to census-block locations of mothers using restricted-access information on the universe of births in nine California cities. In effect, we are able to compare the infant health of mothers that experience gunshots during pregnancy to mothers that do not, while accounting for the expected differences between census-blocks, times of the year, and mother characteristics.

The findings show that in utero exposure to gunshot noise is linked to a higher incidence of very preterm deliveries (< 32 weeks) and very low birth weights (< 1,500 grams). These effects are primarily concentrated among mothers with relatively low levels of formal education (< bachelor's degree), underscoring the heightened vulnerability of underprivileged persons to the adverse effects of gunshot noise. Moreover, we find that the time-of-day the gunshot occurs is directly linked to detrimental birth outcomes—mothers that experience gunshots during times of day when they are likely to be home (e.g., outside 9:00am-5:00pm) are most affected, therefore implying that mothers must hear the gunshot to receive the corresponding stress it creates. These results are also robust to compositional changes in the profile of mothers giving birth.

In obtaining these findings, this research makes significant contributions to several areas of scholarly work. First, we expand on the existing body of literature studying the negative outcomes of crime exposure in utero. While previous studies in this space have largely relied on broadly defined county-level exposures (Dursun, 2019; Banerjee and Bharati, 2020), we enhance geographic precision by utilizing highly localized exposure data at the census block group level. Second, we add to the literature on the broad health impacts of gun-related violence. Existing research often focused on highly acute and rare instances of gun-related crime, such as mass shootings and school shootings (Soni and Tekin, 2020; Cabral et al., 2020). We complement this body of work by examining the impact of a relatively less acute, yet more frequent, type of gun-related stress in gunshot noise. Lastly, we add to the scholarship leveraging ShotSpotter as a unique data source to get a more accurate measure of gunshot frequency. Previous works have used ShotSpotter as a measure of underlying crime (Carr and Doleac, 2016, 2018) or as a novel way to measure police mistrust (Ang et al., 2021). Here, we utilize the unique functionality of this technology in order to get a more accurate measure of exposure to gunshots.

The sections of this paper are organized as follows: Section II discusses the relevant literature, Section III describes the data used, Section IV explains the identification strategy, Section V outlines the results, and Section VI concludes.

2 Literature Review

2.1 In Utero Stress and Birth Outcomes

Health at birth serves as the first measure of human capital accumulation. This is because health at birth affects one's health in adolescence and adulthood as well as a range of later life non-health outcomes, including educational attainment (Royer, 2009). An important mechanism explaining this connection, extensively explored in recent economic literature, is the *fetal programming hypothesis* (Almond and Currie, 2011; Almond et al., 2018). The hypothesis was initially developed by epidemiologist David Barker to connect nutritional deficiencies in utero with increased susceptibility to adverse later life outcomes. However, it has been increasingly applied in the context of psychological stress experienced by a mother as a result of exposure to events such as natural disasters, acts of terrorism, armed conflict, mass shootings, domestic violence, the loss of a family member, and even sporting events (Torche, 2011; Brown, 2018; Camacho, 2008; Quintana-Domeque and Rodenas-Serrano, 2017; Dursun, 2019; Mansour and Rees, 2012; Currie et al., 2022; Duncan et al., 2016).

It is worth noting that a handful of studies connects exposure to in utero stressors to improvements in birth outcomes (Torche and Villarreal, 2014; Lichtman-Sadot et al., 2022). Stress can lead mothers to adopt health enhancing behaviors, such as frequent prenatal visits or choosing to remain at home and rest instead of commuting to work. However, these studies report different effects across socio-economic status, suggesting that this may be a relevant dimension impacting one's vulnerability to stress as well as availability of coping strategies.

2.2 Effects of Crime Victimization and Exposure

The literature extensively documents the adverse impacts of crime on individuals who are directly victimized. Physical health is directly affected by victimization if the criminal act results in bodily injury. Even in the absence of bodily harm, victims often experience mental health distress (Cornaglia et al., 2014). Moreover, crime victimization has been shown to influence non-health-related outcomes, including career trajectories (Bindler and Ketel, 2022).

In addition to affecting victims, crime also affects individuals who strongly identify with the victims, such as those residing in the same area or belonging to the same racial group (Powdthavee, 2005; Bindler et al., 2020). However, it is essential to acknowledge that for a crime incident to influence the outcomes of individuals not directly victimized, there must exist a mechanism for these individuals to learn about the crime. Consequently, studies utilizing geographical variation too coarse to allow for direct witnessing of the crime employ media coverage as a pathway. To this end, Banerjee and Bharati (2020) utilize exogenous variation in news coverage of mass shootings to illustrate that media coverage mediates the relationship between mass shootings in the mother's county of residence and birth outcomes. Similarly, Curtis et al. (2021) correlate highly publicized instances of anti-Black violence, as proxied by the number of Google searches, with an increase in worse mental health days among Black Americans.

3 Data

In this section, we outline the two main sources of data: restricted-access birth records in California and ShotSpotter gunshot detections from nine cities in California. As an overview, Figure 1 plots each city in the sample and the corresponding time frame used within the analysis. On average, a city is within the sample for approximately 5.5 years, with the oldest records dating to January 2008 and the most recent December 2020.

3.1 Birth Data

Restricted-access birth records are obtained from the California Department of Public Health. The data covers the universe of births in California and includes information on mothers' address of residence, demographics, as well as pregnancy and infant characteristics (California Department of Public Health, 2022). We geocode all addresses and map to the corresponding census block using the Census Bureau TIGER/Line Shapefiles for the year 2020.

Following recent scholarship on gun violence and birth outcomes (Currie et al., 2023), we define the primary birth outcomes of interest as *Very Low Birth Weight* and *Very Preterm Delivery*. *Very Low Birth Weight* is an indicator variable that takes the value of one when an infant's birth weight is less than 1,500 grams (Daysal et al., 2022), whereas *Very Preterm Delivery* is an indicator variable that takes the value of one when the gestation length is less than 32 weeks (World Health Organization, 2018). In addition to these primary measures, we also create four secondary birth outcomes: *Birth weight*, *Low birth weight*, *Gestation length*, and *Preterm delivery*. *Birth weight* captures an infant's weight at birth in grams; *Low Birth Weight* is an indicator variable that take the value of one when an infant's birth weight is less than 2,500 grams (World Health Organization, 2014); *Gestation length* captures the gestation length in weeks; *Preterm delivery* is an indicator variable that take the value of one when the gestation length is less than 37 weeks (World Health Organization, 2018).

3.2 Gunshot and Crime Data

We obtain gunfire data from nine cities within California that have ShotSpotter technology implemented: Bakersfield, East Palo Alto, Fresno, Oakland, Richmond, San Diego, San Francisco, San Pablo, and Stockton. These nine cities represent all cities within California that meet the following criteria: ShotSpotter must have been implemented between years 2000-2020, the city must have agreed to release location-based information on ShotSpotter detections via a California Public Record Act request, the city must reside in California.[1] The resulting sample of cities corresponding counties can be seen in Figure 2. Of the nine cities, 55% are located in northern California.[2]

To identify the occurrence of gunfire, we exploit each city's utilization of ShotSpotter tech-

[1]For some select cities, we use ShotSpotter data collected by the Justice Tech Lab whom obtain the data through California Public Record Act requests (Carr and Doleac, 2016, 2018).

[2]While ShotSpotter technology is in over 150 cities world-wide, there is a high representation within northern California where its parent company, SoundThinking, is located.

nology. ShotSpotter is an acoustic gunfire detection software which uses an array of microphones and sensors placed on streetlamps and buildings across the city to identify, locate, and rapidly dispatch police to the sound of gunfire. The sensors are wirelessly connected to servers and are equipped with machine-learning technology in order to detect the sound of gunfire. Given that only 12% of gunfire is reported by civilians (Carr and Doleac, 2016), ShotSpotter represents a novel way to more closely capture the true incidence of gunfire, bypassing the reliance on human reporting.

The technology utilizes a machine-learning algorithm in order to decipher gunshot noises from other sounds such as fireworks or car-backfires. Once a sensor detects a gunshot, a recording of the sound is forwarded to ShotSpotter's 24-hour review center, where an expertly trained employee checks the recording for false positives. After confirmation, the information is sent to the police communications department where police officers are subsequently dispatched.[3] These dispatches are assigned a unique identification code which classifies the dispatch as ShotSpotter-initiated.

ShotSpotter sensors cannot capture every instance of gunfire, although the company claims that the sensors have 97% accuracy with a 0.5% false-positive rate. One field study finds a lower accuracy rate of roughly 81%, although the study was conducted in ShotSpotter's infancy (Goode, 2012; Irvin-Erickson et al., 2017; Mazerolle et al., 1998). Over the past decade, both the company and police departments claim that the technology has greatly improved. Moreover, although not specifically verifying the accuracy, studies have shown that ShotSpotter implementation results in greater numbers of gun-related dispatches (Mares and Blackburn, 2021). Hence, ShotSpotter detections represent a way to more precisely measure the presence of gunfire in comparison to 911 reports.

[3]This entire process, according to SoundThinking, ShotSpotter's parent company, takes under 60 seconds.

3.3 Sample Restrictions and Summary Statistics

We impose several restrictions on the sample. First, to successfully match ShotSpotter exposures to birth outcomes, we restrict to mothers residing in the nine cities for which we have access to both birth data and gunshot data: Bakersfield, East Palo Alto, Fresno, Oakland, Richmond, San Diego, San Francisco, San Pablo, and Stockton.

As a second restriction, we exclude mothers residing in census blocks which have not adopted ShotSpotter and therefore lack information on gunshots. Within a city, ShotSpotter sensors tend to be implemented in areas that experience relatively higher incidence of gun violence than areas in which ShotSpotter is not implemented. Therefore, areas without ShotSpotter sensors may not be comparable to areas with access to ShotSpotter sensors.

Third, we include only mothers from whom the gestation period matches availability of ShotSpotter data (i.e., expected conception month falls on or after the first month when ShotSpotter data is available, and expected birth month falls before or on the last month when ShotSpotter data is available).[4] In doing so, we make certain that we have data on ShotSpotter alerts throughout the entire duration of pregnancy, as mothers whose pregnancy period only partially matches availability of ShotSpotter data in their census block of residence could be erroneously misclassified as not exposed to gunshot simply due to lack of data availability.

Finally, we focus solely on mothers between 15 and 49 years old, as well as on singleton pregnancies.[5] Low or high maternal age as well as twin, triplets and higher plurality pregnancies are typically associated with lower birth weights than otherwise comparable pregnancies.

Table 1 summarizes the birth outcomes and demographic characteristics of mothers in the sample. In Panel A, we create summary statistics for the birth outcomes of interest. The two main outcomes, Very Preterm and Very Low Birth Weight, are rare among mothers—roughly 1.4% and 1.1% of infants are born with such conditions. Gestation length is approximately 38

[4] Expected conception month refers to the month obtained by subtracting gestation length from actual month of birth. Expected birth month refers to the month obtained by adding 10 months to the expected conception month.

[5] We also exclude births for which gestation length, weight at birth, birthdate, or maternal address of residence is unknown.

weeks on average, while infants are generally born weighing 3,299 grams (7.2 lbs).

In Panel B of Table 1, we present summary statistics of the main control variables describing the observable characteristics of the mothers. An average mother in the sample is approximately 29 years old, although mothers can be as young as 15 or as old as 48. Nearly 17% of mothers in the sample have completed at least a bachelor's degree, and nearly 60% of mothers identify as Hispanic. The sample is diverse with roughly 12% of mothers Asian, 15% Black, and 53% White.

4 Empirical Strategy

4.1 Baseline Specification

To evaluate the effect of gunshot noise on birth outcomes, we estimate the following regression model using OLS:

$$Y_{ict} = \beta ShotSpotter_{ct} + \gamma X_{ict} + \pi_c + \rho_t + \upsilon_{ict} \tag{1}$$

where Y_{icy} denotes birth outcome of mother i residing in census block c, with a child conceived in month and year t. The primary treatment variable, $ShotSpotter_{ct}$, is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c during pregnancy that started in month and year t. This represents around 50% of mothers who experience exposure to gunshot noise during pregnancy within the sample. Pregnancy is defined in terms of expected, not actual, date of birth. We obtain conception month and year by subtracting the length of gestation from the actual birth month and year. We then calculate expected birth month and year by adding ten months to the conception date. We use expected birthdate because the actual birthdate might be endogenous to gunshot noise, as the likelihood of exposure is lower with shorter pregnancies (Currie et al., 2023). X_{ict} is a vector of observable maternal characteristics such as, age, education, sex of the infant, race, ethnicity, and the number of prior live births (see Table 1). We include census-block fixed effects π_c in order to account for time-invariant differences, such as crime, that may persist across neighborhoods. Additionally, year-by-month fixed effects ρ_t allow us to account for the expected differences across different times of the year. Finally, υ_{ict} is the error term, and standard errors are clustered at the census block level to account for the serial correlation that may occur within blocks. Taken together, the model compares mothers that experience gunshot noise during pregnancy to mothers that do not experience gunshot noise, while accounting for the expected differences between census blocks, times of day, and mother characteristics.

The coefficient of interest is β, which captures the average effect of in utero exposure to gunshot noise (as detected by ShotSpotter) during pregnancy on a mother's birth outcome. However, in order for β to be interpreted as casual, three main assumptions must be met; mothers that experience gunshots would have had similar birth outcomes as mothers that did not experience gunshots, the number ShotSpotter gunshot detection devices are stable throughout the time period, and the composition of births is not changing.

For the first assumption that birth outcomes would be similar without the exposure to gunshots, we conduct a placebo test wherein we estimate whether gunshots experienced *after* the delivery of a child affect birth outcomes. In particular, we test whether mothers that experience gunshots only after their birth exhibit similar changes in their birth outcomes as mothers that experience gunshots during pregnancy. As discussed further in Section 5.2, we find little evidence of mothers displaying worse birth outcomes without in utero exposure to gunshots.

The second assumption is that ShotSpotter gunshot detection devices are stable throughout the sample period. If cities were to increase the number of gunshot detection sensors, then it is possible that gunshot *detections* become more prevalent while the underlying number of gunshots remains constant. However, we mitigate this concern by restricting the sample within each city to only periods where there is no increase in the amount of coverage.[6]

Next, the third assumption is that there is no change in the composition of births. For

[6]Specifically, for the city of Fresno, we had to omit any data later than 2018 as it went through a large expansion of the technology.

instance, it is well documented that Black mothers tend to have worse birth outcomes than non-Black mothers (Office of Minority Health, 2022). If exposure to treatment induces more Black mothers to conceive and/or carry the pregnancy to term, we could report an increase in adverse birth outcomes despite mothers not experiencing higher risk due to gunshot exposure. To test this, we show that in utero exposure to gunshot noise does not predict the sex of the infant, whether the mother is giving birth to her first infant, maternal age, whether the mother is Hispanic, Black, or has a bachelor's degree or more (Table 2).

Finally, it is important to note that β does not enable us to directly delineate the effects of *hearing* a gunshot noise and the effect related to the police dispatch cars that follow. Although it has been shown that increased police presence is associated adverse birth outcomes among minority mothers (Hardeman et al., 2021), we emphasize that gunshots can be heard within a large radius where a listener may not see the following police dispatch.

5 Results

5.1 Main Results

Table 3 presents the effects of gunshot noise during pregnancy, estimated using Equation 1, on Very Low Birth Weight (birth weight <1,500 grams) in Panel A and Very Preterm Delivery (gestation length <32 weeks) in Panel B. We find that gunshot noise causes large increases in these outcomes for mothers with low education (no bachelor's degree), and they are driven by times of the day when mothers are likely to be at home.

Contrary to the initial hypothesis that gunshot noise should affect all mothers, we do not find any evidence of in utero exposure to gunshot noise affecting birth outcomes when including all mothers in the sample (Column 1). However, this does not necessarily imply that exposure to gunshot noise does not result in psychological stress. In particular, stress from gunshot noise may result in two simultaneous, and confounding effects; on one hand, some mothers may seek to mitigate their enhanced stress levels with extra prenatal care and doctors visits, thus improving their birth outcomes. On the other hand, other mothers may not be able to adopt behaviors and leverage resources to mitigate the negative consequences of resulting stress. Hence, these two confounding effects may drive the initial null result we find.

Columns 2 and 3 of Table 3 separate subgroups that may respond differently to the stress of gunshot noise: mothers with low levels of formal education (below bachelor's degree), and high levels of formal education (bachelor's degree and above). Consequently, the results show that at least one gunshot during pregnancy is associated with a 30% increase in the incidence of very low birth-rates and a 33% increase in very preterm deliveries relative to the mean for low education mothers only—high education mothers show little evidence of an effect. These results for low education mothers are statistically significant at the 10% and 5% level respectively. Hence, it may be that low education mothers have less access to resources that could be used to mitigate the adverse effects of stress, and in-turn, their birth outcomes are hindered. We further explore this potential mechanism in Section 5.3.

In Columns 4 through 7, we separate the effects of gunshot noise on high and low education mothers by the time of day in which the gunshot is detected—Working Hours (9:00am-5:00pm) and Non-Working Hours (before 9:00am or after 5:00pm). This is motivated by the notion that if a gunshot occurs and a mother is not home to hear it, then they should not receive the corresponding increase in stress. Indeed, we find no evidence of working hour gunshots affecting birth outcomes of mothers with either low or high levels of education (Columns 4 and 5). Conversely, gunshot effects on low education mothers appear to be driven by exposures to gunshots that occur outside working hours. In particular, Column 6 shows that at least one non-working hour gunshot during pregnancy is linked to an increase in the incidence of both very low birth weights and very preterm deliveries by 34% and 28% relative to the mean, respectively. These results are statistically significant at the 5% and 10% level, respectively. Hence, these results further demonstrate that salience is key to gunshot noise acting as a stressor—mothers must be in the vicinity of the gunfire in order to receive the stress it creates.

It is interesting to note the magnitude of the effects reported for mothers with low levels of education are substantial compared to previous literature. For reference, Dursun (2019) finds that a mass shooting in mother's county of residence during pregnancy is linked with an 8% increase in the incidence of very low birth weights and a 7% increase in the incidence of very preterm births. The effects we report here are approximately four times as large as the ones reported by Dursun (2019). Although gunshot noise is a less acute stressor than a mass shooting, our data enables us to capture the effects of gunshot noise occurring within a very close proximity to the mother's residence, i.e., at a census block level, as opposed to county level.

5.2 Placebo Test

To verify that trends in unobservable characteristics associated with gunshot noise do not affect birth outcomes, we conduct a placebo test estimating the effect of gunshots occurring after expected delivery on birth outcomes. Exposures after delivery should have no effect on birth outcomes, as birth has already taken place. We focus the placebo test on mothers with low levels of formal education and exposures to non-working hours gunshots, as this is where our results are concentrated.

To conduct the placebo test, we estimate the following model:

$$Y_{ict} = \alpha + \sum_{x=1}^{3} \beta_x ShotSpotter_{ct,x} + \gamma X_{ict} + \pi_c + \rho_t + \upsilon_{ict} \quad (2)$$

where $ShotSpotter_{ct,1}$ is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c during pregnancy that started in month and year t; $ShotSpotter_{ct,2}$ is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c up to 10 months after delivery; $ShotSpotter_{ct,3}$ is a binary variable equal to one if at least one ShotSpotter alert has been reported in census block c between 11 and 20 months after delivery. The model is estimated using the original sample restricted to only the mothers for whom both the expected gestation period and the period up to 20 months following expected delivery falls within the period of availability of the ShotSpotter data within their census blocks of residence.

Figures 4 and 5 document the results of the placebo test for Very Low Birth Weight and Very Preterm respectively. "Pregnancy" denotes the effect of exposure to at least one non work-time gunshot during pregnancy; "After Delivery I" denotes the effect of exposure to at least one non work-time gunshot up to 10 months after expected delivery; "After Delivery II" denotes the effect of exposure to at least one non work-time gunshot from 11 through 20 months after expected delivery. Circles represent the point estimates, and the error bars denote the 95% confidence intervals.

The results are qualitatively similar to the baseline results for exposures during pregnancy. At the same time, we find no evidence of post-trends as the effects of gunshot exposures after expected delivery on very low birth weights and very preterm deliveries.

5.3 Mechanism

Although the results show that gunshot detections during pregnancy result in worse birth outcomes for low-educated mothers, it is important to understand why this does not appear to be the case for high-education mothers. As briefly discussed in Section 5.1, it could be that mothers with high education seek care to mediate their higher stress levels. In this subsection, we test this hypothesis by testing whether exposure to gunshots results in high-education mothers experiencing more prenatal care (as measured by visits), or alternatively, whether low-education mothers exhibit a higher likelihood of risky coping behaviors (smoking).

Table 4 presents the results when we estimate Equation 1 with three new outcomes: a binary variable for whether a mother engaged in smoking behavior during pregnancy (Columns 1 and 4), the number of prenatal care visits (Columns 2 and 5), and the probability of initiating care after the first trimester (i.e., all care is received within trimester 2 or 3). In Columns 1 and 4, we test whether mothers engage in smoking more frequently when hearing gunshots. We find little evidence of such behavior for both high and low education mothers—the point estimates are small and not statistically significant. However, it is important to note that smoking is a self-reported measure. Moreover, Columns 2 and 4 estimate the model for the number of prenatal care visits, while Columns 3 and 6 for delayed care. However, in each of the specifications, we find little evidence of an effect on prenatal visits or late visit following in utero exposure to gunshot noises—the marginal effects are close to zero and precisely estimated for each subgroup.

Given the results, it is notable that prenatal visits are an imperfect measure of mothers' mediating their stress. For instance, it is difficult to know whether a stressed mother will seek more care (out of fear for the child) or less care (out of inability to cope). Therefore, the null results we find may be a composition of these two effects, thus providing an unclear story on how mothers effectively cope with more stress. Moreover, the number of prenatal visits is self reported by mothers *after* their delivery—a question that can be difficult to recall and subject to considerable bunching. Indeed, as shown in Figure 6, the number of prenatal visits exhibits clear bunching at 10 and 12.

5.4 Other Birth Outcomes

Table 5 shows the results when equation (1) is used to predict the effects of gunshots on supplementary birth outcomes of mothers with low levels of formal education: birth weight (in grams), low birth weight, gestation length (in weeks), and preterm delivery. We find no evidence of adverse effects on these secondary birth outcomes.

6 Conclusion

Gun-related violence has surged in recent years, with both high-profile mass shootings and less severe gun-related crimes on the rise across the country. However, tracking the health implications of the latter has been a challenge for researchers due to under-reporting. This paper overcomes this obstacle by leveraging novel data from ShotSpotter, an automatic gunshot detection technology. Through combining ShotSpotter's gunshot data with the universe of birth records in nine cities in California, we document the intergenerational impact of exposure to gunshots noise in utero.

Despite gunshots representing a relatively less acute form of stress, we document significant adverse effects on birth outcomes, particularly pronounced in infants born to mothers with lower levels of education. The magnitude of these effects is substantial, and the effects are driven by exposures during non-working hours, when mothers are more likely to be physically at home, and able to hear the corresponding noise.

One of the key strengths of this study is that we manage to identify exposures at a fine geographic level, potentially allowing for direct exposure to the gunshot noise. In contrast, much prior work in the birth outcome space has relied on county-level exposures, which likely involve mothers learning about the crime incident through the news media instead of witnessing it directly. At the same time, the study has several important limitations. First, we focus on mothers residing in a sample of California cities. This both reduces statistical power but also poses challenges for external validity. Future research should strive to collect a broader range of data, potentially spanning other cities. Furthermore, as mentioned before, the estimates capture the effect of stress associated with both gunshot noise and increased police presence. Future work should aim to collect data on police dispatches to particular locations to isolate these effects. Despite these limitations, this research provides evidence of adverse consequences of exposure to even the less severe manifestations of gun violence, emphasizing the critical need for policymakers to target efforts to mitigate it.

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Tables

Table 1: Summary Statistics

	Mean	Std.Dev.	Min	Max
Panel A: Birth Outcomes				
Very Preterm	0.014	0.115	0	1
Very Low Birth Weight	0.011	0.105	0	1
Preterm	0.084	0.277	0	1
Low Birth Weight	0.064	0.245	0	1
Gestation Length (weeks)	38.688	2.079	1	48
Birth Weight (grams)	3,299.841	571.560	68	6,350
Panel B: Controls				
Age	28.531	6.322	15	49
Bachelors or Higher	0.168	0.374	0	1
Hispanic	0.595	0.491	0	1
Asian	0.125	0.331	0	1
Black	0.153	0.360	0	1
White	0.539	0.499	0	1
First time Mother	0.388	0.487	0	1
Male Infant	0.509	0.500	0	1
Total Observations:	38,373			

Note:
Included mothers are those that reside in the nine California cities for which locational gunshot data occurs. Mothers must reside in a census block which are known to have the ShotSpotter technology during their gestation period. Mothers of ages 15 to 49 are included, as well as only singleton pregnancies. Panel A contains the two primary outcomes of interest: Very Preterm (< 32 weeks) and Very Low Birth Weight (< 1,500 grams). Four other less serious birth outcomes are also provided: Preterm, Low Birth Weight, Gestation Length (weeks), and Birth Weight (grams). In Panel B, controls used in Equation 1 are presented.

Table 2: Effect of Gunshot Noise on Controls (OLS)

	Male Infant	First-time Mother	Mother's Age	Hispanic	Bachelors or Higher	Black
	(1)	(2)	(3)	(4)	(5)	(6)
Gunshots	0.003	-0.009	0.045	-0.006	-0.003	0.004
	(0.007)	(0.007)	(0.090)	(0.006)	(0.005)	(0.005)
Mean of Dependent Variable	0.509	0.388	28.531	0.595	0.168	0.153
Observations	38,373	38,373	38,373	38,373	38,373	38,373
FE: Census Block	X	X	X	X	X	X
FE: Year by Month	X	X	X	X	X	X

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by census block. All models include census-block and year-by-month fixed effects. Gunshots is a binary variable equal to one if a mother experienced a gunshot as detected through ShotSpotter during their pregnancy. Male Infant is a binary variable equal to 1 if the infant is male. First-time Mother is a binary variable equal to 1 if the pregnancy corresponds to mother's first live birth. Mother's Age denotes mother's age in years. Hispanic is a binary variable equal to 1 if the mother identifies as Hispanic. Bachelor's or Higher is a binary variable equal to 1 if the mother has at least a bachelor's degree. Black is a binary variable equal to 1 if the mother identifies as Black.

Table 3: Effect of Gunshot Noise on Very Low Birth Weight and Very Preterm (OLS)

				Working Hours		Non-Working Hours	
	All Mothers	Low Education	High Education	Low Education	High Education	Low Education	High Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Very Low Birth Weight							
Gunshots	0.039	0.333*	-0.144	-0.190	-0.178	0.383**	-0.257
	(0.139)	(0.176)	(0.286)	(0.213)	(0.966)	(0.181)	(0.266)
Mean of Dependent Variable	1.115	1.125	0.778	1.125	0.778	1.125	0.778
Observations	38,373	27,272	5,622	27,272	5,622	27,272	5,622
Panel B: Very Preterm							
Gunshots	0.182	0.446**	-0.243	0.008	0.872	0.377*	-0.723*
	(0.152)	(0.190)	(0.388)	(0.242)	(1.131)	(0.194)	(0.381)
Mean of Dependent Variable	1.348	1.360	0.976	1.360	0.976	1.360	0.976
Observations	38,373	27,272	5,622	27,272	5,622	27,272	5,622
FE: Census Block	X	X	X	X	X	X	X
FE: Year by Month	X	X	X	X	X	X	X

Note:
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Standard errors are clustered by census-block. Panel A estimates the effect of gunshot noise on Very Low Birth Weight, defined as a birth below 1,500 grams. Panel B, estimates the effect of gunshot noise on Very Preterm Birth, defined as a gestation length less than 32 weeks. The treatment variable, Gunshots, is an indicator equal to one if a gunshot is detected during pregnancy. All coefficients multiplied by 100 for ease of interpretation. Low Education is defined as having less than a bachelor's degree, while High Education is defined as having a bachelor's degree or higher. Moreover, Working Hours is defined as experiencing a gunshot between the hours 9:00am-5:00pm during pregnancy. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

Table 4: Effect of Gunshot Noise on Prenatal Visits (OLS)

	Low Education			High Education		
	Smoking	Visits	Delayed Care	Smoking	Visits	Delayed Care
	(1)	(2)	(3)	(4)	(5)	(6)
Gunshots	0.003	-0.031	0.006	0.001	-0.154	0.007
	(0.002)	(0.065)	(0.007)	(0.002)	(0.141)	(0.011)
Mean of Dependent Variable	0.020	10.963	0.174	0.002	11.113	0.092
Observations	27,232	27,024	27,024	5,615	5,608	5,608
FE: Census Block	X	X	X	X	X	X
FE: Year by Month	X	X	X	X	X	X

Note:
* p < 0.1, ** p < 0.05, *** p < 0.01
Standard errors are clustered by census block. Low education are mothers with less than a bachelor's degree, while High Education are mothers with a bachelor's or higher. The outcome variables are Smoking, Visits, and Delayed Care. Smoking is a binary equal to 1 if a mother engaged in smoking behavior during pregnancy. Visits is a continuous variable denoting the number of prenatal care visits a mother attended. Delayed Care is a binary variable equal to 1 if the initial visit occurs after the first trimester. Specifically, Delayed Care is equal to one if a mother receives care only during trimesters 2 and 3. Gunshots is a binary variable equal to one if a mother experienced a gunshot as detected through ShotSpotter during their pregnancy. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

Table 5: Effect of Gunshot Noise on Other Birth Outcomes (OLS)

	Birth Weight	Low Birth Weight	Gestation Length	Preterm
	(1)	(2)	(3)	(4)
Gunshots	4.002	-0.748*	-0.040	0.063
	(10.068)	(0.442)	(0.037)	(0.518)
Mean of Dependent Variable	3,296.515	6.556	38.646	8.753
Observations	27,272	27,272	27,272	27,272
FE: Census Block	X	X	X	X
FE: Year by Month	X	X	X	X

Note:
* p < 0.1, ** p < 0.05, *** p < 0.01
Standard errors are clustered by census block. Coefficients in columns (2) and (4) multiplied by 100 for ease of interpretation. The sample includes only low-education mothers (no bachelors degree attained). All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1. Gunshots is a binary variable equal to one if a mother experienced a gunshot as detected through ShotSpotter during their pregnancy. Birth Weight is measured in grams, while Low Birth Weight is an indicator equal to 1 if an infant's weight is less than 2,500 grams. Gestation Length is measured in weeks, and Preterm is a binary variable equal to 1 if the gestation length is less than 37 weeks.

Figures

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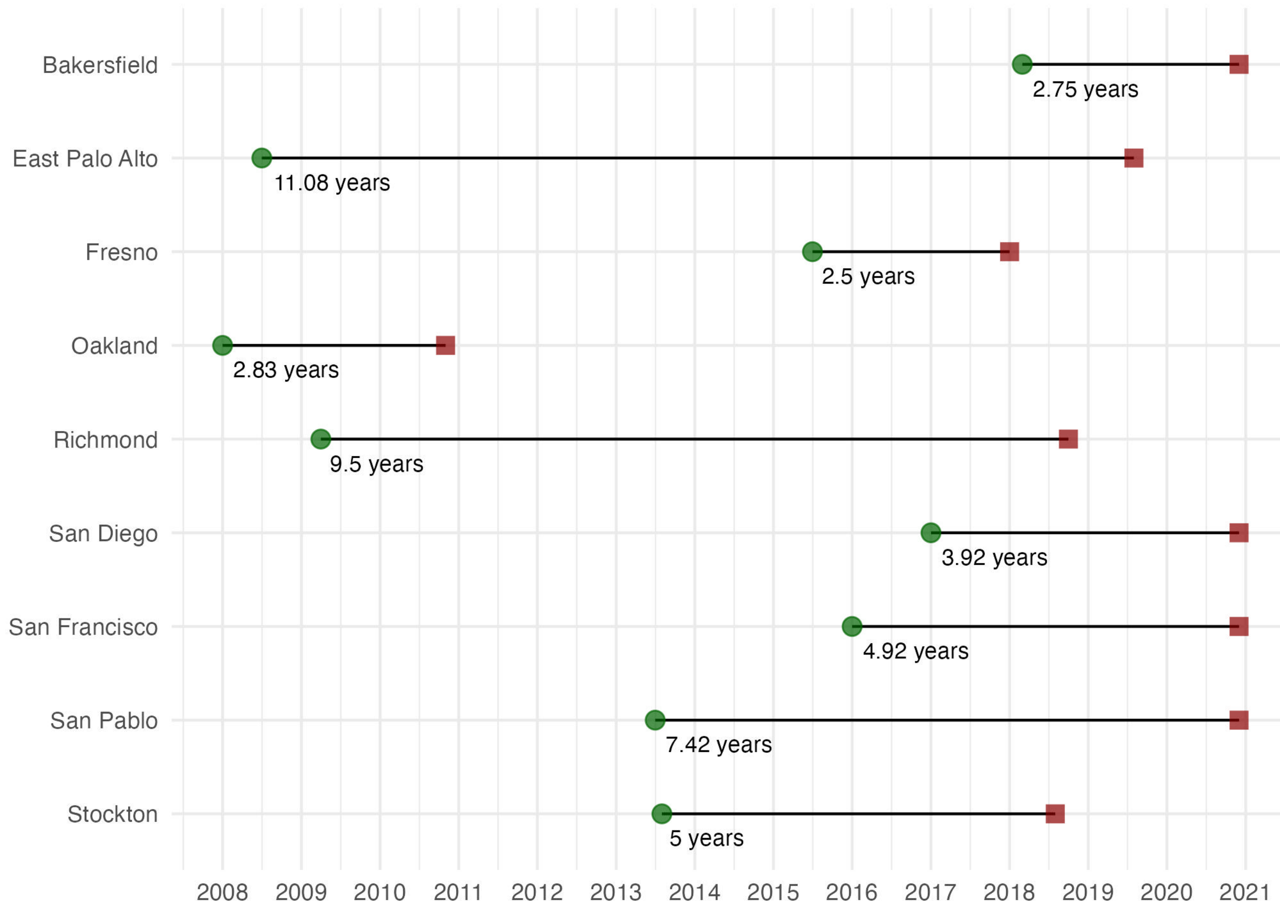

Figure 1: Time Period Utilized for Sample Cities

Note: This figure shows the nine sample cities and the corresponding time period that is analyzed. The entire time period covers from January 1, 2008 through December 31, 2020. While birth data exists for this entire time period, the sample is restricted due to the availability of ShotSpotter gunshot detection data. Hence, the time periods utilized reflect periods in which we have both birth data and gunshot detection data. The average coverage time per-city is 5.5 years.

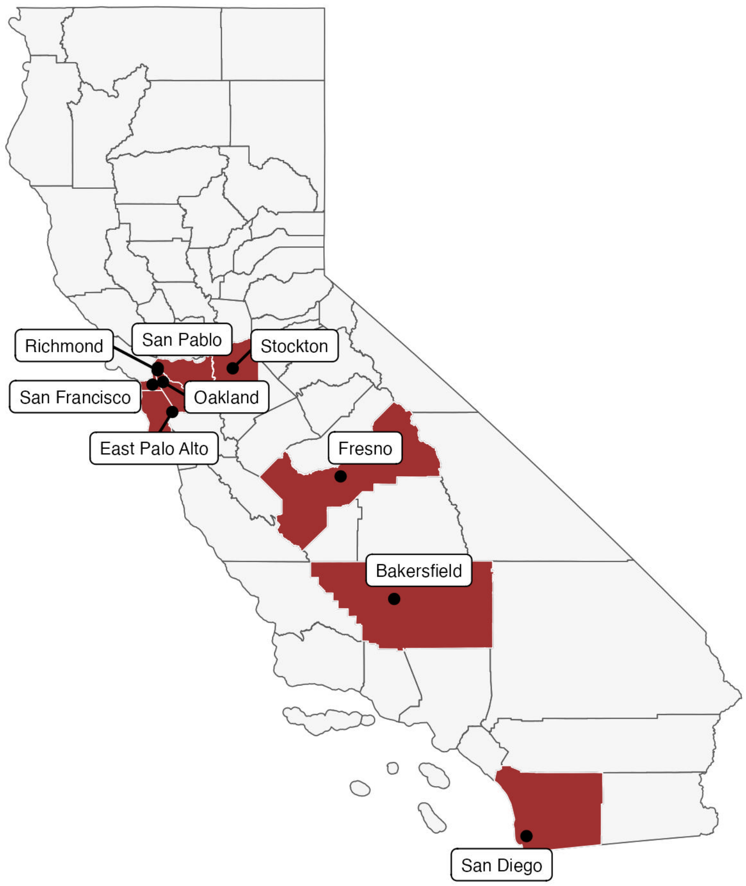

Figure 2: Sample City Locations

Note: This figure shows the location of the nine cities (blue points) that are in the sample. In particular, the nine cities are Bakersfield, East Palo Alto, Fresno, Oakland, Richmond San Francisco, San Diego, San Pablo, and Stockton. The red portion of the map represents the city's residing county.

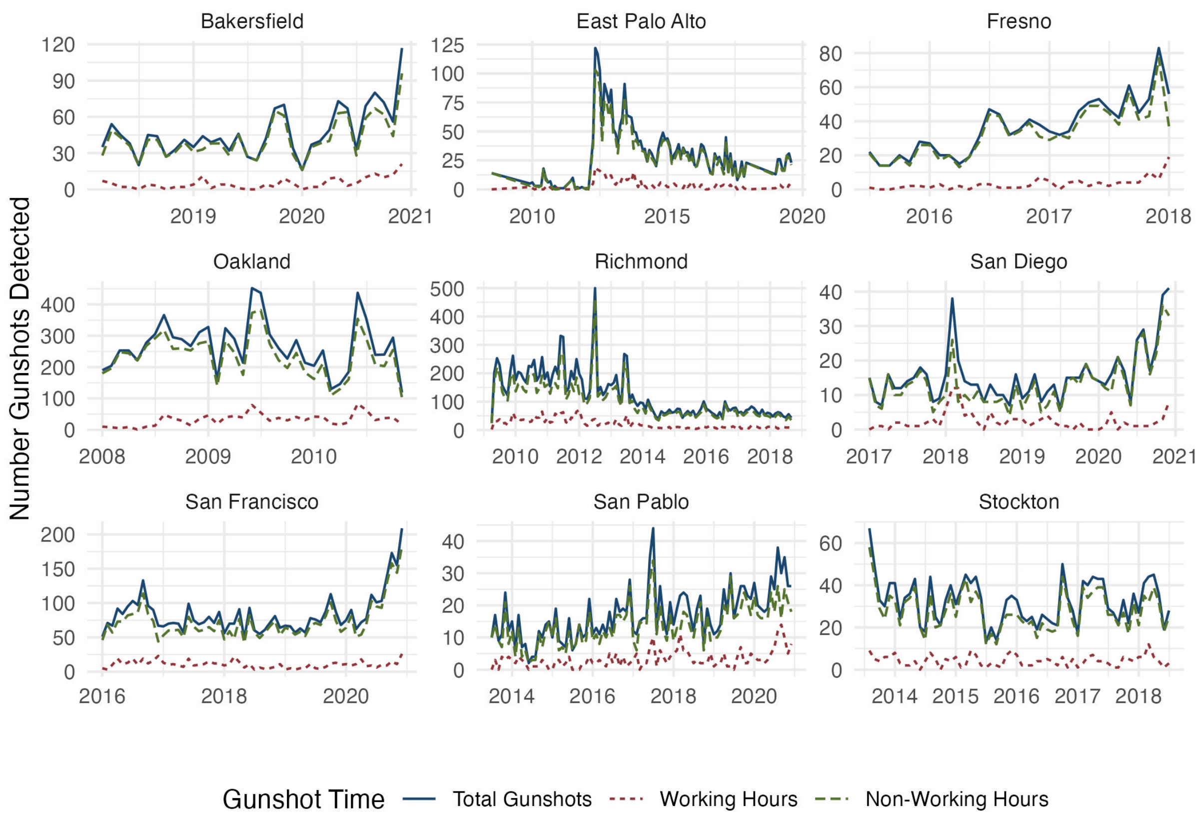

Figure 3: Gunshot Detections Over Time (monthly)

Note: This figure plots the number of gunshots at the monthly level (y-axis) over time. Three lines are shown: Total Gunshots, Working Hours, and Non-Working Hours. Total Gunshots refers to the total number of gunshots detected, while Working Hours and Non-Working Hours refer to the number of gunshots detected during the time periods 9:00am-5:00pm and 5:00pm-9:00am, respectively. Therefore, Total Gunshots is the sum of Working Hours and Non-Working Hours. As shown, there are far fewer Working Hours gunshots. We use this as motivation for analysis in Section 5.

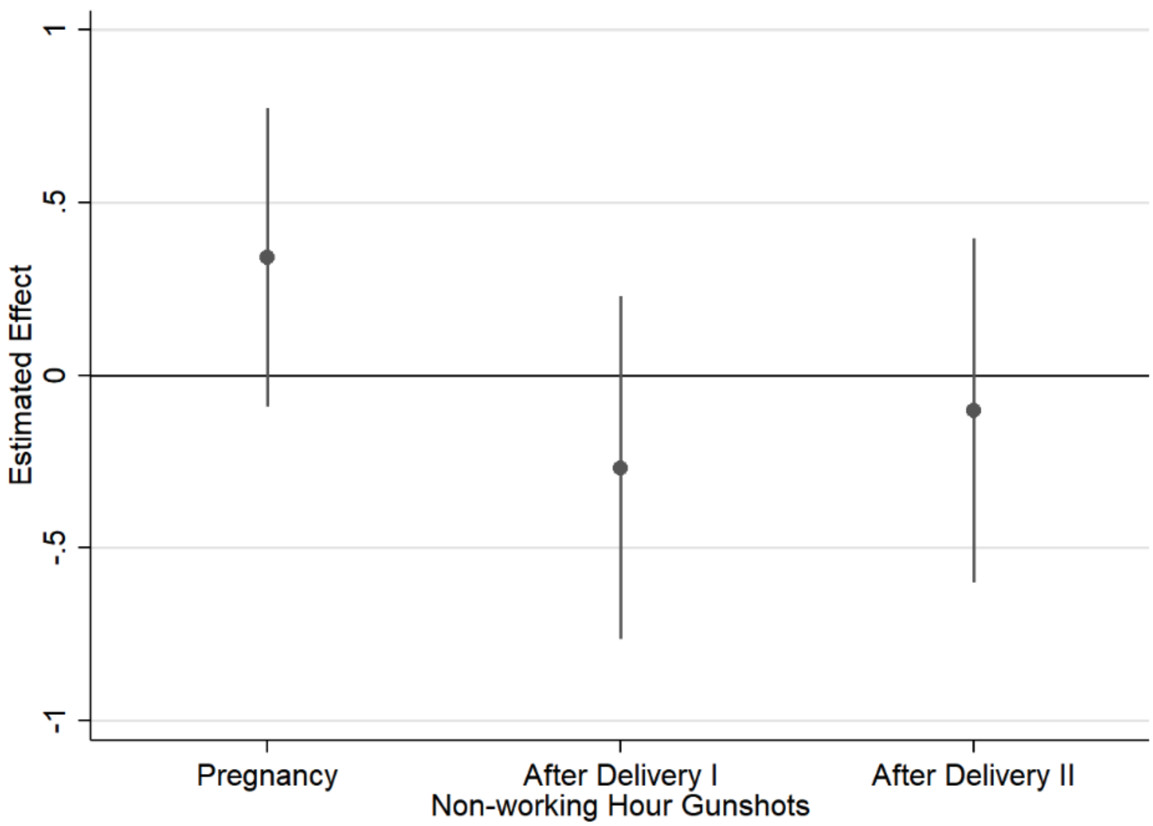

Figure 4: Placebo Test for Very Low Birth Weight

Note: Circles represent the point estimates, and the error bars denote the 95% confidence intervals, estimated using equation (2) and restricted to mothers with low formal education (i.e., below a bachelor's degree). "Pregnancy" denotes the effect of exposure to at least one non work-time gunshot during pregnancy; "After Delivery I" denotes the effect of exposure to at least one non work-time gunshot up to 10 months after expected delivery; "After Delivery II" denotes the effect of exposure to at least one non work-time gunshot from 11 through 20 months after expected delivery. Very Low Birth Weight is defined as a birth below 1,500 grams. Moreover, Non-Working Hours is defined as experiencing a gunshot between the hours 5:00pm-9:00am. All coefficients multiplied by 100 for ease of interpretation. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

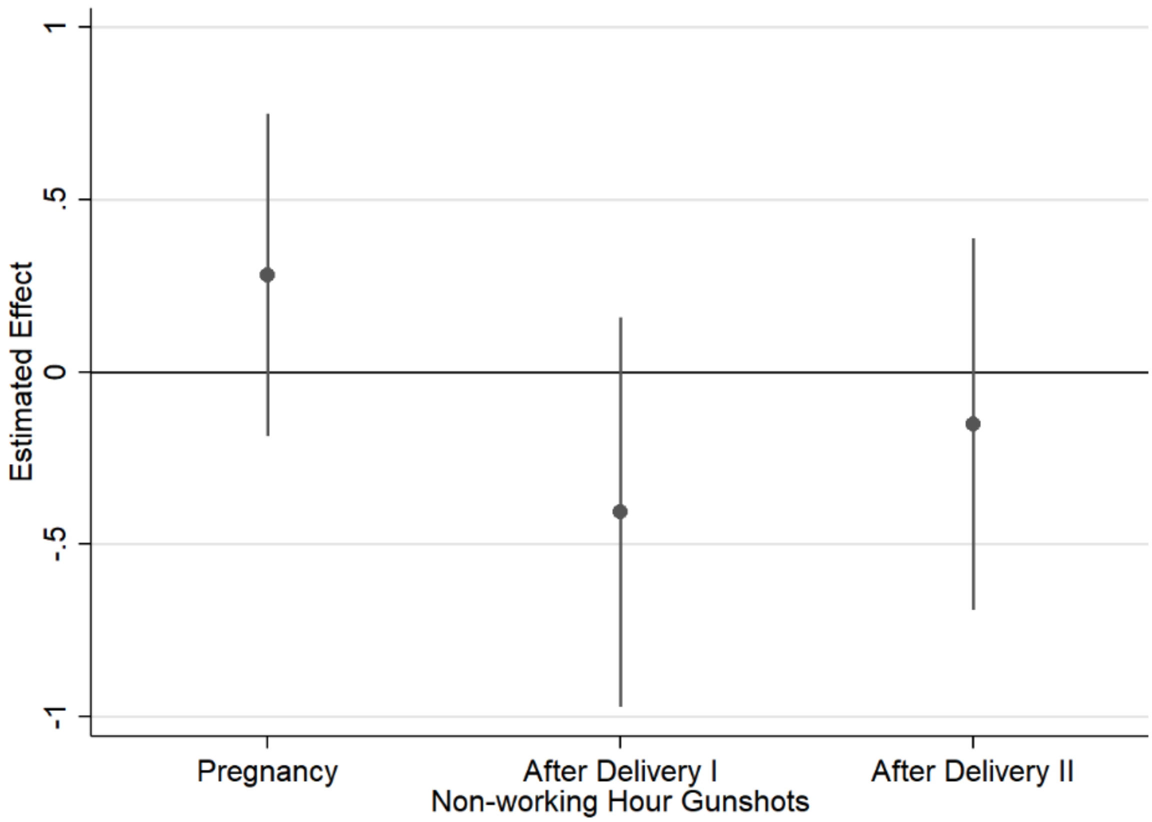

Figure 5: Placebo Test for Very Preterm

Note: Circles represent the point estimates, and the error bars denote the 95% confidence intervals, estimated using equation (2) and restricted to mothers with low formal education (i.e., below a bachelor's degree). "Pregnancy" denotes the effect of exposure to at least one non work-time gunshot during pregnancy; "After Delivery I" denotes the effect of exposure to at least one non work-time gunshot up to 10 months after expected delivery; "After Delivery II" denotes the effect of exposure to at least one non work-time gunshot from 11 through 20 months after expected delivery. Very Preterm is defined as a gestation length less than 32 weeks. Moreover, Non-Working Hours is defined as experiencing a gunshot between the hours 5:00pm-9:00am. All coefficients multiplied by 100 for ease of interpretation. All models include census-block, year-by-month fixed effects, the set of controls as defined in Table 1.

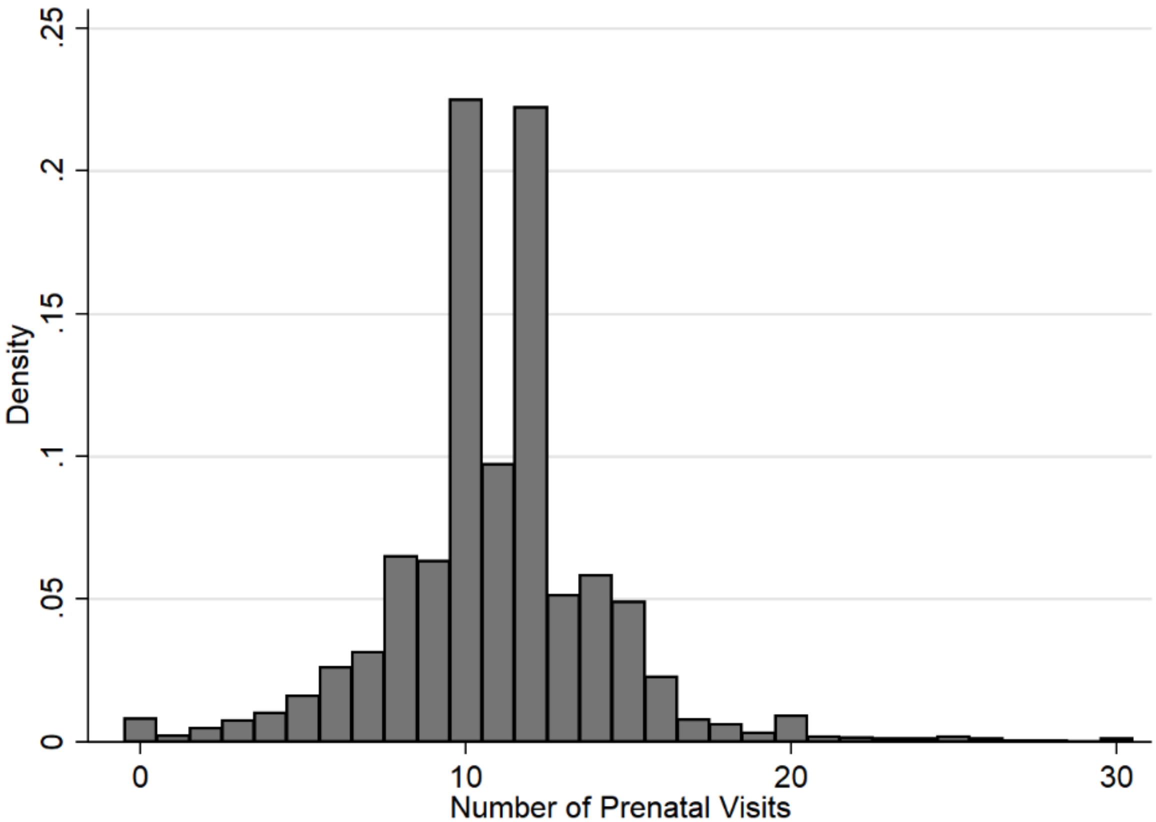

Figure 6: Distribution of Prenatal Visits

Note: Prenatal Visits refer to the total number of prenatal visits reported by the mother. The distribution shows 99% of the observations (top 1% of the distribution is not shown).