



Staggered deployment of gunshot detection technology in Chicago, IL: a matched quasi-experiment of gun violence outcomes

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Abstract

Objectives We examine the potential effects of gunshot detection technology longitudinally in Chicago through a synthetic control quasi-experiment.

Methods Police districts receiving gunshot detection technology were compared to a synthetic control unit via a staggered difference-in-difference design. Across eleven unique gunshot detection technology deployment phases, the analyses produce results for aggregate, initial versus expanded, and phase-specific deployment effects across five gun violence outcome measures.

Results Gunshot detection technology had no effect on fatal shootings, non-fatal shootings, general part I gun crimes, or shots fired calls for service. Gun recoveries significantly increased in the aggregate, initial, and expanded models, and in several individual phases relative to controls.

Conclusions The results align with prior literature that has found a procedural benefit, but not a crime prevention benefit, of gunshot detection technology. Law enforcement agencies seeking crime prevention or reduction solutions may be better served by investing in other options.

Keywords Gunshot detection technology · Gun violence · Crime prevention · Matched quasi-experiment · Synthetic control

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Introduction

An honest assessment of the literature indicates that police have come to privilege technological approaches to crime prevention to the point where modern police operations are largely centered within technology and data systems (Ariel, 2019; Gaub & Koen, 2021). Gunshot detection technology (GDT) is a recent addition to the portfolio of technology-centered gun violence prevention programs. GDT functions through a network of acoustic sensors that detect sounds from firearm muzzle blasts that can be audibly distinguished from other loud noises (Chacon-Rodriguez et al., 2011; Maher, 2007). Often managed by third-party companies like SoundThinking (formerly ShotSpotter), GDT provides a way for law enforcement to respond to shots fired events without relying on citizen calls for service, which may be plagued by inconsistencies such as inaccurate information or failure to report (Carr & Doleac, 2016; Huebner et al., 2022). In the case of SoundThinking, all GDT alerts are manually reviewed by an offsite team of acoustic experts at the company headquarters. GDT alerts confirmed as gunfire events are relayed to the dispatch center of the law enforcement agency in question, which may result in a more efficient and effective deployment of police personnel and emergency resources than what results from traditional approaches that exclusively rely on citizen calls for service to the 9–1–1 emergency line.

Despite the increased popularity of GDT, empirical research has yet to answer several key questions related to its effects on gun violence outcomes. Further, in the GDT evaluation research produced, the generalizability of results has often been limited by design quality and data limitations and/or contradictory conclusions (Doucette et al., 2021; Mares, 2022). Even amidst an underdeveloped and inconclusive evidence base, and increasingly negative community sentiments surrounding GDT, law enforcement agencies have continued to adopt GDT. The need for rigorous, longitudinal evaluations of GDT effects is paramount.

The current study examines potential GDT effects across a target area that increasingly expanded between 2012 and 2018 in Chicago, IL. The study includes the simultaneous examination of several gun violence outcomes of interest that may be impacted by GDT. The outcomes tested include fatal shootings, non-fatal shootings, general part I gun crimes, shots fired calls for service, and gun recoveries. The analyses quantitatively measure changes in associated metrics across pre- and post-GDT time periods using a quasi-experimental design with an empirically derived control group through synthetic control matching techniques. The approach ensures that GDT target areas are effectively matched to approximately equivalent control areas for comparison through the sequencing of empirical covariates in a synthetic control matching model. The unique GDT deployment phases in Chicago were then evaluated for aggregate, initial and expanded, and phase-specific treatment effects using difference-in-difference analyses. The methods used strengthen the validity of the conclusions derived, adding necessary clarity to the evidence base on the potential effects of GDT on gun violence.

The findings indicate that GDT had no effect on fatal shootings, non-fatal shootings, part I gun crimes, or shots fired calls for service in Chicago. There

was some variation in the phase-specific effects, but no significant changes were recorded in the aggregate model (phases 1–11), the initial model (phase 1 only), or the expanded model (phases 2–11) when comparing pre- and post-GDT deployment periods relative to controls. However, significant increases in gun recoveries were observed in the target areas relative to the controls. Significant gun recovery increases were observed in the aggregate, initial, and expanded models, as well as across several individual phases. The implications of the results for future research and practice on GDT and other relevant law enforcement technology are discussed in detail.

Literature review

Police technology

The ongoing development of technologies has been identified as a promising solution for law enforcement (Ariel, 2019). However, research has generated mixed findings on the level to which technology achieves intended public safety goals. For example, CCTV has demonstrated a limited impact on measurable outcomes, with effects varying greatly across geographic settings and operational contexts (Piza et al., 2019). Meta-analyses on body-worn cameras have also determined that officer and citizen behavior is largely unchanged (Lum et al., 2019, 2020) with no discernible effect on prosecutorial or court-related outcomes (Petersen & Lu, 2023). Research on Crime Gun Intelligence Centers (CGICs) and Real-Time Crime Centers have found that CGICs are associated with increases in case clearance (Arietti, 2024; Flippin et al., 2022; Guerette & Przeszlowski, 2023; Koper et al., 2019), but that effects on crime reduction are mixed (Hollywood et al., 2019; Mei et al., 2019; Uchida et al., 2019).

With the recent proliferation of GDT in law enforcement, the continued development of a scientific research literature situating the effects of technology on crime is necessary. Research on GDT has traditionally focused on the technology's performance in detecting gunfire events, identifying the locations of gunfire, and measuring officer responses to GDT alerts (Carr & Doleac, 2016; Choi et al., 2014; Irvin-Erickson et al., 2017; Mazerolle et al., 1998; Piza et al., 2023a; Watkins et al., 2002). In this sphere of the research, results suggest GDT poses observable benefits related to a number of procedural aspects of policing, such as the accelerated detection and response to gunfire and increased collection of ballistic evidence (Cook & Soliman, 2024; Mares & Blackburn, 2012, 2021; Piza et al., 2023a, 2023b). Though, research on GDT's impact on gun violence outcomes has remained more inconclusive. While some studies have found reduced levels of shots fired calls for service following the installation of GDT, most research has found that GDT does not impact part I gun violence categories involving confirmed crime victims (Lawrence et al., 2019; Mares & Blackburn,

2012, 2021; Piza et al., 2023b; Vovak et al., 2021). However, the research designs typically used have not provided empirical clarity.

Prior GDT research

Several GDT studies have been plagued by issues with spatial delineation. Litch and Orrison (2011) explored the impact of GDT on crime in Hampton, VA, and Newport News, VA. Their study compared outcomes in the GDT coverage areas to control areas with similar population density and crime rates, for a 5-month pre- and post-intervention period in each city. The authors found no evidence of a crime prevention effect for violent, weapons, and other offenses in GDT coverage areas during the post-intervention periods. However, the GDT coverage and control areas were approximated due to crime data only being available at the district level, making it difficult to operationalize more precise boundaries. Doucette et al. (2021) conducted a similar evaluation of GDT effects across 68 large metropolitan counties from 1999 to 2016. No significant differences were observed in firearm homicides across counties with and without GDT. However, the authors were also unable to operationalize precise areas covered by GDT as this information was not publicly available, which may have biased the results in the direction of null findings.

Other GDT studies may be less generalizable due to a lack of an empirically derived counterfactual. Mares and Blackburn (2012) examined the impact of GDT on reported gun crimes and shots fired calls for service in St. Louis, MO. St. Louis' GDT system was installed in August 2008 and covered an area of approximately 1 square mile. The study compared two neighborhoods where the technology was installed to four comparison neighborhoods with similar crime, demographic, and economic patterns. Results indicated that GDT was associated with a decrease in shots fired calls for service in the experimental neighborhoods. However, the authors found no discernable impact on reported gun crimes. Mares and Blackburn (2021) then conducted a follow-up study to evaluate the expansion of the St. Louis GDT system across three time periods. Each study period focused on pairing expanded GDT areas with similar untreated administrative areas absent GDT based on demographic similarities. Mares and Blackburn (2021) found around 30% reductions in shots fired calls for service, with no reduction being observed for violent crime. Each of these evaluations did not employ an empirical matching technique in the comparison of targeted areas relative to controls.

Several studies have utilized more empirical matching strategies and have considered GDT expansion. Lawrence et al. (2019) evaluated the impact of GDT on violent crimes and calls for service in Denver, CO, Milwaukee, WI, and Richmond, VA. They assessed differences in outcomes pre- and post-implementation in GDT coverage areas compared to statistically matched control areas. In Milwaukee, GDT effects were also considered across three additional expansion areas, while in Richmond the initial GDT deployment and two additional expansion areas were considered. GDT was associated with increased calls for service for shooting-related crimes across the three cities, and an increase in firearm-related crime in Denver. In

Richmond, there was some evidence of reductions in firearm-related crime, although no significant change was observed in Milwaukee. The studies indicated bi-directional GDT effects for shots fired calls for service, and inconclusive evidence surrounding potential effects on the gun violence outcomes tested.

Other GDT research has also yielded inconsistent conclusions surrounding crime and gun violence outcomes. Vovak et al. (2021) assessed the impact of an integrated GDT/CCTV system in Wilmington, DE on homicide and non-fatal shootings. The study found that crime was lower in the post-intervention period, but homicides and non-fatal shootings increased in the post-intervention period. Though, the models only considered city-level effects through a comparison of Wilmington to similar cities (close in proximity, population, socio-demographics, and crime rates). However, even studies incorporating more micro-level designs with rigorous control groups have found limited evidence of GDT's gun violence prevention effects. For example, Piza et al. (2023b) analyzed the effects of GDT in Kansas City, MO by comparing cumulative crime changes across street segments in the GDT target area to a weighted control group of street segments without GDT. The study found that shots fired calls for service were 22% lower in the GDT target area than the weighted control area, but no changes were observed for shootings, gun assaults, or robberies committed with a firearm (i.e., incidents with confirmed victims).

Two recent studies present exceptions to the results that have generally been observed in the GDT literature. While most evaluations have suggested GDT has a limited effect on gun violence outcomes, the Center for Crime Science and Violence Prevention (CCSVP) found that GDT reduced violent firearm crimes by about 24% in Winston-Salem, NC. Mares (2023) used a longitudinal difference-in-differences model to estimate the effect of GDT on citizen-reported gunshots and aggravated assault with a firearm in Cincinnati, OH. Results indicated that GDT was associated with a 45% reduction in reported shots fired and a 46% reduction in aggravated assaults with a firearm in targeted census tracts compared to controls.

GDT research overview

The present state of the evidence base surrounding GDT is inconclusive due to design quality and data limitations and inconsistent effects on key outcomes of interest. As public safety agencies continue to invest in technology as a possible way to enhance public safety (Ariel, 2019) and support reform (Task Force and on 21st Century Policing Solutions, 2023), it is critical that GDT research ascertains accurate program effects. The growing demand for GDT research is compounded by the proliferation of evidence for other technologies like BWCs (Lum et al., 2020) and CCTV (Piza et al., 2019), both of which have been subjected to meta-analyses examining program effects with a total of 30 and 76 original evaluation studies included, respectively. The GDT literature is not nearly as developed, with only nine evaluation studies testing the technology's crime prevention capacity identified during our literature search, despite the first GDT study being published over 25 years ago (Mazerolle et al., 1998). At present, the capacities of GDT as a crime prevention mechanism may be overstated (Niner, 2022), and negative community perceptions

of GDT continue to increase (City of Chicago Office of the Inspector General, 2021; Stanley, 2021). GDT has high deployment and upkeep costs, and there is some mounting resentment in law enforcement circles surrounding the continued expectation to readily integrate new technology into existing police culture (Willis, 2022).

The scientific knowledge on GDT is further hindered by limitations in common methodological approaches used in prior GDT evaluation studies. Matched quasi-experiments that use quantitative methods to identify treatment and control areas are nearly absent from the GDT literature, with the lone exception of a recent project conducted in Kansas City, MO (Piza et al., 2023b, 2023c). Study periods of GDT evaluations are also often relatively short, with research analyzing program effects in as short as 5 months after GDT implementation. Other concerns center on the common expansion of GDT in cities. Outside of Mares and Blackburn's (2021) evaluation of GDT in St. Louis and Lawrence et al.'s (2019) study of Milwaukee, WI, and Richmond, VA, research has focused on static target areas, providing little insight on what police should expect as GDT coverage areas expand. Target areas in previous studies have also focused on small-scale deployment ranging from 1 to 13 square miles. Research has yet to evaluate GDT systems that expand across larger target areas. In the current study setting of Chicago, GDT coverage expanded from just over 3 square miles to nearly 137 square miles across a 7-year period. It is with these issues in mind that we approached the current study.

The current study

Study scope

The present study aims to evaluate the effectiveness of GDT on multiple gun violence outcomes in Chicago, IL. The study advances upon several prior attempts to assess the effects of GDT by (1) including a multitude of gun violence outcome measures, (2) using a rigorous quasi-experimental research design with empirical controls, (3) testing for aggregate GDT effects across Chicago deployments longitudinally with appropriate pre and post-period lengths, (4) considering both initial and expanded GDT deployment effects over time, (5) and examining potential heterogeneity in phase-specific GDT effects. These inquiries are particularly salient in the present climate as the debate surrounding law enforcement use of GDT endures.

Four research questions are examined through unique statistical models, as specified below.

1. Aggregate model: Does GDT significantly impact gun violence outcomes in GDT-targeted Chicago police districts compared to controls across all aggregate deployments (phases 1–11)?

2. Initial model: Does GDT significantly impact gun violence outcomes in GDT-targeted Chicago police districts compared to controls in the initial deployment (phase 1)?
3. Expanded model: Does GDT significantly impact gun violence outcomes in GDT-targeted Chicago police districts compared to controls across subsequent deployments (phases 2–11)?
4. Phase-specific models: Does GDT significantly impact gun violence outcomes in GDT-targeted Chicago police districts compared to controls in individual phases (phases 1–11 separately)?

Methodology

Study setting

Chicago is the third largest city in the USA and has a population of over 2.75 million people according to the 2022 census. US Census Bureau figures indicate 33% of Chicago residents are Black and 29% Latino. Approximately 21% of all persons subsist below the poverty level. The Chicago Police Department (CPD) employed 13,160 sworn officers and 855 civilians in 2019 (the final year of the current study period) according to FBI figures.¹

Chicago provides a unique backdrop for a study on the efficacy of police technology in light of its long history of contentious police and community relations and marked increase in gun violence patterns (Grimm & Schuba, 2022). Previous strategies used in Chicago like the *Strategic Subjects List*, which used an in-house generated algorithm to identify and monitor persons of interest, have generated extensive controversy (City of Chicago Office of the Inspector General, 2020). In 2020, the CPD discontinued the *Strategic Subjects List* largely in response to both community and city government groups. The current state of relations between the police and the community has also been weakened following lethal police encounters such as the fatal shooting of Laquan McDonald by CPD officers in 2014 and the reverberations of the death of George Floyd across the US in 2020. Perhaps unsurprisingly in the context of Chicago, the original deployment of GDT and the subsequent contract extension signed in 2023 were both met with vocal opposition (see, e.g., www.cancelshotspotter.com and www.stopshotspotter.com).

Chicago Mayor Brandon Johnson partially campaigned on a promise to terminate the City's ShotSpotter contract, citing concerns the system was unreliable, overly susceptible to human error, and played a pivotal role in the police killing of 13-year-old Adam Toledo.² Johnson delivered on his promise on February 13, 2024, by announcing his decision to discontinue the use of ShotSpotter, with a

¹ <https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/tables/table-78/table-78-state-cuts/illinois.xls>

² See pages 2 and 8 in the Johnson campaign's Plan for a Safer Chicago: https://uploads-ssl.webflow.com/63508047b998ed2c03e7e37d/63e1dee8fd3b411c43aa72dc_Plan%20for%20a%20Safer%20Chicago-2.pdf

phasing out of the technology set to begin September 2024.³ In the sake of transparency, the corresponding author presented the findings of the current study to members of the Johnson administration before the decision to cancel ShotSpotter was announced. However, given Mayor Johnson's prior campaign promises and the timing of the presentation, which occurred only a couple of weeks prior to the cancellation announcement, we are unable to say how much this research influenced the decision.

Chicago also provides an interesting case study for examining GDT effects as many other GDT deployments were more concentrated in nature. In our literature search, the largest GDT coverage area reported was 12.68 square miles in Milwaukee, WI (Lawrence et al., 2019). Most prior research has reported GDT target areas of approximately 5 square miles or less. CPD first deployed GDT in a pilot phase in September 2012, covering an approximately 3-square mile area. In 2017, CPD began extending GDT coverage across the city through ten additional deployment phases (11 total).⁴ Figure 1 depicts GDT coverage in Chicago police districts across each of the eleven total unique phases, as well as the timing of each phase deployment.⁵ The cost of Chicago's current GDT coverage area (~136 square miles) is between \$8.8 and \$12.3 M annually based upon on the advertised annual subscription cost of between \$65 and \$90 K per square mile for the ShotSpotter system.⁶

The initial GDT target area accounted for about 1.4% of Chicago's total geography and housed approximately 5% of the city's shots fired calls for service (13,811 of 271,985), fatal shootings (104 of 2,117), non-fatal shootings (876 of 18,470), and part I gun crimes (1,576 of 36,460) between 9/1/2012 and 2/5/2017. The proportion of residents who are non-white (90.10% vs. 51.13%) and households under the poverty rate (33.88% vs. 21.59%) were higher in the initial GDT target area than in Chicago as a whole. The fully installed GDT system covers approximately 60% of Chicago. From the beginning of the GDT system expansion (2/6/2017) to the end of our study period (12/31/2019), the full GDT coverage area housed approximately 60% (48,829 of 69,252) of shots fired calls for service, over 80% (1,268 of 1,578) of fatal shootings, and nearly 80% of non-fatal shootings (11,034 of 14,069) and part I gun crimes (18,276 of 23,675) in Chicago. The proportion of residents who are non-white (67.94% vs. 49.84%) and households under the poverty rate (23.81% vs. 18.31%) were higher in the full GDT target area than Chicago as a whole, although the differences were not as pronounced as those observed for the initial target area (see Table 1).

³ See: https://www.chicago.gov/city/en/depts/mayor/press_room/press_releases/2024/january/city-of-chicago-statement-on-shotspotter-contract.html

⁴ Phase 1, 9/1/2012; phase 2, 2/6/2017; phase 3, 4/24/2017; phase 4, 7/14/2017; phase 5, 9/26/2017; phase 6, 10/30/2017; phase 7, 12/31/2017; phase 8, 1/31/2018; phase 9, 3/7/2018; phase 10, 4/11/2018; phase 11, 5/16/2018.

⁵ The CPD only started tracking GDT detections after several expansion phases. 40,445 gunfire events were detected via ShotSpotter from 11/3/2017 to 12/31/2019.

⁶ See Sect. 8 in the ShotSpotter Frequently Asked Questions document: https://www.shotspotter.com/system/content/uploads/SST_FAQ_January_2018.pdf

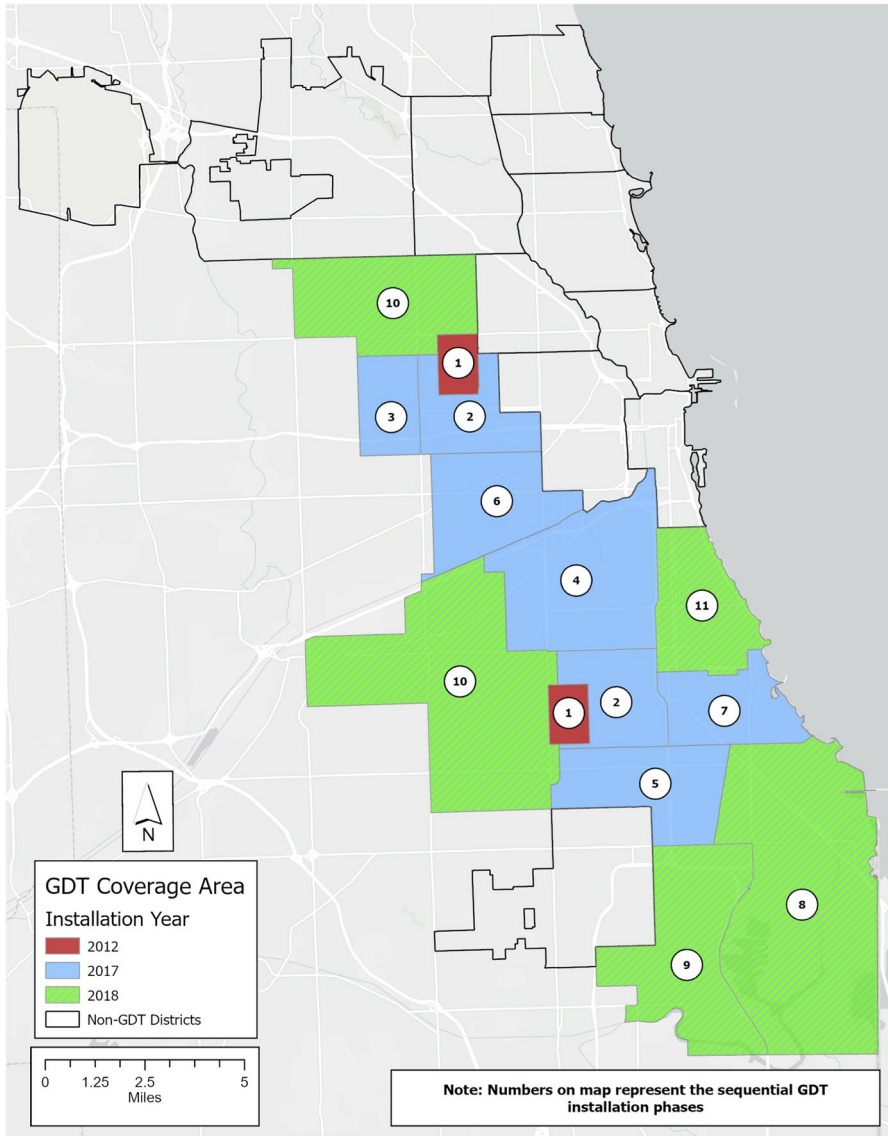


Fig. 1 Chicago police districts and GDT deployment

Data sources and unit of analysis

Data for the study were sourced by the CPD. The data include crimes occurring in Chicago from 2008 to 2019, with individual incidents recorded at the address-level. Crime-related outcomes of interest for the study were identified as fatal shootings, non-fatal shootings, and part I gun crimes, which were filtered by querying shooting

Table 1 Study area characteristics

Measures	Initial installation (9/1/2012–2/5/2017)		Full installation (2/6/2017–12/31/2019)	
	GDT target area	Chicago	GDT target area	Chicago
Area	3.05 mi ²	227.63 mi ²	136.70 mi ²	227.63 mi ²
Shots fired calls for service	13,811	271,985	48,829	69,252
Fatal shootings	104	2,117	1,268	1,578
Non-fatal shootings	876	18,470	11,034	14,069
Part I gun crime	1,576	36,460	18,276	23,675
Non-white population	90.10%	51.13%	67.94%	49.84%
Poverty rate	33.88%	21.59%	23.81%	18.31%

Initial GDT installation occurred on 9/1/2012. Full installation began on 2/6/2017, ending on 5/16/2017. For the initial installation, crime and shots fired data cover the period 9/1/12–2/5/2017. For the full installation, crime and shots fired data cover the period 2/6/2017–12/31/2019. The gun assaults and robberies category also includes battery incidents to reflect Illinois law. All incidents involving shooting victims are excluded from the gun assault and robbery category so that the crime categories are mutually exclusive. GDT target area demographics are measured from the 31 (initial installation) to 474 (full installation) intersecting census tracts. American Community Survey 2016 and 2019 5-year estimates are reported

and weapon columns provided in the official CPD data.⁷ Other data includedg shots fired call for service events, GDT alerts, and gun recoveries, which were also measured at the incident-level.⁸ Other forms of district and city-level data were also collected to inform the study. Census data from the American Community Survey (ACS) were used to derive disadvantage and demographic measures at the block-group level, and LandScan data from the Oak Ridge National Laboratory were used to account for the ambient population (estimates for the on-street population actively mobile in a 1 × 1 km² area). Other relevant data like CCTV camera locations and law enforcement activity indicators like field contacts and arrest levels were also obtained from the CPD in the initial data delivery.

All point-level data were geocoded and aggregated to official police district boundaries to reflect GDT deployment geographies for the analysis. There are 23 unique police districts in Chicago, and the GDT deployments were administered to cover a police district in its entirety.⁹ The district level was also used as the unit

⁷ The part I gun crimes variable consisted of reported assaults, batteries, and robberies involving guns that were not jointly classifiable as a fatal or non-fatal shooting to ensure mutually exclusive classification.

⁸ A custom Chicago address locator was built by the research team to spatially plot incident-level data with address information when XY coordinates were not provided. The geocoding was executed and manually reviewed by the research team, with hit rates above 99% for all data types included.

⁹ The phase 1 GDT deployment was the only GDT target area that did not directly coincide with the boundaries of an existing police district. Parts of police district 7 and police district 11 were in the initial GDT area. These areas were clipped to form new “districts” separate from the remaining non-targeted areas of districts 7 and 11. Several of the later phases included deployment in multiple districts simultaneously, but the entire district geography was in the coverage area. In total, 14 police districts (12 original districts plus two clipped district boundaries for phase 1) of the 25 total operationalized police districts (23 original districts plus two clipped district boundaries) received GDT.

of analysis to facilitate the synthetic control matching approach, which enabled the comparison of changes in target areas relative to empirical controls.

The study period spanned from January 2008 to December 2019. Each district that received GDT was assigned as GDT activated in the first full month after the ShotSpotter system was installed (1-month lag). The lag coincides with the monthly intervals used as study time periods and ensures that the technology was up and running for a full uniform time block before being operationalized as active. After accounting for GDT assignment, the gun violence outcome variables of interest were aggregated to calculate police district-level counts for fatal shootings, non-fatal shootings, part I gun crimes, shots fired calls for service, and gun recoveries. Events were categorized as having occurred either pre- or post-GDT by month and district. Districts and months were used to increase event counts, to improve pre-period matching performance, and to align with common spatial (districts) and temporal (months) law enforcement delineations.

Synthetic control matching

The present study employed synthetic control matching methods (Abadie & Gardeazabal, 2003; Abadie et al., 2010, 2011) to improve upon prior GDT evaluations that have predominately used less empirical techniques to generate controls. Synthetic control methods *synthetically* create a control unit from the pool of available, non-treated control units (non-targeted police districts) via weighting. Instead of generating a one-to-one match with a real existing unit, or coarsening and binning donor pools, the synthetic unit generated is an equivalent match (or nearly equivalent) to the target area based on pre-period outcome variable trends and selected covariates through weighted sampling (Saunders et al., 2015). The method is particularly adept at generating an empirical match for target areas without a true counterfactual (Abadie et al., 2015) or in treatment areas that may not have comparable, untreated control boundaries within the same jurisdiction (Piza et al., 2020). Synthetic control methods are robust to covariates and can be rigorously balance tested to ensure the matching is appropriately sequenced (Ferman, 2021).

Staggered treatment adoption

Recent developments in synthetic control methods and difference-in-difference (DID) have recognized the need to develop techniques that can account for staggered treatment adoption in which treatment timing and/or locations are deployed over time (Porreca, 2022). Traditional DID methods using two-way fixed effects do not account for treatment variations or temporally delineated effects among units with longer treatment exposures (Goodman-Bacon, 2021). Several approaches have since been developed that attempt to assess program effects for staggered adoption, including synthetic difference-in-difference (Arkhangelsky et al., 2021), partially pooled staggered synthetic control methods (Ben-Michael et al., 2021, 2022), and group time averages for two-way fixed effects for difference-in-difference with multiple time periods (Callaway & Sant'Anna, 2021). Each method conceptualizes a novel way to estimate treatment effects for multiple time periods and treated units.

We determined that the group time averages method is best situated for the present study's application (Callaway & Sant'Anna, 2021).

The staggered synthetic control method proposed by Callaway and Sant'Anna (2021) allows for traditional synthetic control methods to be applied to multiple target units emerging at different time periods in a longitudinal design. The method was recently used in a prior criminal justice application that examined the potential for treatment heterogeneity across CCTV deployment locations in Detroit (Circo et al., 2023). The approach is similarly applicable in the present setting, as Chicago introduced GDT across eleven unique phases from the initial 2012 pilot test through May of 2018. GDT was deployed in entire police districts in Chicago at differing phases and timepoints, and the coverage areas were not uniformly sized or time sequenced. The staggered synthetic control method is flexible to different target area operationalizations and uses a weighting schema that minimizes variation in the average pre-treatment outcome trends between treated and control units. Model results can also indicate both aggregate and phase effects of GDT in Chicago using different specifications. The technique is also robust to the inclusion of matching covariates, includes options for specifying the control donor pool as "never treated" or "not yet treated" units, and allows bootstrapping of standard errors for result corroboration.

The following formula depicts the staggered DID design:

$$ATT(g, t) = E[Y_i(1) - Y_i(0) | G_8 = 1]$$

The average treatment effect on the treated (ATT) is calculated through the group-time combination (g, t), which is reflected as the difference between the target units and all control units for every instance of time. The staggered DID approach also includes a dynamic modeling strategy that is weighted based on the amount of time a target unit is exposed to the treatment. This is especially relevant to the present study as GDT was first administered in Chicago in 2012 before steadily expanding again in 2017 across additional deployments. The extension of the DID framework across other model specifications also allows for heterogeneous effects to be estimated at each delineation of time (every month) instead of exclusively relying on a singular average treatment effect.

The R package *did* was used following the procedures outlined in Callaway and Sant'Anna (2021), and in accordance with the application of the method to the prior study of CCTV camera deployment in Detroit (Circo et al., 2023). Pre-treatment trends and auxiliary covariates were used to match target units to donor pools of control units via propensity scores and inverse probability tilting (Graham et al., 2012).¹⁰ Once parallel trend assumptions were satisfied (Cunningham, 2021), the synthetic control DID

¹⁰ For each outcome measure of interest, the treated units were compared to donor pool models of "never treated" and "not yet treated" control units separately. Prior DID research has yet to operationalize a best practice for selecting from donor pools (Callaway & Sant'Anna, 2021). However, the current study reports findings from the "not yet treated" models due to the increased size of the control pool, the robust use of future treated units as controls, to maximize the potential equivalency of treated and control units to satisfy parallel trends assumptions, to reduce volatility in model standard errors, and in accordance with prior staggered DID research (Circo et al., 2023). Additionally, the "never treated" models often failed to converge due to low control unit counts. This may be an artifact of model fit, though, as the never treated control unit pool was smaller, leading to higher ATT values and percent differences, but unreliably high standard errors.

method allowed for staggered adoption to be accounted for in the model by averaging GDT effects across all recorded deployment phases. The aggregate treatment effect value was calculated by conditioning on exposure length through dynamic modeling (i.e., some target units were treated earlier). After deriving an aggregate effect, phase-specific effects were ascertained using the group modeling technique.

Synthetic control model covariates

The modeling strategy involved examining each outcome of interest in a separate model. The covariates used were consistent across models and were incorporated to generate an empirical match for the target area using weights derived from the donor pool of operationalized control units. Each model creates a near equivalent pre-treatment trend between target and control areas in the pre-period, so that the potential for differences attributable to treatment in the post-period can be better isolated. The covariates used are detailed below. All study outcomes achieved a balanced match in their respective model according to parallel trend assumptions.

1. Pre-period outcome measures: pre-GDT monthly counts of fatal shootings, non-fatal shootings, part I gun crimes, shots fired calls for service, and gun recoveries.
2. Non-firearm crime: monthly count of non-firearm part I crimes recorded in each district.
3. Concentrated disadvantage index (CDI): a summated, standardized, census block group-level measure for percentages of; households receiving public assistance, households below the poverty line, persons unemployed, households with a single female head and child under the age of 18, and residents under 18, as measured in the annual American Community Survey 5-year estimates (for each study year). The index was operationalized as the district average for all block groups within the district.
4. Demographic index: a summated, standardized, census block group-level measure for percentages of; non-White residents, residents aged 15–29, vacant properties, and renter-occupied properties, as measured in the annual American Community Survey 5-year estimates (for each study year). The index was operationalized as the district average for all block groups within the district.
5. Ambient population index: a summated, standardized 1 km by 1 km measure of the active on-street population as measured in the annual Oak Ridge Laboratory Land Scan Data (for each study year). The index was operationalized dichotomously with district units classified as being above or below the total district average.
6. CCTV count: monthly count of all CCTV cameras recorded in each district.
7. Enforcement activity: monthly count of field contacts and arrests recorded by district.

Analysis and outputs

To answer research question one, average treatment effects were ascertained to indicate the aggregate effect of GDT on each outcome. The models produce a *simple* result, which is calculated as the average of effect of all deployments, and a *dynamic* result, which calculates the average treatment effect conditioned on length

of exposure to treatment. In this case, the *dynamic* result represents a more appropriate measure because the timing of GDT target deployment varied substantially, and there is reason to assume that higher activity, or higher-risk, districts may have received GDT first (Circo & McGarrell, 2021).¹¹ To answer research questions two and three, additional models were executed to test the effects of the initial phase one pilot GDT deployment (*simple*—this technique applies with just one treatment time) versus expanded GDT deployments across phases 2–11 (*dynamic*—multiple phases). The results from these models account for the delay between phase one and all other subsequent phases and may speak to the potential of an immediate versus a sustained, delayed, or diminished GDT effect. To answer research question four, phase-specific effects were calculated using the *group* modeling specification. The *group* modeling specification calculates unweighted average effects for each phase after treatment activation. To corroborate the observed results, a multitude of tables, plots, and figures depicting aggregate treatment effects, initial and expanded effects, and phase-specific effects were produced.¹²

Results

Aggregate treatment effects—research question 1

Table 2 includes the results for the gun violence outcomes tested in the synthetic DID aggregate models. The *dynamic* model results indicate that GDT led to significant increases in gun recoveries in the target areas relative to the controls. There were about 172 more gun recoveries in targeted districts compared to controls, translating to 11.17% more gun recoveries in GDT target districts than controls. The results indicate that GDT did not have a significant effect on fatal shootings, non-fatal shootings, part I gun crimes, or shots fired calls for service.

The dynamic plots in Fig. 2 depict the length of exposure on the *x*-axis in monthly intervals (every month/year combination in the study period) and the average effect on the treated on the *y*-axis. The *x*-axis position of -1 coincides with the time period just before treatment (1 month before), and the position of 1 corresponds to the first month after treatment. At 0 on the *x*-axis (where red turns to blue in the plot—each vertical bar represents 1 month) the average treatment effect at first exposure is indicated and the prolonged effect of exposure to treatment is mapped across subsequent time periods on the *x*-axis. The effects for the insignificant outcomes (fatal shootings, non-fatal shootings, part I gun crimes, shots fired calls for service) are highly unstable and vary in direction across intervals over time. Alternatively, the gun recovery effects trend post-GDT held a pattern of significant increase after first

¹¹ Of particular interest to the study was the phase one GDT deployment, which took place in September 2012 (the next deployment was February 2017). Conditioning on timing and exposure length in the dynamic model controlled for the variation of GDT delivery and allowed us to examine all phase deployments aggregately.

¹² All results were calculated using 95% confidence intervals for the not yet treated models.

Table 2 Dynamic aggregate average treatment effects (phases 1–11)

Outcomes	ATT	SE	Count	% difference	Lower CI	Upper CI
Gun recoveries	12.331*	3.502	172.634	11.17%	5.468	19.194
Fatal shootings	0.910	0.821	12.740	0.78%	-0.699	2.518
Non-fatal shootings	2.633	2.533	36.862	3.43%	-2.332	7.597
Part I gun crimes	0.689	3.801	9.646	1.03%	-6.760	8.138
Shots fired CFS	21.017	56.671	294.238	3.17%	-90.056	132.090

*= Statistically significant at $p < 0.05$

The count column reflects the expected difference in the count of total events in targeted GDT districts (14 total target district areas in phases 1–11, including 12 actual police districts and two phase 1 clipped area districts) compared to controls in the post-period. The percent column indicates the percent difference in event levels for all targeted districts relative to control units in the post-period

exposure. However, the dynamic plots do reveal some potentially unique and high individual monthly gun recovery values, which may be evidence of time-specific GDT effects driving the observed significant increase.

Initial GDT effects—research question 2

Due to the substantial time gap between the initial deployment and the subsequent expansion of GDT, the potential for differential effects across time was important to test. Two separate models were examined with just phase one¹³ (*simple* specification) and phases 2–11¹⁴ (*dynamic* specification) to account for possible differences in GDT effects over time. The results of the initial GDT model are included in Table 3. The analyses yielded no significant GDT effects for fatal shootings, non-fatal shootings, part I gun crimes, or shots fired calls for service. Like the aggregate models, gun recoveries did significantly increase in the initial GDT model. The results indicate that there were nearly 21 more total gun recoveries on average in each of the two targeted districts in phase one than expected relative to controls, and that there were about 8.29% more-gun recoveries in targeted districts than controls. This finding suggests that gun recoveries significantly increased in the target areas following the first GDT deployment.

Expanded GDT effects—research question 3

The results of the expanded model for phases 2–11 are included in Table 4. The results test if the expansion of GDT led to a delayed or sustained effect on the gun violence outcomes of interest following the initial model. There was no evidence

¹³ The initial model used a pre-period spanning 1/1/2008–9/30/2012. The first deployment phase (with the lag) started 10/1/2012 and the post-period ran through 12/31/2014.

¹⁴ The expanded model used a pre-period spanning 1/1/2008–2/28/2017. The second deployment phase with the lag started on 3/1/2017 and the post-period ran through 12/31/2019. The phase 1 districts and treated areas were excluded from the model.

Fig. 2 Dynamic average treatment effects plot

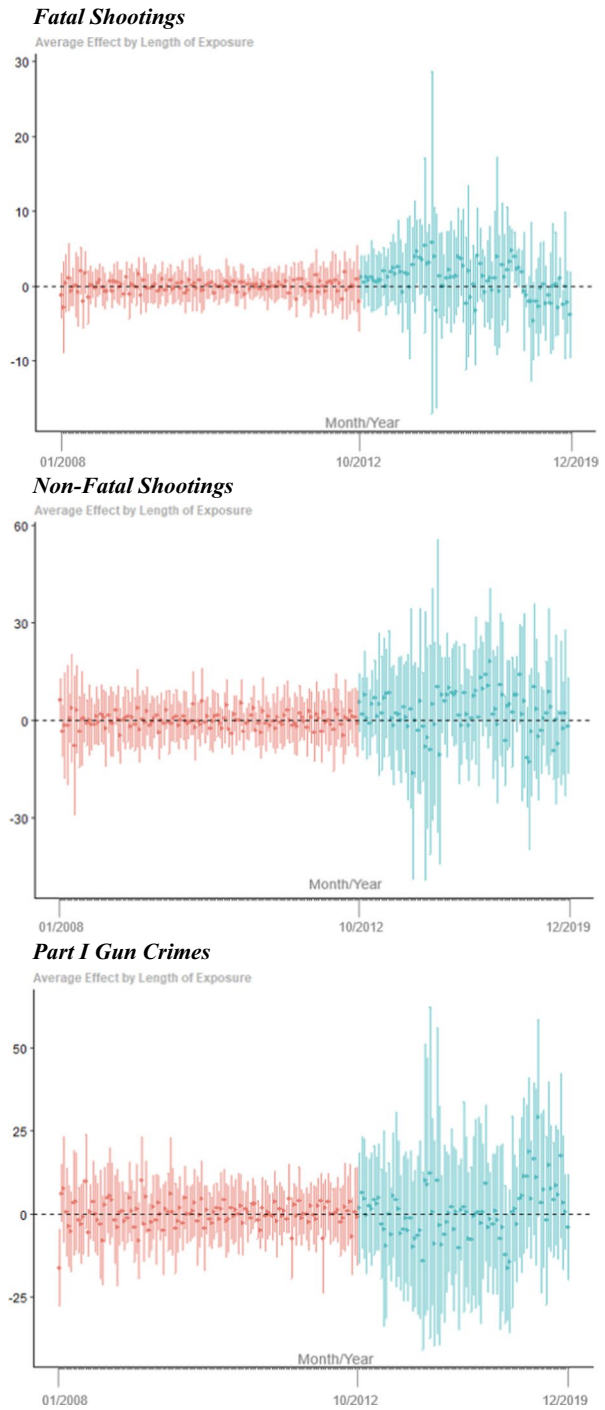


Fig. 2 (continued)

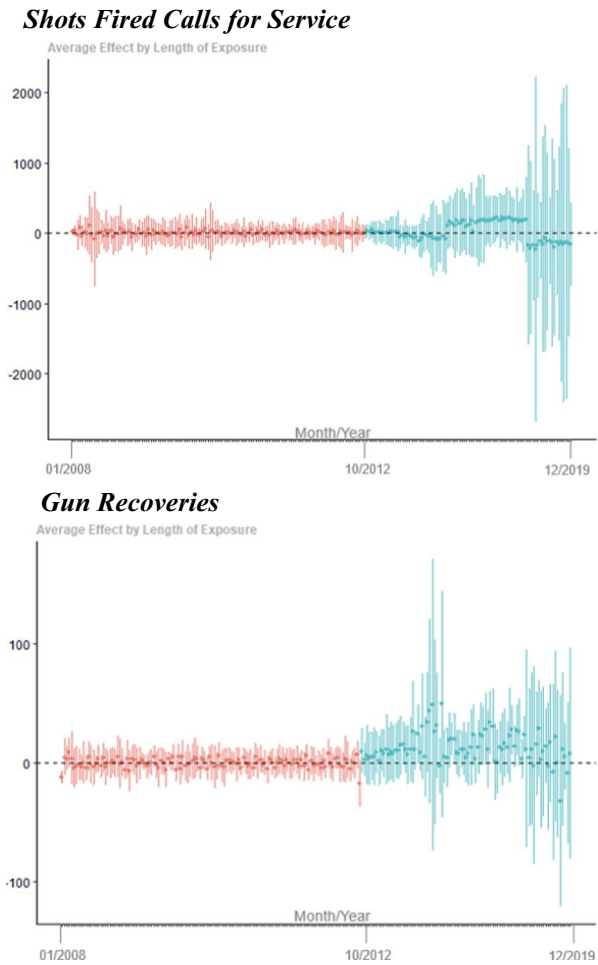


Table 3 Initial simple aggregate average treatment effects (phase 1)

Outcomes	ATT	SE	Count	% difference	Lower CI	Upper CI
Gun recoveries	10.483*	4.333	20.966	8.29%	1.990	18.976
Fatal shootings	1.628	1.811	3.256	6.93%	-1.921	5.176
Non-fatal shootings	5.110	2.825	10.220	2.68%	-0.426	10.647
Part I gun crimes	-5.126	6.009	10.252	-2.35%	-16.903	6.652
Shots fired CFS	106.832	55.425	213.664	3.47%	-1.800	215.464

*= Statistically significant at $p.<0.05$

The count column reflects the expected difference in the count of total events in targeted GDT districts (2 targeted districts in phase 1) compared to controls in the post-period. The percent column indicates the percent difference in event levels for all targeted districts relative to control units in the post-period

Table 4 Expanded dynamic aggregate average treatment effects (phases 2–11)

Outcomes	ATT	SE	Count	% difference	Lower CI	Upper CI
Gun recoveries	16.636*	7.900	199.632	12.28%	1.153	32.119
Fatal shootings	1.821	1.363	21.852	1.51%	-0.850	4.492
Non-fatal shootings	-0.962	5.673	11.544	-1.03%	-12.081	10.157
Part I gun crimes	0.849	6.720	10.188	1.02%	-12.323	14.020
Shots fired CFS	-20.795	34.880	249.540	-2.65%	-89.158	47.570

*= Statistically significant at $p < 0.05$

The count column reflects the expected difference in the count of total events in targeted GDT districts (12 in phases 2–11) compared to controls in the post-period. The percent column indicates the percent difference in event levels for all targeted districts relative to control units in the post-period

of a delayed effect for fatal shootings, non-fatal shootings, part I gun crimes, or shots fired calls for service in the expanded model. As GDT was deployed across more target areas and phases, there was no observable effects on the gun violence outcomes tested. Though, the significant gun recovery increases were sustained in the expansion model, with targeted districts accounting for about 12.28% more-gun recoveries than control units in the post-period. tfooter>

Phase specific effects—research question 4

Lastly, the study sought to determine if there were any specific phases that resulted in significant changes in the outcome measures. The examination of *group*-specific effects is not conditioned dynamically by treatment time and length of exposure to the treatment, instead unweighted phase-specific effects are calculated for each phase individually following treatment activation. The models indicated that there was a substantial amount of phase-specific heterogeneity in both the strength and direction of effects, and there was little consistency across GDT effects in Chicago.

The results of the phase-specific analyses are detailed in Fig. 3.¹⁵ Most phases did not experience significant changes following GDT deployment and phases with observable significant changes were often inconsistent and bi-directional in effect.¹⁶ Fatal shootings exhibited significant increases in three phases. GDT-targeted districts experienced significantly more fatal shootings than control districts during phase 3 (ATT=2.21), phase 5 (ATT=6.31), and phase 7 (ATT=5.55). Significant effects for non-fatal shootings were observed in four phases. GDT-targeted districts experienced significantly fewer non-fatal shootings than control districts during phase 3 (ATT = -14.17) and significantly more non-fatal shootings than controls in phase 7 (ATT=4.75), phase 8 (14.58), and phase 11 (13.41). Significant effects were only observed for part one gun crime in one phase, with significant decreases recorded in phase 4 (ATT = -17.37). For shots fired calls for service, significant

¹⁵ The tabular results of the “not yet treated” phase specific effects models are included in the appendix.

¹⁶ The ATT values for the phase specific models generally reflect the observed difference between target and control units. Since these models examine singular phases, there is no count multiplier or percent difference metrics that extrapolate the effect across other targeted deployment areas (the ATTs would only be multiplied if there were multiple district target areas treated at the same time).

Fig. 3 Phase average treatment effects plot

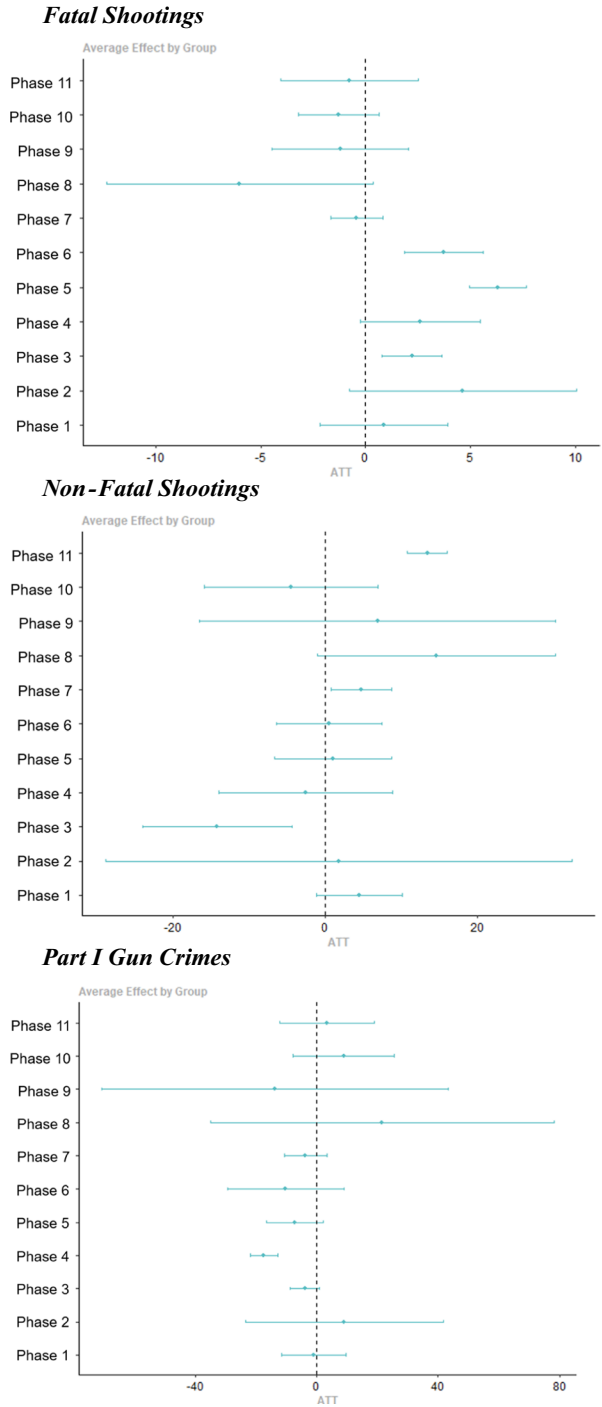
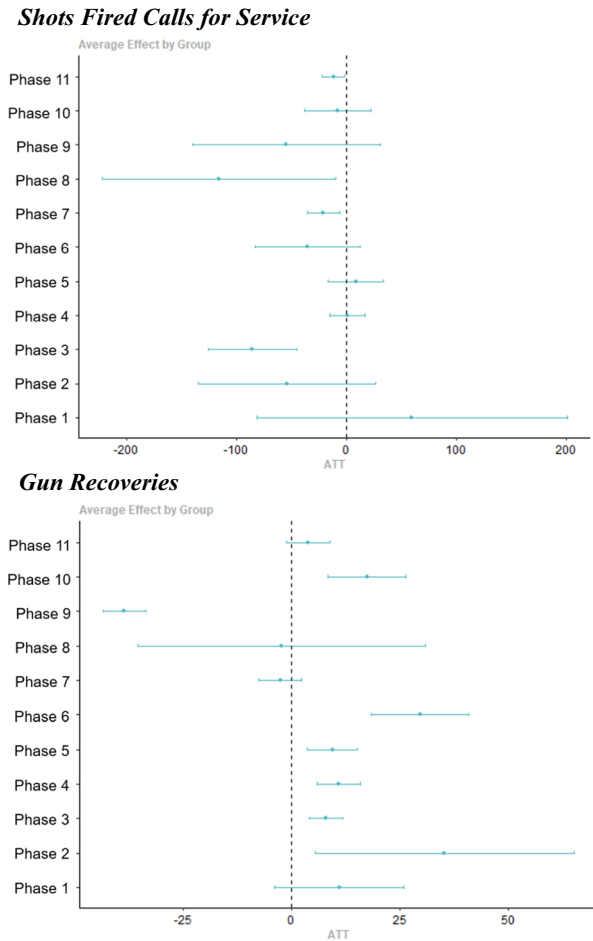


Fig. 3 (continued)



decreases were observed in four phases, including phase 3 (ATT = -85.36), phase 7 (ATT = -20.69), phase 8 (-115.65), and phase 11 (-11.38). Despite significant increases in the aggregate model, gun recoveries also experienced phase heterogeneity. Significant gun recoveries were observed in phase 2 (ATT = 35.36), phase 3 (ATT = 8.08), phase 4 (ATT = 10.96), phase 5 (ATT = 9.52), phase 6 (ATT = 29.79), and phase 10 (ATT = 17.48). Alternatively, a significant decrease in gun recoveries was observed for phase 9 (ATT = -38.51).

Discussion

The results demonstrate that GDT did not significantly impact fatal shootings, non-fatal shootings, part I gun crimes, or shots fired calls for service. This result was observed across aggregate average treatment effects conditioned dynamically over time, initial effects of GDT deployment in phase one, expanded effects of GDT

deployment in phases 2–11, and in most individual phase-specific effects. Following the rigorous testing of multiple deployment locations and time periods in Chicago, the findings are consistent with recent rigorous evaluations of GDT that have found the technology possesses limited effectiveness as a crime prevention tool (Piza et al., 2023b). Future implementation of GDT may need to be predicated on the benefits proposed beyond crime reduction, as GDT may not inherently possess deterrent effects or work as a standalone crime reduction solution.

GDT did lead to increased levels of gun recoveries, with results indicating GDT-targeted police districts experienced about 11.17% (aggregate), 8.29% (initial), and 12.28% (expanded) more gun recoveries than control units. However, this may be a function of crime trends, enforcement practices above and beyond the scope of GDT deployment, or individual phase-specific effects. For example, the phase-specific models indicate that the general pattern reflected significant gun recovery increases across phases, with phases two and six reporting especially high ATT values that may have driven some of the observed increases. However, a few phases also produced insignificant or bi-directional effects. A cursory review of the non-significant phases indicates that many were later-stage deployments (phases 7, 8, 9, and 11) and may have reflected target areas with comparatively lower levels of gun violence that are less likely to be subjected to additional enforcement strategies. Similarly, it may also suggest that GDT produces diminishing returns across subsequent expansions into lower priority areas. Results from the current study are contextualized for both practitioners and researchers in the following paragraphs.

Law enforcement personnel have continued to vocally contend for the benefits of GDT and related technologies in law enforcement practice (Lawrence et al., 2019). The procedural benefits of GDT including accuracy in gunfire detection, spatial precision for gunfire event locations, improved response times to gunfire events, and increased evidence collection are noteworthy. However, these prescribed impacts of GDT have not consistently materialized into additional crime reduction-related benefits. Agencies seeking crime reduction effects may experience better returns from other law enforcement solutions, or by using GDT in a complimentary capacity with other solutions in highly localized, micro-level scales.

It should also be noted that Chicago is a unique study location to consider the impacts of GDT on crime levels for several reasons. First, Chicago has long struggled with some of the highest violence levels (particularly for gun violence) in the nation. The level of violence, coupled with adverse relations between the police, politicians, and the community may create barriers to crime prevention regardless of the technologies or initiatives available. Second, the staggered deployment of GDT in Chicago is rare when compared to the documented experiences of other jurisdictions. GDT is more often used to target smaller, more precise locations that are discernible hot spots for gunfire. Cities like Winston-Salem, NC, and Cincinnati, OH, that experienced some measurable degree of crime reduction from GDT deployment observed the effect at much smaller locales than the coverage area size tested in Chicago. The eleven distinguishable GDT phases in Chicago ultimately resulted in over 60% of the city being covered by GDT. Third, and perhaps most important, is that GDT may not be designed to be a crime reduction tool. The technology is predicated on responding to gunfire events *after* the event occurs. This is further exacerbated by the discrete

operation of GDT, which often requires GDT sensors and locations to not be disclosed to ensure the expensive technology is not discovered or damaged (see Piza et al., 2023a, 2023b). Although coverage areas may be publicly disclosed—as was the case in Chicago—the specific locations of GDT sensors and the locations under GDT provision are not directly visible to individuals to facilitate a potential deterrence effect. This is dissimilar to other surveillance technology, such as CCTV.

Despite such concerns and the lack of observable GDT effects on several gun violence outcomes, the significant increase in gun recoveries is important and noteworthy. GDT may assist law enforcement personnel in responding quicker to gunfire events and getting more guns off the street. The results indicate that significant gun recovery increases were observed across all model conceptualizations. However, the consistency of this finding may demonstrate an overall strategy to target guns in Chicago. The observed results may be contingent on patrol and personnel capacities, as the ability to recover a gun—even in a GDT target area—may not be intrinsically or causally tied to the identification of a gun-fire event. GDT may represent one aspect of a larger initiative to recover guns.

Increasing gun recoveries is meaningful, but even the seemingly positive and beneficial increases in gun recoveries may lead to contentious community relations if residents perceive GDT as a justification for overly enforcement-centric approaches. Such concerns recently culminated in Chicago, as the controversy surrounding the technology, the high-dollar cost, and the lack of GDT effects on key gun violence outcomes led to a mayoral decision to terminate ShotSpotter. Beyond Chicago, other cities investing in, or considering, GDT should state their intended goals and objectives prior to purchase and deployment. The evidence continues to indicate that GDT may hold some resource benefits related to founded incidents and response times but has less merit for gun violence outcomes or case clearance.

Researchers should continue to prioritize the rigorous evaluation of GDT and related police technologies for a multitude of different outcomes. Rigorous evaluation, including experimental and quasi-experimental designs with empirical control groups, is necessary to ensure the technology is meeting key deliverables. There are many additional deliverables and potentially unrealized benefits of GDT that remain unknown. For example, GDT may influence evidence collection on-scene (see Piza et al., 2023b), or the use of the acoustic sensors could be extended to other applications. The improved response times and spatial precision of GDT could also be leveraged to improve case clearance rates if police are able to secure the scene with increased efficiency, though recent research on GDT in Kansas City found GDT did not improve gun violence clearance rates (Piza et al., 2023c).

Future research should also undertake cost–benefit analyses, especially with third party contracted technology that has continued renewal and maintenance costs. A recent cost–benefit analysis by The Center for Crime Science and Violence Prevention (CCSVP) found that the cost of GDT (ShotSpotter) was offset by reported crime reductions and may generate a return of \$15–\$25 per \$1 spent on GDT. The crime reduction capacity, or dollar-for-dollar benefit, of higher price technologies like GDT may be better realized in localized target areas. In contrast, the annual \$8–\$12 M cost in Chicago may be much more difficult to offset via crime prevention. As police budgets are being called into question, the support for police funding and the allocation of already scarce resources could potentially go farther if more strategically distributed.

Conclusion

Several manuscript limitations warrant mention. First, the study timeline proved challenging, as the approach required accounting for both aggregate and phase-specific effects over a prolonged time period. The modeling strategy included the ability to examine aggregate and phase-specific effects and initial and expanded effects, although other plausible alternative methods exist. Second, the synthetic control designs used to model each set of phases were chosen due to their robustness for empirical matching. Matching with covariates and balancing testing was prioritized to advance prior literature on GDT. This resulted in the decision to use synthetic control method approaches, though, there are a multitude of difference-in-difference analyses that can test for treatment effects over time. Third, the research team elected to test the effects of GDT deployment using police districts, as GDT was deployed at that spatial extent. More micro-level units of analysis may have led to more analytical granularity (see, e.g., Piza et al., 2023b), but the present approach reflects Chicago's operationalized geographies.

The results from Chicago indicate that GDT had no effect on several key gun violence outcomes across deployment phases. However, GDT may possess other benefits, including previously demonstrated benefits like increased response precision and efficiency, the significant increases in gun recoveries observed in this study, or possibly other presently untested investigatory benefits. The deciding factor on the use of GDT may be jurisdiction specific and contingent on community climate, cost, target area size, and deployment goals.

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Declarations

Competing interest The authors report no competing interests.

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