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GUN VIOLENCE

Focused deterrence, strategic management, and effective gun violence prevention

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Abstract

Research Summary: The evaluation literature suggests that focused deterrence strategies are effective in reducing gun violence. However, focused deterrence is notoriously difficult to implement and sustain. The history of focused deterrence implementation failure raises questions about its viability as a gun violence prevention strategy. Stockton, California, implemented focused deterrence three times during the past 25 years. In its most recent version, Stockton officials explicitly designed the strategy to be a permanent feature of the city's violence prevention portfolio. Although program caseloads diminished over the course of the COVID-19 pandemic and the strategy faced leadership and resource challenges, Stockton's efforts prevented the program from being discontinued and, for those gang members who did receive treatment, delivered a robust gun violence prevention strategy. A quasi-experimental evaluation shows that treated gang members were less likely to be shot and reduced their violent offending relative to similar untreated gang members. The focused deterrence impacts also appear to spill over to gang members who were socially connected to treated gang members.

Although Stockton experienced an increase in homicides over the course of the COVID-19 pandemic, the increase was not as steep as other comparable California cities.

Policy Implications: Focused deterrence strategies can be effective responses to gun violence problems when implemented properly. A priori planning is essential when jurisdictions prepare to adopt focused deterrence. Strategic management actions, such as maintaining a robust network of partnering agencies, developing accountability structures and sustainability plans, and conducting upfront and ongoing problem analysis, are critical elements that must be in place for focused deterrence to be effective and sustainable.

Focused deterrence strategies are designed to change violent gun offender behavior by understanding underlying violence-producing dynamics and conditions that sustain recurring shooting problems, and implementing a blended set of law enforcement, community mobilization, and social service actions (Braga & Kennedy, 2021). The evaluation literature suggests that focused deterrence is effective at reducing gun violence by gangs and other criminally active groups (Braga et al., 2018; National Academies, 2018). However, these strategies are also notoriously difficult to implement and sustain (Engel et al., 2013; Fox et al., 2015). An extensive history exists of failed implementations of focused deterrence, and successful programs discontinued after leadership changes (Kennedy, 2011, 2019). The persistent implementation and sustainability challenges of focused deterrence highlight the need for evidence of effective programs that survive difficult challenges over extended periods. Policymakers, law enforcement officials, nongovernmental organizations, and community members need to know whether focused deterrence programs are inherently limited to producing short-term gun violence reduction gains or can be implemented as strategic gun violence management systems that generate sustained preventive impacts.

The City of Stockton, California, has implemented focused deterrence to control homicide problems three times during the past 25 years (e.g., California Partnership for Safe Communities, 2018; Wakeling, 2003). Two previous applications of focused deterrence were discontinued following leadership changes (1998–2002) and after the city faced a fiscal crisis during the Great Recession (2006–2008). Descriptive research suggests citywide homicide rates were lower during the two prior implementations of focused deterrence relative to other times when the program was not in place (Bitrain et al., 2024). As Stockton experienced increased violence during the early to mid-2010s, city leaders deliberately developed the capacity to implement a focused deterrence strategy that was "built to last." By 2018, the city implemented a revitalized Stockton Ceasefire focused deterrence strategy that included communications with high-risk individuals that emphasized procedural justice concepts and a focus on behavioral change, enhanced street outreach and service provision efforts, a focused enforcement model rooted in ongoing problem analysis and shooting reviews, the adoption of citywide accountability structures and

sustainability plan, and other refinements. Beginning in 2020, Stockton Ceasefire–focused deterrence implementers faced the unprecedented challenge of how to sustain the program during the COVID-19 pandemic, George Floyd social justice protests, and an ensuing increase in gun violence. Stockton also experienced multiple leadership changes in city government after the 2018 program launch and, like many cities, a post-2020 decrease in police officers. These difficult conditions provided a substantive challenge to the focused deterrence strategic management system and served as a robust test of whether focused deterrence could be sustained as an effective gun violence prevention program over an extended period.

This study evaluates the impacts of focused deterrence on Stockton gun violence among gang members and at the city level over the long term. A quasi-experimental design was used to measure the direct and spillover effects of the most recent Stockton Ceasefire program implementation on treated gang members relative to untreated gang members on individual-level shooting victimization and recidivism outcomes during a six-year period (2017–2022). A longitudinal analysis of citywide homicide trends during 26 years compared Stockton homicide counts during three focused deterrence program periods (1998–2002, 2006–2008, and 2018–2022) relative to homicide counts in comparable California cities to determine whether focused deterrence produced relative reductions in gun violence. The article begins by briefly reviewing the available literature on focused deterrence. It then presents a detailed account of the three implementations of Stockton Ceasefire as it evolved over the span of 26 years as a context for the longitudinal program impact analyses. Subsequent sections present the data, methods, and models used to evaluate Stockton Ceasefire. The analyses suggest that Stockton Ceasefire generated direct and indirect violent gun victimization and recidivism reductions for treated gang members relative to untreated gang members during its most recent application. Citywide homicide rates were also significantly lower during the three Ceasefire implementation periods, but the citywide effect was attenuated during 2018–2022 due to diminished intervention capacity. The concluding section discusses the policy implications of these findings.

1 | LITERATURE REVIEW

Focused deterrence strategies, sometimes called "pulling levers" policing programs, are often framed as problem-oriented exercises in which specific recurring crime problems are analyzed, and responses are highly customized to local conditions and operational capacities (Braga & Kennedy, 2021). Three main variations of focused deterrence are group violence intervention, drug market intervention, and individual offender programs. The group violence intervention attempts to halt persistent gun violence generated by gangs and other criminally active groups. Focused deterrence operations have tended to follow this basic framework:

- Selection of a particular crime problem, such as serious gun violence.
- Pulling together an interagency enforcement group, typically including police, probation, parole, state and federal prosecutors, and sometimes federal enforcement agencies.
- Conducting research, usually relying heavily on the field experience of front-line practitioners, to identify key offenders, groups of offenders, such as street gangs and drug crews, and characterizing the context of their violent gun behaviors.
- Framing a special enforcement operation directed at those offenders and their co-offenders and designing all legal tools or levers to sanction groups of offenders in ways that reduce the chances they will continue to commit serious gun violence.

- Matching enforcement operations with parallel social services and moral voices of affected communities to those same offenders and groups.
- Communicating directly and repeatedly with offenders and their co-offending groups to let them know that they are under scrutiny, what criminal acts will receive special attention, when that has in fact happened to offenders and groups, and what they can do to avoid enforcement action (adapted from Kennedy, 2019: pp. 206–207).

Focused deterrence programs attempt to promote legitimacy by ensuring that crime control efforts are focused on the safety and well-being (including preventing contact with the criminal justice system) of group members and others at high risk of violence. Focused deterrence is not an indiscriminate increase in sanction severity. Rather, it is focused on those believed to be responsible for a disproportionate share of gun violence in communities (Braga & Kennedy, 2021). When implemented as designed, focused deterrence responds to triggering events, such as a gun homicide committed by a specific gang, and the rationale supporting subsequent enforcement actions is transparent to community members and offenders.

Effective focused deterrence interventions involve community stakeholders in the design and implementation of the program (Braga & Kennedy, 2021). Contacts between offenders and the authorities are conducted through focused deterrence sessions in a procedurally just way that offers credible threats of sanctions and help and assistance to offenders who are willing to desist from violent offending (Kennedy, 2011). Qualitative evidence suggests these activities improve strained relationships between minority neighborhoods and the police departments that serve them through the principled engagement of community members, communications with offenders in procedurally just notification sessions, and the provision of service and opportunities to offenders (Brunson, 2015; Meares, 2009).

1.1 | Theoretical perspectives supporting focused deterrence

Deterrence is affected by offender perceptions of sanction risk and certainty of detection (Nagin, 2013). The effective communication of the risks of punishments to relevant audiences is an important step in generating deterrence (Zimring & Hawkins, 1973). Focused deterrence strategies attempt to prevent shootings by directing deterrence messages to individuals identified as being at especially high risk of offending with, or victimized by, a firearm. In addition to sanctions, the messaging also includes the potential benefits of available services (e.g., tattoo removal, employment, drug counseling) to improve their life trajectories and makes normative appeals to halt violent gun behavior that traumatizes communities (Kennedy, 2011).

Focused deterrence relies on group notification meetings (i.e., call-ins, forums) and customized individual notifications from criminal justice officials to inform offenders that violent gun behavior will be responded to with certain and swift consequences (Braga & Kennedy, 2021). Direct communications, coupled with swift and certain sanctions for violating established behavioral norms, influence offender perceptions of apprehension risk. Face-to-face meetings with offenders are an important first step in altering their perceptions of sanction risk (Nagin, 2013). Direct communications and affirmative follow-up responses are the types of new information that may cause offenders to reassess the risks of continuing their violent behavior (e.g., shooting at gang rivals) (McGarrell et al., 2006).

Focused deterrence strategies make the prospect of sanctions more legitimate by bringing in individual norms, values, and informal social control (Kennedy, 2011). Legitimacy represents more

than mere public support; rather it represents the willingness of the public to recognize and voluntarily defer to official authority (Beetham, 1991). Research suggests that the way police treat citizens directly and measurably influences citizen perceptions of the police (Reisig et al., 2007; Tyler, 2006). Police are viewed as legitimate authorities when citizens perceive that the police treat citizens with respect and make their decisions to use authority fairly (Tyler, 2006). Punishments are deemed illegitimate when they are excessive, poorly motivated, and not well aligned with individual and community norms (Kennedy et al., 2017). Sanctions should be consistent with community and individual norms rather than threats wielded by external and possibly hostile authorities. Focused deterrence builds upon concepts of police legitimacy and procedural justice when communicating with offenders and engaging community partners. Call-ins with groups of violent offenders and custom notifications with individual offenders are designed to inform them that they were selected for intervention by virtue of their serious violent behavior rather than their status (such as gang member or drug seller); the partnership then expresses concern for the well-being of the community and of the offenders themselves and provides offenders a clear choice by offering help and promising sanctions for continued violence in a respectful and business-like manner (Braga & Kennedy, 2021).

Community-based organizations and resident groups can be potent violence prevention partners for law enforcement agencies (Sharkey, 2018). Focused deterrence programs seek to stimulate informal social controls to reduce both violent offending and punishment rates. These programs customarily include relevant community-based organizations in the larger violence reduction partnership to deliver key intervention actions. Key community partners have included Black clergy groups (such as Boston's Ten Point Coalition and Black Ministerial Alliance), street outreach workers, and local nonprofits focused on providing mental health services and improving education and employment opportunities (Braga, Turchan, & Winship, 2019). Community-based organizations are included in numerous program activities such as upfront conversations on selecting violent gangs for intervention, the development and execution of communication campaigns with groups and individuals, mobilizing community anti-violence voices through neighborhood "peace walks" and community cookouts, and connecting specific high-risk youth with services and opportunities (Braga & Kennedy, 2021).

1.2 | Evaluation evidence

An ongoing Campbell Collaboration systematic review shows the positive impact of focused deterrence programs on serious gun violence. The most recent iteration of the systematic review identified 24 quasi-experimental evaluations of focused deterrence programs (Braga et al., 2018). These studies evaluated focused deterrence programs implemented in small, medium, and large cities. Except for an evaluation of a focused deterrence program implemented in Glasgow, Scotland, all included studies were conducted in the United States. Twelve evaluations tested the impacts of gang and group intervention programs on violence, nine evaluations considered the effects of focused deterrence on crime problems connected to street-level drug markets, and three evaluations appraised crime reductions generated by focused deterrence programs targeting individual repeat offenders. Nineteen of the 24 focused deterrence evaluations (79.2%) included in the review reported at least one significant crime control effect (Braga et al., 2018). A meta-analysis of the program impacts found moderate reductions in targeted crime problems associated with focused deterrence programs, with the largest impacts produced by the gang and group-violence reduction programs.

Most focused deterrence studies included in the Campbell review measured program impacts on crime outcomes in large areas such as neighborhoods or policing districts (12 of 24; 50%) or cities (9 of 24; 37.5%). Area-level studies provide prima facie evidence that focused deterrence programs are associated with crime prevention gains. However, area-level studies do not provide direct empirical evidence that treated gangs or treated individuals changed their violent behaviors in response to focused deterrence interventions. Three studies evaluated program impacts on crime outcomes measured at the gang level s (12.5%), and only one study measured impacts on individual-level crime outcomes (4.2%).[1] Focused deterrence was associated with significant reductions in shootings committed by treated gangs relative to untreated gangs in Boston (Braga, Hureau, & Papachristos, 2014), Chicago (Papachristos & Kirk, 2015), and Los Angeles (Tita et al., 2004).

Scientific evidence on the impacts of focused deterrence on targeted crime problems continues to grow. The rigor of the gang and group-violence reduction focused deterrence evaluations has steadily improved over time with contemporary quasi-experimental designs using more sophisticated methods, such as propensity score matching and synthetic controls, to ensure comparable treatment and control groups (Braga et al., 2019; Circo et al., 2020; Kapustin et al., 2024; Roman et al., 2019). Importantly, four randomized controlled trials testing the impacts of focused deterrence on individual offenders have been completed since the release of the most recent iteration of the Campbell review. Three reported large reductions in targeted crime problems whereas the fourth found mixed effects. These randomized experiments measured program impacts on treated individuals for relatively short post-treatment periods, ranging from 12 to 24 months.

In Sacramento, California, a randomized experiment found evidence that a focused deterrence intervention, where police officers visited the homes of prolific offenders to offer "carrots" (desistance pathways) and "sticks" (increased sanction threats), reduced subsequent recidivism by 21% in the 12 months following random assignment, along with suggestive evidence that it decreased the future offending of those individual's prior co-arrestees (Ariel et al., 2019). In St. Louis, Missouri, a randomized evaluation found parolees and probationers who were invited to attend the focused deterrence notification meeting were less likely to be arrested during the following 17 months relative to those who did not attend the meeting (Hamilton et al., 2018). Similarly, Davis et al. (2024) reported that juveniles in Chicago detention centers randomized to pre-release focused deterrence notification forums were 18% less likely to be re-arrested within one year of release. These reductions were driven by statistically significant reductions in arrests for violent and drug crimes by 43% and 40%, respectively. In New York City, Aboaba et al. (2023) found adults who were called in to attend notification forums were substantially less likely to violate their parole in subsequent 12-month and 24-month post-release observation periods, and this effect was driven primarily by reductions in violations due to absconding. However, the program did not affect individual arrest rates or neighborhood levels of gun violence.

1.3 | Implementation challenges

Focused deterrence strategies must be implemented properly to generate crime reductions. The available program evaluation evidence suggests that it can be difficult for local jurisdictions to achieve successful program implementations that remain robust over time (Kennedy, 2011; Tillyer et al., 2012). Focused deterrence interventions are comprised of multifaceted activities and a complex interagency structure that presents multiple opportunities for implementation and fidelity

problems (e.g., see Circo et al., 2021; Grunwald & Papachristos, 2017; Saunders et al., 2016). The systematic review of focused deterrence programs found that nearly one-third of the 24 included evaluations reported at least one threat to the treatment integrity stemming from implementation challenges (Braga et al., 2018). The deficient implementation of these programs could exacerbate poor police–community relations and generate collateral harm through increased surveillance and harsh enforcement (Griffiths & Christian, 2015).

The seminal Operation Ceasefire strategy in Boston was discontinued due to a lack of ongoing analysis of evolving gun violence problems, the absence of a governance structure to support its continued implementation after key working group members moved to other positions, and political in-fighting between partnering agencies (Braga et al., 2008). Moreover, replication programs in Baltimore, Minneapolis, and San Francisco unraveled rapidly due to political problems and a lack of interagency partnership after encouraging initial crime control success (see Braga, Turchan, & Winship, 2019; Kennedy, 2011). The gun violence reduction effects of the Chicago Project Safe Neighborhoods focused deterrence strategy were diluted as the program expanded to larger areas of the city without increases in funding and resources (Grunwald & Papachristos, 2017). Other focused deterrence programs also experienced very concerning program management challenges during implementation. The robustness of the Rochester Ceasefire intervention was limited by uncertain enforcement actions, poor inter-agency communication and coordination, and deficiencies in marketing the deterrence message to the targeted audience (Delaney, 2006). The Kansas City No Violence Alliance group violence reduction strategy had to overcome early problems stemming from a lack of leadership and poor communication among participating agencies before the intervention took hold (Fox et al., 2015).

Cities need to develop the following capabilities to facilitate the successful implementation of focused deterrence: creating a network of capacity, developing accountability structures and sustainability plans, and conducting upfront and ongoing problem analysis (Braga & Kennedy, 2021; Circo et al., 2021; Engel et al., 2013; Saunders et al., 2016). Convening an interagency working group with a locus of responsibility for managing the targeted crime problem is a key operational component of focused deterrence. The working group needs to be supported by a "network of capacity"—a larger collaboration that spans the boundaries that divide criminal justice agencies from one another, criminal justice agencies from human service agencies, and criminal justice agencies from the community (Moore, 2002). These kinds of collaborations are necessary to legitimize, fund, equip, and operate complex strategies that are most likely to succeed in both controlling and preventing equally complicated problems such as gang-involved gun violence (Braga & Kennedy, 2021). The inclusion of key administrative staff, such as program directors, can help ensure that the interagency capacity needed for successful implementation is built and maintained for extended time periods (Circo et al., 2021).

The reliance of focused deterrence programs on a small number of key actors across multiple organizations to implement the strategy successfully can make these initiatives highly vulnerable to personnel turnover. Unless strong accountability structures and sustainability plans are in place, the loss of key personnel can disrupt working group processes and hinder the preventive actions available through the larger network of capacity (Braga & Kennedy, 2021; Braga, Zimmerman, et al., 2019; Tillyer et al., 2012). The consequences of personnel turnover can be minimized through upfront planning for these inevitable events. The Cincinnati Initiative to Reduce Violence (CIRV) developed a comprehensive approach to remedy sustainability concerns through a formal multilevel governance structure (Engel et al., 2013): The *Governing Board* was at the highest level and consisted of high-ranking city officials who were responsible for overseeing the project, providing resources, and overcoming implementation obstacles; the *Strategy and Implementation*

Team reported to the Governing Board and included spokespersons, heads of individual strategy team, consultants, and an executive director; this body was responsible for daily operations, strategy development, and monitoring results; and four *Individual Strategy Teams* comprising a law enforcement team, social services team, community engagement team, and systems team were responsible for implementing specific components of the initiative.

The role of problem analysis in focused deterrence, and problem-oriented policing more generally, is crucial as it requires the careful examination of underlying factors that lead to persistent gun violence problems (Braga & Cook, 2023; Circo et al., 2021; Goldstein, 1990). Following the completion of the initial analysis, key elements of the focused deterrence framework can be customized to local gun violence conditions and the operational capacities of partnering agencies. Ongoing problem analysis can be supported by regularly scheduled shooting incident reviews where knowledgeable practitioners provide detailed qualitative insights on the nature of shooting events and the criminally active groups and gangs that perpetrate these incidents (Klofas & Hipple, 2006). These data can be incorporated into "shooting scorecards" that create rank-ordered frequencies of the gangs and groups that commit the highest number of shootings and experience the greatest number of shooting victimizations during a specific period (such as a week, month, or year; see Braga, Hureau, & Grossman, 2014). When supported by a management accountability system, scorecards ensure that partnering agencies stay focused on risky groups over time and support the overall implementation of the strategy.

2 | FOCUSED DETERRENCE IN STOCKTON, CALIFORNIA

2.1 | The nature of serious violence in Stockton

Stockton is a midsized city in San Joaquin County, California, that covers an area of 53 square miles and, according to 2022 U.S. Census estimates, has 321,819 residents. Hispanics (44.9%), Asians (20.9%) primarily of Southeast Asian descent, and African Americans (11.3%) represent the three largest race and ethnic groups in Stockton. Stockton's median household income ($54,297) falls well below the national average ($61,937), and the city has a much higher poverty rate (20.5%) than the national rate (13%). Stockton suffers from persistently high levels of violence. Stockton had a violent index crime rate of 1,151.5 per 100,000 residents in 2022, more than double the violent index crime rate for the state of California (494.5 in 2022).[2] Consistent with national trends, Stockton and the state of California experienced sudden homicide rate increases in 2020 coinciding with the initiation of the COVID-19 pandemic and the civil unrest following the George Floyd murder by Minneapolis police officers. Between 2019 and 2021, California homicide rates increased by 39.5% from 4.3 to 6.0 per 100,000 residents while Stockton homicide rates increased in relative terms more modestly, rising by 13.8% from 10.9 to 12.4 per 100,000 residents.

Stockton has partnered with academics and nonprofit organizations to conduct in-depth analyses of the nature of its homicide problem on an ongoing basis during the past several decades (e.g., see Braga, 2005; California Partnership for Safe Communities [CPSC], 2012, 2018). These problem analyses generally find that homicides in Stockton are highly concentrated among a small number of people and that firearms are the weapons of choice. Younger men tend to be the victims and perpetrators of homicides. Homicide victims and perpetrators are usually well known to the criminal justice system and often under some form of court supervision (e.g., on parole or probation). The most recent problem analysis found that about 75% of 2016–2017 homicides involved gang members as victims, suspects, or both (CPSC, 2018). More than two-thirds of these gang-involved

homicides were generated by intergroup disputes, internal group disputes, personal disputes, and drug business disputes. Stockton gangs most at risk for homicide victimization represented less than 1% of the city's population.

2.2 | The evolution of the Stockton ceasefire strategy

The City of Stockton adopted focused deterrence to control gang-involved gun violence three times during the last 25 years with full implementations occurring between 1998 and 2002, 2006 and 2008, and 2018 through the present. The first two iterations (1998–2002, 2006–2008) closely followed the well-known Ceasefire group violence reduction strategy pioneered in Boston during the 1990s (Braga et al., 2001; Kennedy et al., 1996). Outbreaks of gang-involved gun violence were addressed through a classic focused deterrence "carrots and sticks" approach. The Stockton Police Department (SPD) and its criminal justice partners applied pulling levers enforcement to halt shootings immediately and hold offending gangs accountable for their violent behavior. Gang outreach workers (known as Peacekeeper), social service, and community-based organizations aided gang members who wanted to transition away from violent gang activities. Call-ins were used to communicate deterrent messages, offer social services, and discuss changing norms supportive of violence held by offending gangs.

A quasi-experimental evaluation of the 1998–2002 Stockton Ceasefire implementation suggested the strategy was associated with a 42% reduction in gun homicides that was distinct from gun homicide trends in other midsize California cities (Braga, 2008). Unfortunately, the evaluation also noted that SPD leadership changes resulted in the cessation of the Ceasefire strategy by January 2003. The second iteration of Stockton Ceasefire was discontinued after the city experienced an extreme fiscal crisis driven by the sharp decline in global economic activity associated with the Great Recession of 2008. No formal impact evaluation of the second Ceasefire effort was completed. Beyond the resource strains induced by the very difficult economic environment, a Yale University case study noted that both applications of focused deterrence were largely supported by temporary, informal management structures and concluded the approach had never been institutionalized as a formal strategy by the City of Stockton during those periods (Bitrain et al., 2024). The Yale study also documented persistent community concerns over a focused deterrence approach that seemed to privilege harsh punishments and enforcement threats over robust service delivery intended to improve the life prospects of at-risk young people.

Stockton became the largest U.S. city to declare bankruptcy in 2012. The bankruptcy led to unprecedented reductions in city services, including police lay-offs that exacerbated already strained community–police relations, that left residents concerned about their future in a city that felt like it was on the verge of collapse (Bitrain et al., 2024). Police work was, at best, reactive and limited to traditional law enforcement activities. Violence increased dramatically. Stockton set an all-time homicide rate record in 2011 of 19.9 per 100,000 residents and again in 2012 with a rate of 24.3. During the next five years (2013–2017), Stockton slowly emerged from financial ruin and started to rebuild the capacity to implement a revitalized Ceasefire strategy designed to improve the life prospects of high-risk youth and enhance citizen trust and confidence in the police. This required the city to make significant changes in staffing, organization, and management of its violence prevention resources. In 2014, Stockton established an Office of Violence Prevention (OVP) to coordinate gang violence prevention and intervention services (employment and housing assistance, mental health and substance abuse counseling, education service, life coaching, and mentors) and manage the Peacekeepers gang outreach worker pro-

gram. In partnership with the National Initiative for Building Community Trust and Justice and the CPSC, the SPD trained its officers in the basic elements of procedural justice, undertook listening and reconciliation sessions to address concerns held by community members, and used these insights to inform changes to departmental policies and practices (see National Initiative, 2018). The SPD, the City Manager's Office, and community leaders explicitly drew on the principles of procedural justice as they crafted the revitalized Stockton Ceasefire–focused deterrence initiative.

Although limited violence prevention activities commenced in earlier years, the current configuration of the Stockton Ceasefire program was fully implemented in 2018. The SPD created a Ceasefire Division that directs the focused enforcement component of the strategy including street enforcement to suppress outbreaks of gang violence, investigations to identify key offenders and hold them accountable for shootings, and intelligence collection to understand group membership, alliances, and violent disputes. The SPD tracks gang-related gun violence and determines whether specific groups need focused attention by virtue of their repeated involvement in shootings through weekly shooting incident reviews and use of scorecards. SPD maintains close working relationships with the San Joaquin District Attorney and other federal, state, and local criminal justice agencies on Ceasefire enforcement operations. Key OVP Ceasefire components include violence interruption (conflict negotiation and mediation) by Peacekeepers and case management of high-risk clients to ensure appropriate services and opportunities are provided. OVP collaborates with a variety of educational partners, social service providers, and community-based organizations to ensure that a robust menu of treatments is available to its clients. OVP Peacekeepers also respond to shooting scenes and coordinate with the San Joaquin Hospital in the delivery of services to gunshot wound survivors and their families.

SPD, OVP, and other Ceasefire partners stay in constant communication when gun violence erupts. At weekly coordination meetings, SPD and OVP share general information on recent shootings and organize a collaborative response to gang shootings. Selected at-risk gang members are invited to attend a personal "Safety Meeting" with a Peacekeeper, a high-ranking SPD official (usually the Deputy Chief or Chief), and an influential community member where they are informed of their personal safety risk and offered services. Gang members who want help are engaged by Peacekeepers and provided tailored services including life skills coaching and cognitive behavioral therapy. The SPD works with criminal justice partners to launch pulling levers enforcement operations that are customized to the individuals in the selected gang with punishment that is only as harsh as it needs to be to change behavior. Finally, SPD, OVP, and the Ceasefire partners hold quarterly "call-in" sessions for selected gangs. These large-scale meetings are intended to be procedurally just in the engagement of gang members with the intention of respectfully informing (rather than threatening) them of the risks of continuing their violent lifestyles and the support they can receive to improve their life trajectories.

Stockton Ceasefire practices are grounded in the ongoing systematic collection and analysis of data by the SPD and OVP. These data-driven management processes, which include caseload reviews, shooting incident reviews, case coordination meetings, offer the City Manager, SPD Chief, OVP Director, and other decision-makers timely and comprehensive information on violent gun crime patterns and trends in Stockton. Real-time monitoring of the Ceasefire strategy is facilitated through performance management meetings, allowing for timely adjustments to operations when needed. Inspired by performance management models like CompStat in New York City, CitiStat in Baltimore, and CincyStat in Cincinnati (see Behn, 2014), *StocktonStat* functions as a performance management system designed to enhance efficiency, decrease expenses, and

enhance the delivery of municipal services to the community. StocktonStat involves biweekly performance review meetings with the SPD and OVP. During these sessions, key Ceasefire process measures (e.g., enforcement actions taken, custom notifications delivered, services accepted by clients, etc.) and outcome measures (e.g., homicides and shootings) are reviewed. These meetings were designed to ensure that the Ceasefire program is properly focused on high-risk gangs and its activities are being implemented with fidelity to the model.

The Stockton Ceasefire strategic management system seemed to sustain program operations through a series of leadership changes including the turnover of the City Manager (2020), Mayor (2021), SPD Chief (2021), and OVP Director (2021). The 2020 COVID-19 pandemic and George Floyd protests further strained city resources. And, amid increasing gun violence, the number of SPD officers declined by 20.5% from 473 in 2018 to 376 in 2021 (SPD, 2022). Not surprisingly, these challenges diminished the capacity of partnering agencies to treat violent gang members during this period as evidenced by a linear decline in Ceasefire clients from 513 subjects in 2019 to 342 subjects in 2020 to 225 subjects in 2021 ($N = 1{,}080$ treated gang members). The initiation of treatment was determined by the timing of the first face-to-face communication (through participation in a safety meeting or a larger call-in) that was followed by a blend of enforcement and service provision activities. Most of the 1,080 treated gang members received only one Ceasefire notification ($N = 929$, 86.0%), whereas a small number received two or more notifications ($N = 151$, 14.0%). All treated gang members were offered services and opportunities, but only 389 (36.0% of 1,080) engaged OVP for an initial assessment and subsequent referral to service providers. Only 192 participated in programming (49.4% of 389) with these individuals, on average, taking advantage of 8.6 distinct types of services including employment support, education, housing assistance, mental health counseling, access to material goods, and other more specialized services (e.g., tattoo removal and driver license renewal).

3 | PROGRAM EVALUATION DATA AND METHODS

3.1 | Individual-level impact evaluation: Research design and data

A quasi-experimental design was used to compare shooting victimization and recidivism trends for Stockton gang members who experienced the Ceasefire intervention to shooting victimization and recidivism trends for comparison gang members who did not directly receive Ceasefire (Shadish et al., 2002). Through the larger group call-ins and street conversations between Ceasefire partners and socially connected gangs, the Stockton-focused deterrence program was explicitly designed to ensure that knowledge of Ceasefire actions would diffuse to non-Ceasefire rivals and allies, and therefore influence these gang members' own behaviors. Therefore, the inclusion of gang members that were socially connected to individuals targeted by Ceasefire for services as a comparison group would violate the "stable unit treatment value assumption" (SUTVA). As suggested by Rubin (1990), SUTVA requires that an outcome observation on one unit should be unaffected by the assignment of treatments to the other units. To safeguard against interference between units, our quasi-experimental design focuses on the differences in shooting victimization and arrest outcomes for gang members who were directly exposed to the Ceasefire program (treated) and gang members who were indirectly exposed to Ceasefire gangs via rivalries and alliances (vicarious), relative to gang members who had no rivalry or alliance with a Ceasefire gang (non-treated) (Braga et al., 2013, 2019).

The SPD and OVP enhanced the Ceasefire strategy over the course of 2018 to be a more robust violence prevention intervention. These enhancements included improved data collection on treated clients subjected to the Ceasefire intervention. Due to these changes in program activities and data quality, we limited the evaluation to subjects treated between 2019 and 2021. The SPD provided a Ceasefire intervention database comprised of 1,080 gang members[3] treated between 2019 and 2021 who were not previous participants in this focused deterrence program.

Untreated gang members were identified through analysis of the 2022 SPD gang database. After excluding gang members who were previously part of the focused deterrence program, the initial untreated comparison group consisted of 1,608 gang members. The 2018 CPSC problem analysis identified active rivalries and alliances among Stockton gangs. The problem analysis results and the gang affiliations available in the SPD gang database were used to identify vicariously treated individuals.[4] Individuals were counted as being vicariously treated if they were socially connected through group rivalries and alliances to individuals treated by Ceasefire. This process identified 456 gang members who were socially connected to the Ceasefire intervention during the study period. This left a total of 1,152 untreated gang members for inclusion in the control group. The pooled study population of treated ($N = 1{,}080$), vicariously treated ($N = 456$), and untreated ($N = 1{,}152$) gang members accounted for 61.4% of all fatal and nonfatal shootings in Stockton (711 of total 1,158 shootings) between 2017 and 2022.

3.2 | Individual-level impact evaluation: Balance assessment across treated, vicariously treated, and untreated groups

The SPD and OVP data included information on sex, dates of birth, and race of the subjects in the treated, vicariously treated, and untreated groups. The SPD provided fatal and nonfatal shooting incident data between 2017 and 2022. The SPD also furnished California Law Enforcement Telecommunications System (CLETS) and extended arrest data to develop criminal histories of gang members included in the study groups. These covariates were used to assess whether any systematic differences existed among the subjects included in the three study groups. The individual characteristics of subjects in the treated, vicariously treated, and untreated groups were compared using Cohen's *d* standardized mean difference metrics (Cohen, 1988) and two-sample Kolmogorov–Smirnov (K-S) tests for equality of cumulative distribution functions to determine whether the groups were substantively different from each other based on individual characteristics, past shooting victimizations, and prior criminal histories. An omnibus *F* test was also used to assess whether the variance on these factors between groups was significantly greater than within groups.

The results of these comparisons showed that the study groups were generally alike, with most contrasts producing small mean differences and similar joint distributions (Table 1).[5] However, the comparisons also suggested that the treated group members were modestly younger[6] and included more non-White subjects relative to the untreated and vicariously treated groups. The joint *F* test rejected the null hypothesis of no difference between the group comparisons ($p < 0.001$). As such, our statistical models estimating the impact of the Ceasefire program on treated subjects relative to untreated subjects needed to include covariates to adjust for these modest differences. The available 2017–2022 outcome data on shooting victimizations and arrests allowed us to analyze individual outcomes for at least two years prior to Ceasefire treatment and at least one-year post treatment.

TABLE 1 Comparison of subject characteristics in the treated, vicariously treated, and untreated groups.

	Treated			Vicarious			Untreated	
	Mean	Std. dev.	D_T	Mean	Std. dev.	D_V	Mean	Std. dev.
Age	25.5	7.27	−0.27	29.54	6.61	−0.02	30.0	8.33
Male	0.97	0.18	0.02	0.96	0.19	0.01	0.96	0.20
Black	0.35	0.48	0.04	0.32	0.47	0.01	0.31	0.46
Asian	0.14	0.34	0.02	0.11	0.31	−0.03	0.12	0.33
White	0.02	0.13	−0.12	0.04	0.18	−0.06	0.07	0.25
Hispanic	0.50	0.50	−0.01	0.54	0.50	0.04	0.50	0.50
Gunshot victim	0.10	0.32	0.01	0.10	0.33	0.09	0.10	0.32
Prior arrests	8.92	8.46	−0.10	8.99	5.60	−0.10	10.71	8.84
Pr. violent arrests	2.64	3.22	−0.08	2.43	3.21	−0.10	3.18	3.69
N	1,080			456			1,152	

Notes: *D* = standardized mean difference, T = treated vs. untreated, V = vicarious vs. untreated. *K-S* test results are not shown. Joint *F* test = 7.29, *df* = (8; 2,679), $p < 0.001$.

3.3 | Individual-level impact evaluation: Statistical models

There were 2,688 unique subjects in the pooled impact evaluation database. The longitudinal analysis of the effects of Stockton Ceasefire on treated ($N = 1{,}080$) and vicarious treated ($N = 456$) gang members relative to untreated gang members ($N = 1{,}152$) considered counts of shooting victimizations, arrests, and violent arrests by quarter (2,688 × 4 quarters × 6 years = 64,512 possible observations) between 2017 and 2022. In total, 24 fatal gunshot wound victims in the pool were identified between 2019 and 2022 (15 untreated, 8 treated, and 1 vicariously treated). In the longitudinal database, missing values were entered in the quarters following the death quarter (total missing quarters = 179 in the database).

A variation of a multilevel regression model was used to analyze the quarterly change in outcomes for treatment and comparison group members during the six-year observation period. More specifically, individual random effects models were used to estimate individual changes in outcomes during the observation period (Gelman, 2005; Singer & Willet, 2003). The model estimated adjusts for the fact that each gang member may have a different average propensity for a given outcome. This is consistent with the variation observed in outcomes by subjects; that is, some gang members were highly active, and others were less active.

This analysis involved the estimation of the impact of Stockton Ceasefire on shooting victimizations, total arrests, and violent arrests of directly treated gang members and vicariously treated gang members relative to shooting victimizations, total arrests, and violent arrests of untreated gang members during the study period via the difference-in-differences (DID) estimator. The DID method estimates the difference in a treated gang and vicarious gang member's post-intervention outcome at time *t* compared with their pre-intervention outcome, relative to the same difference for the untreated gang members in the sample (see, e.g., Ridgeway et al., 2019). As such, the random effects regression model consisted of the following form:

$$Y_{it} = \beta_0 + \beta_1 Treated_i + \beta_2 Vicarious_i + \beta_3 Post_t + \beta_4 Treated_i \times Post_t + \beta_5 Vicarious_i \times Post_t + \beta X_{it} + \alpha_i, \quad (1)$$

In this model, Y_{it} represents whether a specific outcome for an individual gang member occurred in a given quarter during the six-year study period. The regressor $Treated_i$ is a dummy variable identifying whether a gang member was directly subjected to the Ceasefire intervention (1) or not (0), whereas $Vicarious_i$ is a dummy variable identifying whether a gang member was socially connected as an ally or rival to a treated gang (1) or not (0). The reference group in each case comprises gang members that were neither the direct nor indirect subjects of Stockton Ceasefire. The regressor $Post_t$ is a dummy variable for whether the quarter is during the post-intervention period (1) or during the pre-intervention period (0). The timing of the exact quarter in which each individual subject was directly or indirectly exposed to Ceasefire was used to determine the start of the post-intervention period. For untreated subjects, the post-intervention period started in January 2019. The coefficients β_4 and β_5, conforming to the product of the group dummies with the post-intervention period, are the DID estimates of the direct and vicarious effects of Ceasefire, respectively. The model also controlled for individual factors that may be correlated with the outcome measures, such as age, sex, race, total prior arrests, and the presence of a prior history of violent arrests; where β represents the vector of estimates of the matrix of attributes (X). The random effects intercept term is represented by the term α.

Stata 18 statistical software was used to calculate the maximum likelihood estimate of the parameters for the DID estimator and to compute the associated probability values. Poisson panel regression models were used to estimate the effects of direct and vicarious treatment on quarterly total arrests and violent arrests of subjects as these outcomes were distributed as rare event counts. Logistic panel regression models were used to estimate the effects of direct and vicarious treatment on quarterly shooting victimizations by subjects as this outcome followed a binary distribution (i.e., no gang member was shot more than once in a single quarter). The Poisson regression parameter estimates were expressed as incidence rate ratios (IRR), or the ratio of change in the quarterly counts of total arrests and violent arrests, respectively. The logistic regression parameter estimates were expressed as odds ratios (OR), or the odds that shooting victimization occurred in a quarter given treatment exposure compared with the odds of shooting victimization occurring in a quarter without treatment exposure. Robust standard errors clustered at the individual level were used to account for unmeasured dependence within subjects over time and overdispersion (Berk & MacDonald, 2008). As a robustness check, all count models were also estimated with a negative binomial variant of the Poisson regression and the substantive results did not change. Following convention, the two-tailed 0.05 level of significance was selected as the benchmark to reject the null hypothesis of "no difference."

Statistical analyses that involve multiple comparisons run the risk of reporting "false discoveries" as multiple simultaneous statistical tests are conducted (Miller, 1981). As the number of comparisons increases, it becomes increasingly likely that the two groups being compared will differ on some particular outcome. When compared with analyses that involve only a single outcome as a comparison, confidence in analyses that involve multiple comparison outcomes is generally weaker. In this study, 6 simultaneous comparisons were made (3 outcomes × 2 treatment contrasts). Many techniques can be used to correct multiple comparison problems by re-calculating probabilities obtained from a statistical test that was repeated multiple times. The traditional Bonferroni method has been suggested to be too conservative by imposing too stringent safeguards against Type 1 errors (Benjamini, 2010). In this analysis, we used the Hochberg correction to calculate family-wise error rate (FWER) adjusted p-values (Hochberg, 1988). For all outcome measures in the random effects models, the FWER procedure was used to

determine whether any significant results calculated through traditional *p*-values generated by the DID estimators were actually "false discoveries."

Two other modeling approaches were used to ensure that the results of these models were robust to alternative specifications. Fixed-effects panel regression models were estimated to remove the influence of any unmeasured variables that reflect time-invariant individual characteristics on individual outcomes. The fixed-effects regression model takes the following form:

$$Y_{it} = \beta_1 Treated_i + \propto_i + \beta t_i + u_{it}, \tag{2}$$

where Y_{it} represents the quarterly count of a specific outcome for an individual gang member during the six-year study period. The regressor $Treated_i$ is a dummy variable identifying whether a gang member was subjected to the Ceasefire intervention (1) or not (0) in a specific quarter and the coefficient β_1 is the estimated treatment effect on individual outcomes.[7] Three versions of this regressor were used in fixed-effects models: all treated (direct and vicarious combined) relative to untreated, directly treated relative to untreated, and vicariously treated relative to untreated. The fixed effect term for each individual i is represented by the term $\propto_i$. The model also controlled for secular trends by including quarter dummy variables and β represents the vector of estimates of these quarter dummy variables (t).

To assess how outcomes evolved in the quarters leading up to and after gang members were directly and vicariously exposed to the Ceasefire treatment, an event study regression model was estimated as an expansion of model (1) in the following form:

$$Y_{it} = \sum_{\substack{j=-8 \\ j\neq -1}}^{15} \theta_j 1(t = j) + \beta X_{it} + \alpha_i + \varepsilon_{it}. \tag{3}$$

The first set of parameters θ denotes indicators for each quarter relative to the exposure of gang members or vicarious members to treatment. Each event study model was estimated for the treated and vicarious group separately relative to the untreated control group. We exclude the quarter before (−1) the implementation of Ceasefire to serve as the reference period. The event study model controls for individual covariates, such as age, sex, race, and prior criminal history, reflected in the matrix of X attributes (Steinberg et al., 2019; Stevenson & Wolfers, 2006). Robust standard errors were clustered by individual gang members to adjust for unmeasured dependence at the individual level over time.

3.4 | City-level impact evaluation: Research design, data, and statistical models

A descriptive longitudinal analysis was conducted to assess the city-level impact of Ceasefire on total homicide trends in Stockton relative to other selected California cities. Eleven comparison cities in California were included in the analysis: Anaheim, Bakersfield, East Palo Alto, Fresno, Long Beach, Oakland, Richmond, Riverside, Sacramento, San Francisco, and Santa Ana. These cities represented larger municipalities with populations between 300,000 and 900,000 residents and violent crime rates greater than 200 per 100,000 residents. The smaller cities of East Palo Alto and Richmond were included due to their very high violent crime rates. FBI Supplementary

Homicide Reports data were accessed for these cities through the California Open Justice data portal managed by the California Department of Justice.[8] Trends in monthly counts of homicides for Stockon and the 11 comparison cities were assessed for the 26-year period between January 1, 1997, and December 31, 2022. The units of analysis were 3,744 "city-months" (12 cities × 12 months × 26 years).

DID estimators were used to calculate the impact of Stockton Ceasefire on monthly counts of total homicides during active intervention periods (1998–2002, 2006–2008, and 2018–2022) relative to monthly counts of total homicides in comparison cities. Our general random effects panel regression model was as follows:

$$Y_{it} = \beta_0 + \beta_1 Stockton_i + \beta_2 Ceasefire_t + \beta_3 Stockton_i \times Ceasefire_t + \beta_4 t + \beta_5 t^2 + \theta_q + \alpha_i, \quad (4)$$

where Y_{it} represents the count of total homicides for a specific city in a given month during the 26-six-year study period. The regressor $Stockton_i$ is a dummy variable identifying Stockton (1) or the 11 comparison cities (0). The regressor $Ceasefire_t$ is a dummy variable for whether the month is during the Stockton Ceasefire intervention period (1) or not (0). The coefficient β_3, conforming to the product of the $Stockton_i$ dummy with the $Ceasefire_t$ dummy, is the DID estimate of the Stockton Ceasefire treatment effect. The model also controlled for monthly linear trends (t), monthly quadratic (t^2), and quarterly variations (q), represented by coefficients β_4, β_5 and the vector θ . The city-level (i) random effects intercept term is represented by the term α.

A second random effects panel regression model was specified with three DID estimators each representing specific periods that Ceasefire was active in Stockton (DID1 = 1998–2002, DID2 = 2006–2008, DID3 = 2018–2022). Poisson panel regression models were used to estimate the effects of the Ceasefire intervention in Stockton on monthly total homicide counts as these outcomes were distributed as rare event counts. Robust standard errors clustered at the city level were used to account for unmeasured dependence within cities over time and overdispersion (Berk & MacDonald, 2008). All models were also estimated with negative binomial variants of the Poisson regression as a robustness check, with substantively similar results. Poisson regression parameter estimates were expressed as IRR. To ensure that the results for were not sensitive to use of random-effects specifications, models were also estimated that included city, year, and month fixed-effects with standard errors clustered at the city-level. The results were substantively similar to the random-effects specifications. Table 2 presents 2022 demographic, crime, and employment data for Stockton and the 11 comparison cities included in this analysis.

4 | IMPACT EVALUATION RESULTS

4.1 | Individual-level impact evaluation results

Table 3 presents the results of the random effects panel regression models assessing the impact of the Ceasefire intervention on outcomes for directly treated and vicariously treated gang members relative to untreated gang members controlling for individual characteristics. The Stockton Ceasefire program resulted in statistically significant reductions in the likelihood of shooting victimization for directly treated gang members and marginally signification reductions in the likelihood of shooting victimization for vicariously treated gang members. Ceasefire was associated with a statistically significant 39.8% decrease ($p < 0.01$) in the odds that a directly treated gang member was shot, holding the other variables constant. Similarly, Ceasefire was associated

TABLE 2 Demographic, crime, and employment data for Stockton and selected California cities, 2022.

City	Population	Violent crime rate per 100,000	Percent Black residents	Percent persons below poverty	Unemployment rate
Stockton	321,911	1,162.43	15.6%	16.3%	37.0%
Oakland	421,806	1,544.79	24.0%	13.5%	31.7%
Sacramento	516,958	918.64	16.0%	14.8%	35.6%
Richmond	114,521	878.44	23.2%	13.4%	37.0%
Fresno	542,829	870.99	8.6%	22.9%	35.7%
Anaheim	335,946	786.14	2.9%	13.0%	33.2%
San Francisco	837,036	635.93	6.6%	10.3%	31.0%
East Palo Alto	28,776	618.57	13.6%	12.0%	29.8%
Bakersfield	407,491	551.42	7.5%	16.3%	36.3%
Long Beach	460,245	529.72	14.3%	15.4%	33.7%
Riverside	314,818	524.75	9.1%	12.9%	34.2%
Santa Ana	304,258	203.77	1.8%	12.3%	33.3%

Notes: Violent crime data collected from the FBI's Uniform Crime Reports. Population, racial composition, poverty, and employment data collected from the U.S. Census Bureau.

with a 36.9% reduction ($p = 0.051$) in the odds that a vicariously treated gang member was shot controlling for the other covariates.[9]

The random effects panel regression models showed that Stockton Ceasefire resulted in statistically significant reductions in recidivism by directly treated gang members. Controlling for the other covariates, Ceasefire was associated with a 24.6% reduction ($p < 0.001$) in total arrests and a 36.7% reduction in violent arrests by directly treated gang members ($p < 0.001$). Stockton Ceasefire did not reduce general recidivism by vicariously treated gang members. However, the program was associated with a 6.2% decrease in violent arrests by vicariously treated gang members ($p < 0.01$).

Table 4 presents the results of the FWER procedure used to determine whether any significant results calculated through traditional *p*-values generated by the DID estimators in the random effects models may be false discoveries. The FWER *p*-values were congruent with the *p*-values generated by the direct impact DID estimators and confirmed that the Stockton Ceasefire intervention generated statistically significant reductions in shooting victimizations, total arrests, and violent arrests of directly treated gang members. However, the FWER-adjusted *p*-values were not significant at a 5% level needed to reject the null hypothesis of no effects on outcomes for the vicariously treated gang members. The vicarious impact of Ceasefire on violent arrests was statistically significant for violent arrests and marginally significant for shooting victimization at the less restrictive 10% significance level.

Table 5 presents the results of the fixed-effects panel regression models assessing the impact of the Ceasefire intervention on outcomes for all treated gang members (direct and vicarious combined), directly treated gang members, and vicariously treated gang members relative to untreated gang members controlling for individual characteristics. As discussed earlier, the panel fixed-effects models dropped subjects that did not experience an outcome event over the course of the study period. This reduced the total number of cases included in each regression analysis relative to the random effects models. The fixed-effects models showed that Stockton Ceasefire was associated with a 40.4% reduction in the odds that treated gang members (direct and vicari-

TABLE 3 Random effects panel regressions of the direct and vicarious effects of Ceasefire on shooting victimization, total arrests, and violent arrests.

	Shooting victim OR (RSE)	Total arrests IRR (RSE)	Violent arrests IRR (RSE)
Direct effect	0.602**	0.754***	0.633***
	(0.102)	(0.0171)	(0.0174)
Vicarious effect	0.631	0.994	0.938*
	(0.149)	(0.0242)	(0.0269)
Treated gang member	0.778	0.965	1.050
	(0.110)	(0.0265)	(0.0313)
Connected gang member	1.024	1.218***	1.270***
	(0.169)	(0.0416)	(0.0526)
Post-treatment	1.199	0.995	0.986
	(0.146)	(0.0142)	(0.0172)
Age at 01/01/2020	0.938***	0.944***	0.940***
	(0.00631)	(0.00320)	(0.00477)
Black subject race	2.820***	0.920	1.050
	(0.704)	(0.0497)	(0.0570)
Hispanic subject race	1.875*	0.972	1.082
	(0.467)	(0.0506)	(0.0588)
Asian subject race	1.625	0.917	0.997
	(0.446)	(0.0534)	(0.0597)
Male	1.070	1.005	1.037
	(0.216)	(0.0510)	(0.0553)
N prior arrests	1.015**	1.033***	1.031***
	(0.00501)	(0.00278)	(0.00385)
N prior violent arrests	1.207	1.234***	1.389***
	(0.125)	(0.0353)	(0.0418)
N	64,333	64,333	64,333

Notes: Robust standard errors (RSE) were clustered on individuals. White was the reference category for the race dummy variable. OR = odds ratio. IRR = incidence rate ratio.

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

TABLE 4 False discovery rates for random effects panel regression models.

	p-value	FWER-adjusted *p*-value
Direct effects		
Shooting victimization	0.0028	0.0111
Total arrests	0.0001	0.0001
Violent arrests	0.0001	0.0001
Vicarious effects		
Shooting victimization	0.0512	0.1024
Total arrests	0.8186	0.8186
Violent arrests	0.0259	0.0779

TABLE 5 Fixed-effects panel regressions of the total, direct, and vicarious effects of Ceasefire on shooting victimization, total arrests, and violent arrests.

	Shooting victim OR (SE)	Total arrests IRR (SE)	Violent arrests IRR (SE)
Total effect	0.596**	0.837***	0.721***
	(0.0852)	(0.0143)	(0.0152)
N	13,875	62,917	60,637
Direct effect	0.653**	0.762**	0.627**
	(0.106)	(0.0156)	(0.0154)
N	11,571	52,107	50,019
Vicarious effect	0.506**	1.015	0.970
	(0.117)	(0.0230)	(0.0254)
N	9,172	37,382	35,822

Notes: Models include fixed quarter effects. OR = odds ratio. IRR = incidence rate ratio.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

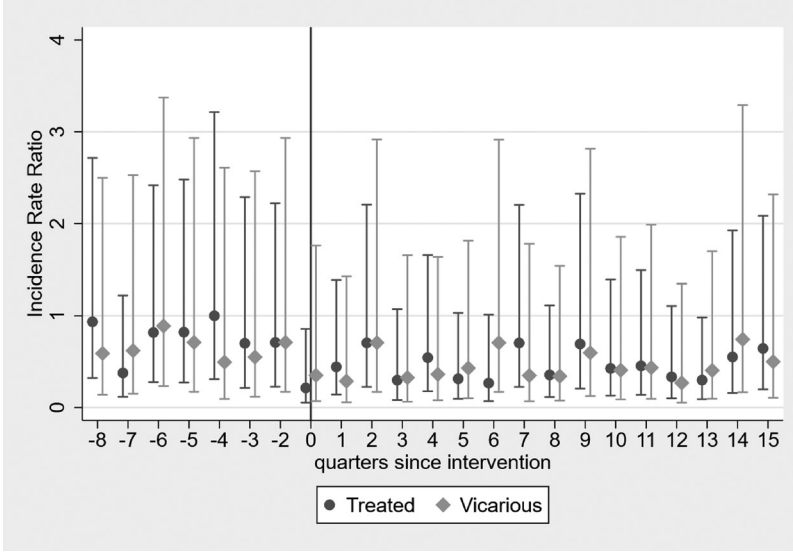

FIGURE 1 Shooting victimization: Event study model coefficients.

ous combined) were shot, as well as associated with a 16.3% reduction in total arrests and 27.9% reduction in violent arrests by treated gang members. These estimated Ceasefire impacts were largely driven by gang members who were directly subjected to treatment. However, the fixed-effects models suggested that Ceasefire was associated with a 49.4% reduction in the odds that vicariously treated gang members were shot.

Figures 1, 2, and 3 present graphs visualizing the event study model results for shooting victimization, total arrests, and violent arrests, respectively.[10] For both direct and vicariously treated gang members, the figures display the quarter-specific coefficients and 95% confidence intervals for the seven quarters before and 15 quarters after Stockton Ceasefire implementation. The coefficients in the figures generally show that the quarterly counts of outcomes before Ceasefire implementation were associated with increased numbers (IRR closer to 1), whereas the monthly

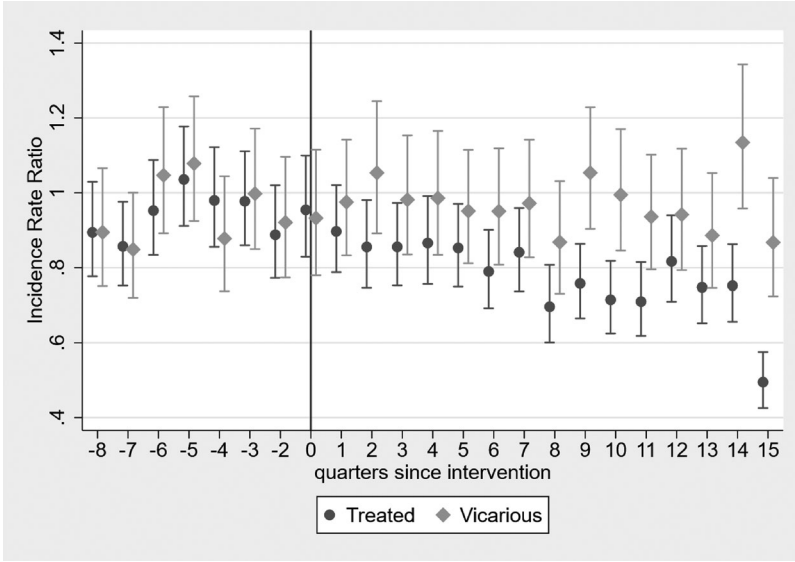

FIGURE 2 Total arrests: Event study model coefficients.

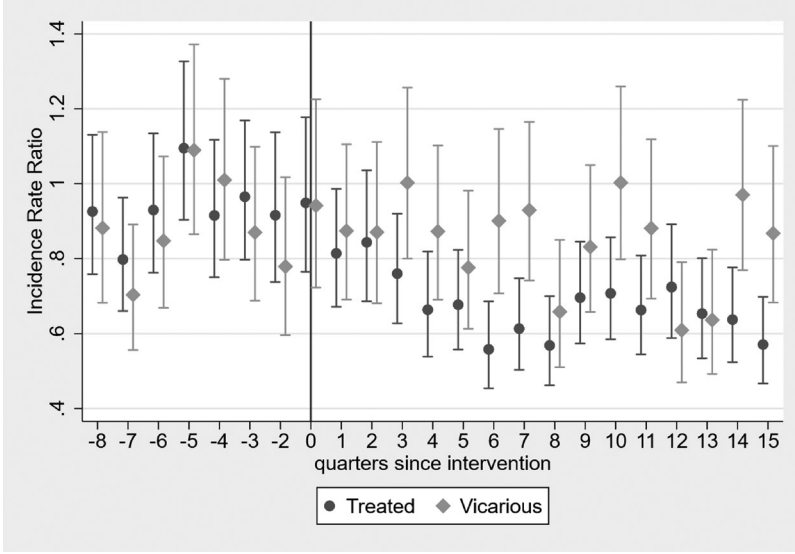

FIGURE 3 Violent arrests: Event study model coefficients.

counts of outcomes after program implementation were associated with decreased numbers (IRR further away from 1).

4.2 | City-level impact evaluation results

Table 6 presents the results of the random effects panel regression models estimating the impact of Stockton Ceasefire on citywide monthly counts of total homicides during active intervention periods relative to citywide monthly counts of total homicides in comparison cities. During the

TABLE 6 Random effects panel regressions of the effects of Stockton Ceasefire on citywide total homicide trends relative to selected California comparison cities.

Variable	Model 1 IRR (RSE)	Model 2 IRR (RSE)
Stockton	1.357 (0.287)	–
Ceasefire (all periods)	1.075 (0.029)**	–
DID (all periods)	0.700 (0.015)**	–
Stockton	–	1.357 (0.287)
Ceasefire 98–02 period	–	0.878 (0.048)*
Ceasefire 06–08 period	–	1.255 (0.059)**
Ceasefire 18–22 period	–	1.073 (0.066)
DID 98–02 period	–	0.668 (0.043)**
DID 06–08 period	–	0.507 (0.029)**
DID 18–22 period	–	0.873 (0.056)*
Trend	1.000 (0.001)	0.998 (0.002)*
Trend^2	1.000 (0.000)	1.001 (0.000)
Mar–May	1.060 (0.019)**	1.059 (0.019)**
Jun–Aug	1.189 (0.030)**	1.190 (0.030)**
Sep–Nov	1.092 (0.032)**	1.095 (0.032)**
Intercept	2.671 (0.426)**	3.236 (0.600)**
Ln(alpha)	−0.853	−0.853
N observations	3,744	3,744
N groups	12	12
Wald chi-square (*df*)	1,137.06 (8)	32,340.40 (12)
Log pseudolikelihood	−7,260.50	−7,217.05

Notes: December, January, and February are used as the reference category for other seasonal dummy variables. DID = differences-in-differences estimator. Robust standard errors (RSE) were clustered on cities. IRR = incidence rate ratio.
*$p < 0.05$; **$p < 0.01$.

three periods when Ceasefire was active, the overall DID estimator suggests that monthly homicide counts in Stockton were reduced by 30% ($p < 0.01$) relative to monthly homicide counts in the 11 comparison cities during these same time periods controlling for secular trends and seasonal variations (Model 1). The three distinct DID estimators suggest that Ceasefire was associated with statistically significant reductions in homicide in each period that the strategy was fully implemented. Controlling for trends and seasonal variations, Stockton Ceasefire was associated with a 33.2% reduction during the 1998–2002 implementation ($p < 0.01$), a 49.3% reduction during the 2006–2008 implementation ($p < 0.01$), and a 12.7% reduction during the 2018–2022 implementation ($p < 0.05$) in monthly homicide counts in Stockton relative to monthly homicide counts in the 11 comparison cities during the same periods (Model 2).

5 | CONCLUSION

The City of Stockton has an extended history of using focused deterrence to reduce serious gang violence. The lack of a formal management accountability structure in city government made

previous implementations in 1998–2002 and 2006–2008 vulnerable to program disruption in the face of financial challenges and personnel turnover. The most recent iteration of the Stockton Ceasefire–focused deterrence strategy has been fully operational since 2018. City officials, police executives, and community leaders developed the capacities to implement a reinvigorated focused deterrence strategy with an emphasis on building structures to ensure sustainability and applying procedural justice and legitimacy building actions to enhance program effectiveness. Key components of this approach included establishing OVP to better support gang outreach by Peacekeepers and provide more robust services and opportunities to gang members who wanted them, improving SPD intelligence collection and analysis to enhance focus on violent gangs actively engaged in shootings, and revamping communications with at-risk gang members to emphasize personal safety concerns rather than threats of punishment. These strategic changes were coupled with the establishment of performance management systems, such as shooting incident reviews, OVP caseload reviews, Ceasefire coordination meetings, and StocktonStat.

The impact evaluation suggests that Stockton Ceasefire was effective at reducing the shooting victimization of treated gang members. Statistical models estimated that treated gang members were 40% less likely to be shot after exposure to the Ceasefire intervention relative to untreated gang members. These findings were robust to several different modeling approaches and *p*-values that were adjusted for multiple comparison tests. The analysis also showed that these violence reduction impacts spilled over to gang members who were not the direct targets of Ceasefire intervention but were socially connected to directly treated gang members through alliances and rivalries. Statistical models found that these vicariously treated gang members were less likely to be shot after indirect Ceasefire exposure relative to untreated gang members. These results were robust to different modeling approaches but not after considering multiple comparison tests. Nevertheless, these spillover effects are suggestive ofCeasefire gun violence reduction impacts that diffuse through social networks in ways that produce public safety benefits beyond those who directly receive the treatment.

The impact evaluation also found that Stockton Ceasefire was effective at reducing recidivism by treated gang members (which, by extension, would also make them less vulnerable to sanctions that result in incarceration). Statistical models estimated that total recidivism rates were reduced by 25% and violent recidivism rates were reduced by 37% for treated gang members relative to untreated gang members. These effects were also robust to alternate modeling specifications and *p*-values that adjust for multiple comparison tests. In contrast to the spillover shooting reduction impacts, the Ceasefire program did not reduce total recidivism by vicariously treated gang members and may have generated small effects on their violent recidivism that were not robust and marginally significant at best. These divergent spillover effect findings may be due to the nature of the intervention. The diminished violent victimization and offending of treated gang members make their rivals less likely to be shot as ongoing conflicts abated. However, vicariously treated gang members did not seem to change their more general offending behaviors.

Stockton, like other cities in California and elsewhere in the United States, experienced large increases in gun violence during the early 2020s. The March 2020 onset of the COVID-19 pandemic forced U.S. police departments to modify their routine operations to ensure that critical public health precautions were engaged while policing services were provided to the public. The management of racial justice protests in the wake of the late May 2020 murder of George Floyd by Minneapolis police officers further strained police department resources in many jurisdictions. Demoralizing calls to "defund the police" led to modest decreases in police budgets in some cities such as Los Angeles, Minneapolis, and New York City (e.g., Gross & Eligon, 2020; Levintova, 2020; Mays, 2020). Starting in 2020 and continuing through 2021, many police departments

experienced small but meaningful declines in officer numbers due to increased resignations and retirements coupled with difficulties in recruiting new officers (Police Executive Research Forum, 2023). Given the association between de-policing and subsequent increases in violence (Braga & Cook, 2023; Devi & Fryer, 2020; Nix et al., 2024), it is perhaps not surprising that many U.S. cities experienced sudden increases in homicides and nonfatal shootings in the summer of 2020. A small change in marginal deterrence among the small number of gangs and other criminal groups that drive violence in a city, such as the diminished capacity of police departments to launch proactive violence prevention actions in 2020 and subsequent years, can generate an outsized increase in serious gun violence (MacDonald, 2023).

Our analyses suggest that Stockton Ceasefire prevented the city from experiencing an upturn in homicide that was as sharp as what occurred in other California cities following the COVID-19 pandemic and Floyd protests. Indeed, the comparative citywide analyses reveal homicide counts that were almost 13% lower than homicide counts in selected California cities during the 2018–2022 period. This is a notably weaker citywide effect than the larger homicide reductions observed during the prior two Ceasefire implementations in 1998–2002 and 2006–2008. This smaller citywide impact seems to be linked to a program caseload that diminished by 56% from 513 treated gang members in 2019 to only 225 in 2021. Nevertheless, given extremely difficult post-2020 governance challenges and multiple leadership changes, it is noteworthy that the City of Stockton maintained a Ceasefire implementation that resulted in any public safety gains during this period. Strong leadership, effective management and performance review structures, and healthy collaborations among criminal justice, social service, and community-based organizations prevented the Ceasefire strategy from completely falling apart during these trying times. Other jurisdictions looking to implement similar focused deterrence strategies need to ensure that these critical elements are in place to keep their cities safe during inevitable and often unforeseeable challenges that will arise. These much-needed arrangements will help prevent the implementation failures that characterize many cities' past experiences with this approach and have the potential to maintain critical strategic violence prevention work through unexpected crises.

Like most other social science inquiries, the findings of this evaluation should be interpreted with caution. The impact evaluation did not use a randomized controlled trial design and, as such, limited causal evidence exists of Ceasefire program impacts. The available official data on program participation were not collected in a manner that allowed the precise identification of violence prevention mechanisms (such as incapacitation, deterrence, or rehabilitation) associated with the observed impacts. As such, we were not able to assess the efficacy of key program components. Data on individual incarcerations and associated sentence lengths were not available to the authors. We were not able to examine possible incarceration effects on individual outcomes and assess whether any observed censoring had differential impacts on treated and untreated subjects during post-intervention periods. Further, the cross-city analyses represent superficial accounts of homicide trends in the comparison cities that do not account for any gun violence reduction interventions that may have been in place during the study period.

Despite these limitations, this study provides rigorous evaluation evidence that focused deterrence can be designed and implemented in ways that guard against eventual implementation failure. In short, focused deterrence programs can be built to last. Preventing steep downturns in marginal deterrence and ensuring the steady delivery of services to the small number of high-risk people who drive gun violence enhances public safety. In Stockton, established gun violence prevention systems faced daunting challenges during the study period. Although treatment caseloads declined as intervention capacities continued to be strained, the changes in violent gun

victimization and violent recidivism for those gang members who were treated seemed to prevent COVID-19 era increases in Stockton gun violence from being as steep as they could have been.

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CONFLICT OF INTEREST STATEMENT

The authors declare no conflicts of interest.

ENDNOTES

[1] The number of evaluations included in the area, group, and individual units of analysis categories are not mutually exclusive. Tita et al. (2004) evaluated the impacts of the Los Angeles Ceasefire group violence reduction strategy on violent crime outcomes at both the gang level and the neighborhood level.

[2] Source: openjustice.doj.ca.gov/exploration/crime-statistics/crimes-clearances (accessed July 6, 2023).

[3] The SPD follows California State law in defining street gangs and gang members. However, SPD also relies on its working knowledge and intelligence of the Stockton Gang Violence Reduction Unit and Crime Response Teams to track specific gang sets, less formal street robbery crews, drug organizations, and other criminally active groups. California penal code states that "A criminal street gang is any ongoing organization, association, or group of three or more persons, whether formal or informal: 1. That has a common name or common identifying sign or symbol; 2. That has, as one or more of its primary activities, the commission of [a crime listed in Pen. Code §186.22(e)(1)–(25), (31)–(33)]; and 3. Whose members, whether acting alone or together, engage in or have engaged in a pattern of criminal gang activity." See California Criminal Jury Instructions 1401 (CALCRIM) (2020). See also Section 752.4: Criteria to Be Designated as a Gang Member or Associate.

[4] Over the past 25 years, ongoing problem analyses have shown that the groups comprising the Stockton gang scene were very stable over time and had persistent conflicts and alliances that fell largely within Asian, Hispanic, and Black racial groups (Braga, 2005; CPSC, 2012, 2018; Wakeling, 2003). Larger Asian gangs, such as the Asian Boys, Crazy Brother Clan, Tiny Rascal Gang, Loc Town Crips, and Moon Light Strangers, were active throughout the study period and had social connections through long-standing "Blood" and "Crip" rivalries. Conflicts among Hispanic gangs mainly involved a very violent long-standing rivalry between Norteño gangs (associated with criminal groups and gangs in Northern California) and Sureño gangs (associated with criminal groups and gangs in Southern California). The Sureños were usually not comprised of smaller gang sets during the study period. However, the Norteños were comprised of multiple gang sets, such as Southside Stocktone, Eastside Stocktone, Barrio Conway, Westside Norteños, and 6th Street, that were persistently active over time. Black gangs were similarly stable with Blood and Crip groups such as Broadway Gangster Crips, East Coast Crips, Westside Bloods, Glock Team, and Northside Gangster Crips. Given the relative stability of Stockton gangs and their conflicts and alliances, the 2018 problem analysis results seemed appropriate to use in our designation of social connections among directly and vicariously treated gang members for the 2017–2022 evaluation period.

[5] We follow convention in referring to small mean differences as those that are less than 0.20 standard deviations. Statisticians recommend using a comparison of average differences, like a standardized effect size, rather than a test statistic and *p*-value (Imai et al., 2008).

[6] In all analytical work, subject ages were calculated as the chronological differences between their dates of birth and the midpoint of the study period (January 1, 2020).

[7] In each regression model, individuals who did not experience an outcome (i.e., shooting victimization, arrest, or violent arrest) in any quarters were dropped from the analysis to a lack of variation over time. As shown in the Impact Evaluation Results section, large numbers of cases were excluded from the fixed-effects analysis. This resulted in the collapse of gang members in the "direct treatment" and "vicarious treatment" groups into a single treatment group in the fixed-effects analysis.

[8] openjustice.doj.ca.gov/data (accessed November 12, 2023).

[9] The strong association between subject age and shooting victimization risk influenced the precision of the vicarious treatment estimate. In general, older subjects were significantly less likely to suffer gunshot victimizations during the study period. When the model was limited to subjects ages 40 and younger, Ceasefire was associated with a statistically significant 28.7% decrease (IRR = 0.713, RSE = 0.118, $p < 0.05$) in the odds that a vicariously treated gang member was shot, holding the other variables constant. The total number of observations included in the analysis decreased by 9.2% from 64,333 to 58,429 because of excluding subjects older than 40 years.

[10] The parallel trends assumption, requiring that the difference between treatment gangs and comparison gangs is constant over time, is critical to the internal validity of the DID model (Angrist & Pischke, 2009). Our analyses suggest that the parallel trends assumption was met. A visual inspection of the estimates and 95% confidence intervals in Figures 1, 2, and 3 show that no statistically significant differences existed in pretreatment trends in the shooting victimization, arrest, and violent arrest outcome measures between the treatment and control groups.

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