Most Challenging of Contexts

Assessing the Impact of Focused Deterrence on Serious Violence in New Orleans

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Research Summary

The use of focused deterrence to reduce lethal violence driven by gangs and groups of chronic offenders has continued to expand since the initial Boston Ceasefire intervention in the 1990s, where prior evaluations have shown relatively consistent promise in terms of violence reduction. This study focuses on the capacity of focused deterrence to impact lethal violence in a chronic and high-trajectory homicide setting: New Orleans, Louisiana. Using a two-phase analytical design, our evaluation of the Group Violence Reduction Strategy (GVRS) observed the following findings: (a) GVRS team members in the City of New Orleans closely followed model implementation; (b) homicides in New Orleans experienced a statistically significant reduction above and beyond changes observed in comparable lethally violent cities; (c) the greatest changes in targeted outcomes were observed in gang homicides, young Black male homicides, and firearms violence; and (d) the decline in targeted violence corresponded with the

This project was funded by the New Orleans Community Support Foundation. Points of view or opinions in this document are those of the authors and do not represent the official position or policies of the City of New Orleans. The authors would like to thank Anthony Braga and the anonymous reviewers for their helpful feedback and comments. We are also grateful to the New Orleans Police Department and the City of New Orleans officials for their efforts and commitment to the project. Additionally, this project benefited from considerable guidance from David Kennedy and the National Network for Safe Communities. We also thank Murat Ozer, Jeffrey Clutter, Samantha Henderson, and Sam Peterson from the University of Cincinnati Institute of Crime Science for their assistance with data collection. Direct correspondence to Nicholas Corsaro, School of Criminal Justice, University of Cincinnati, P.O. Box 210389, Cincinnati, OH 45221 (e-mail: nicholas.corsaro@uc.edu).

DOI:10.1111/1745-9133.12142

implementation of the pulling levers notification meetings. Moreover, the observed reduction in crime outcomes was not empirically associated with a complementary violence-reduction strategy that was simultaneously implemented in a small geographic area within the city.

Policy Implications

The findings presented in this article demonstrate that focused deterrence holds considerable promise as a violence prevention approach in urban contexts with persistent histories of lethal violence, heightened disadvantage, and undermined police (and institutional) legitimacy. The development of a multiagency task force, combined with unwavering political support from the highest levels of government within the city, were likely linked to high programmatic fidelity. Organizationally, the development of a program manager and intelligence analyst, along with the use of detailed problem analyses and the integration of research, assisted the New Orleans working group in identifying the highest risk groups of violent offenders to target for the GVRS notification sessions. The impacts on targeted violence were robust and consistent with the timing of the intervention.

> "(In 2004), researchers conducted an experiment in which police fired 700 blank rounds in a New Orleans neighborhood in a single afternoon. No one reported the gunfire." (Associated Press, 2005, para. 1)

The City of New Orleans has a rich history with unique cultural traditions (Hirsch and Logsdon, 1992). Although perhaps best known for its blend of Creole, French, and African cultures that influences its music, cuisine, activities, and celebrations, it is also well known as a city of misfortune, ravished by Hurricane Katrina in 2005, enduring decades of political corruption, police misconduct, poverty, and persistently high levels of lethal violence (Moore, 2010). The city demonstrates both the best and the worst of urban America, and it is where the most innovative criminal justice reform efforts are most needed but are least likely to succeed. Within this challenging context, city and public officials adopted a violence-reduction strategy to combat persistent patterns of lethal gun violence.

Since the early 1990s, the national urban homicide rate in the United States has hovered between 13.0 and 20.0 per 100,000 in large U.S. cities with a population of 250,000 and greater, and approximately between 8.0 and 10.0 in all U.S. cities with 100,000 or more residents (Uniform Crime Reports, 2013). Based on Uniform Crime Reports homicide data for the years 2008–2012, New Orleans averaged 55 homicides per 100,000 residents. Additionally, since the mid-1970s, New Orleans has maintained a citywide homicide rate trajectory that is significantly higher than 90% of all U.S. cities (McCall, Land, and Parker, 2011). Certainly, the underlying factors that contribute to lethal violence within New

Orleans have been lasting—and require an innovative approach to disrupt the stability in the city's persistently high homicide levels. As such, city officials implemented the Group Violence Reduction Strategy (GVRS) to address the conditions and dynamics that generate the ongoing criminal homicide problem.

The New Orleans GVRS drew on the focused deterrence framework (Kennedy, 1997, 2009a) and operated through interagency partnerships to impact persistent citywide patterns in violence by using data-driven approaches (i.e., homicide incident reviews and gang audits) to identify key offenders (specifically gangs and criminally active groups) responsible for a disproportionate share of the city's serious violence. Officials in New Orleans attempted to increase the perceived risk of apprehension as well as the consequences leveraged against gangs and criminally active groups through offender notification strategies backed up by credible enforcement actions. The approach was specifically designed to halt ongoing violent behavior by the most violent gangs and groups in the city. In the current study, we conduct an empirical evaluation of the New Orleans GVRS first implemented by the City of New Orleans and the New Orleans Police Department (NOPD) in 2012.

This article proceeds as follows: First, we review briefly the principles that guide focused deterrence violence-reduction strategies and the body of evidence that demonstrates their potential effectiveness. Second, we discuss the distinctive context of New Orleans by outlining its unique challenges and describe the problem analyses used by the local criminal justice working group to identify which gangs and groups drive lethal violence within the city. We likewise document the specific tactics that were used to enhance the perceived risk of future sanctions against such gangs and groups. Third, we present a two-phase quasi-experimental design to examine the potential impact of the New Orleans GVRS. Despite the challenging context presented in the City of New Orleans, the findings of this study demonstrate that the focused deterrence initiative corresponded with statistically significant reductions in targeted violence, which likewise corresponded with the timing of the intervention. Finally, we conclude with a discussion of the importance of these findings for both research and practice. This study adds to the growing body of literature that shows focused deterrence has the potential to impact persistent violence in some of the most challenging urban contexts.

Literature Review

A deterrence-based theoretical framework posits that three primary components deter high-risk individuals from engaging in future patterns of offending: the certainty, severity, and celerity of punishment (Cook, 1980; Gibbs, 1975; Nagin, 1998; Paternoster, 1987; Zimring and Hawkins, 1973). Based on an extensive review of both classic and contemporary studies that assessed the various dimensions of deterrence, Nagin (2013) noted that there is little evidence that severity-based deterrence approaches (e.g., life without parole or "three-strikes" laws) are effective (see also Durlauf and Nagin, 2011); however, the evidence in support of deterrent effects related to the certainty of punishment is far more consistent. Moreover, Nagin contended that most compelling deterrent effects are seemingly linked to the police generating an increased *certainty of apprehension,* which can be accomplished in one of two ways:

1. By apprehending the suspect
2. By creating the perception that apprehension risk is sufficiently high

A promising deterrence-based crime prevention initiative, pioneered in Boston during the 1990s, was the "pulling levers" violence-reduction strategy. The well-known Operation Ceasefire initiative was developed in an effort to increase the risks of criminal justice sanctions faced by active, chronic offenders (Braga, Kennedy, Waring, and Piehl, 2001; Kennedy, 1997, 2009a). The cornerstone of the pulling levers focused deterrence strategy is to communicate incentives and disincentives directly to targeted chronic violent offenders to curb illicit and violent behavior and to obtain positive crime control gains. To change the perception of apprehension risk, highly active and violent street gangs (and other criminally active groups) are summoned by the police to "call-in" sessions to make them aware of the specific penalties that will be leveraged against each individual associated with the group if any member continues to engage in serious violence after the notification session.

Deterrence in these types of interventions is most likely achieved by identifying and delivering a message of swiftness and certainty of apprehension (and punishment) to groups of chronically violent offenders who are responsible for most of the city's crime problems. Ultimately, practitioners across various criminal justice agencies work together to convey that violence will no longer be tolerated and that further violations will be followed with legal, but harsh, sanctions available when future violence occurs (Kennedy, 1997, 2009a). High-risk groups of offenders are susceptible to coordinated criminal justice responses such as implementing strict probation and parole enforcement, shutting down otherwise nonviolent sources of income (e.g., gambling houses), paying attention to low-level street crimes such as public intoxication, and ensuring direct and nonlenient prosecutorial attention.

Focused deterrence is rooted in the problem-oriented policing framework. This facilitates the customization of the focused deterrence approach to local conditions. Problem-oriented policing is a highly focused law enforcement approach that is designed to assess, recognize, and disrupt the underlying causes behind chronic crime problems (Goldstein, 1979). In a review of the most effective police approaches to crime prevention, the National Academy of Sciences (2004) described the problem-oriented policing model as follows:

> The heart of problem-oriented policing is that this concept calls on police to analyze problems, which can include learning more about victims as well as offenders, and to consider carefully why they came together where they did. The interconnectedness of person, place, and seemingly unrelated events needs to be examined and documented. Then police are to craft responses that may go beyond traditional police practices. (National Research Council, 2004: 91)

What is perhaps most significant, from an organizational standpoint, is that the problem-oriented policing model (including focused deterrence) is achieved by (a) diagnosing local problems, (b) using research to inform the problem analysis, (c) developing interagency partnerships to address local crime problems, and (d) customizing the operational strategy locally to build long-term capacity. Indeed, the problem-oriented policing model has shown considerable impact on targeted crime problems (National Research Council, 2004; Weisburd, Telep, Hinkle, and Eck, 2010).

It is also worth noting that the notification sessions are held publicly in crime-stricken communities (e.g., neighborhood churches) to illustrate a collective public response to the violence (see Kennedy, 2009a). An emerging body of research has framed the use of offender notification meetings as a way to enhance the perceived legitimacy of the criminal justice system by providing an unbiased and procedurally just response to violence by involving multiple members from the criminal justice agency, citizens, and clergy from high-risk communities in the "deterrence-based" (i.e., threat of enhanced sanctions) notification processes (see Brunson, Braga, Hureau, and Pegram, 2013; Papachristos, Meares, and Fagan, 2007; Wallace, Papachristos, Meares, and Fagan, forthcoming). Thus, various sound theoretical perspectives and organizational approaches guide focused deterrence in real-world settings.

Prior Evaluations of Focused Deterrence Strategies

In field settings that have focused on group and gang violence, reductions in the levels of citywide and/or community violent crime, homicide, and gang-related (or group-related) violence have been observed in Boston (Braga et al., 2001; Piehl, Cooper, Braga, and Kennedy, 2003), Chicago (Papachristos et al., 2007), Cincinnati (Engel, Tillyer, and Corsaro, 2013), Lowell (Braga, Pierce, McDevitt, Bond, and Cronin, 2008), Indianapolis (McGarrell, Chermak, Wilson, and Corsaro, 2006), and Los Angeles (Tita et al., 2004), and Stockton (Braga, 2008). Similar crime-prevention benefits have been documented in urban settings attempting to decrease drug-market–related neighborhood crime when drawing on the focused deterrence model (Corsaro, 2013; Corsaro, Brunson, and McGarrell, 2010; Corsaro, Hunt, Hipple, and McGarrell, 2012; Saunders, Lundberg, Braga, Ridgeway, and Miles, 2014). The timing of the broader reduction in violence in each of these high-risk urban settings typically corresponds with the onset of programmatic implementation. In a recently completed, Campbell systematic review and meta-analysis of focused deterrence programs, Braga and Weisburd (2012: 341) reported that these interventions were associated with an overall effect size on crime outcomes that was generally between .47 and .61, which is consistent with a medium (or moderate) standardized effect size (Cohen, 1988).[1]

1. The studies described here employed at least a quasi-experimental (sometimes with case-control matching) design to rule out (where possible) extraneous influences on targeted crime outcomes (Braga and Weisburd, 2012). Sherman et al. (1998) documented a process by which evaluation rigor can

More recent advancements in the literature have begun to illustrate the mechanisms by which focused deterrence approaches potentially impact aggregate crime rates. Indeed if the strategy is true to form, then groups and gangs who receive a threat of enhanced sanctions combined with social service provisions should experience appreciable declines in offending post implementation (i.e., post call-in). When the Boston Ceasefire strategy was reinstated in the late 2000s after a period of discontinuation from the initial intervention (in the 1990s), research found that gangs called into notification sessions were significantly less likely to generate shootings and to be victimized by firearms when compared with highly comparable (matched) control gangs (Braga, Hureau, and Papachristos, 2014). Additionally, gangs in Boston that were associated with treated gangs (i.e., socially networked), but were not direct recipients of the call-in sessions, also experienced significantly fewer shootings than match-control gangs (Braga, Apel, and Welsh, 2013). A similar study in Chicago showed that gang factions who attended a call-in experienced a 23% reduction in overall shooting and a 32% reduction in firearm victimization in the year that followed the treatment (Papachristos and Kirk, 2015, this issue).

In terms of individual-level studies, a cross-national implementation of the strategy in Glasgow likewise showed evidence that gang-involved youths from treatment neighborhoods were significantly less violent than nontreated gang-involved youths from comparably economically distressed areas (Williams, Currie, Linden, and Donnelly, 2014). Within the United States, research (again from Chicago) showed that individuals who attended focused deterrence notification sessions had a lower recidivism rate, across multiple re-offense categories, when compared with offenders from the same neighborhoods who were not identified and selected to attend call-ins (Wallace et al., forthcoming). The literature has thus illustrated that where focused deterrence strategies are implemented with a high degree of fidelity, aggregate rates of violence often experience significant and sizeable declines; more recent studies have highlighted that the crime reduction benefits are driven by reductions in offending by the very groups and gangs as well as the individuals who receive the treatment (both direct and indirect recipients of the notification sessions). Based on this research foundation, problem analyses within New Orleans suggested that focused deterrence would be a viable intervention strategy because detailed problem analyses illustrated that a discrete number of groups and gangs was responsible for driving disproportionally high rates of lethal violence within the city.

be measured, which is referred to as the "scientific methods scale" (or SMS) that ranges from 1 to 5 (low to high). Studies with multiple units that receive or do not receive the program and attempt to control for other factors (i.e., a classic quasi-experimental design) or that use time-series methods are coded with a score of "3" in the SMS scale. More rigorous studies that use multiple cases and controls and include matching are coded as a "4" on the SMS scale, whereas a score of "5" is reserved for randomized controlled trials. Only evaluations that are graded with a score of "3" or higher are typically included in rigorous systematic reviews (see also Braga and Weisburd, 2014).

GVRS in New Orleans

Practitioners often insist that the unique local context of *their* city, political environment, violence problem, level of resources, neighborhoods, citizens, and so on would prevent the successful implementation of violence-reduction initiatives that have demonstrated success in other jurisdictions. New Orleans certainly presented such a challenge. The legitimacy of the city government of New Orleans, and the NOPD specifically, has previously been called into question (Moore, 2010). Unfortunately, prior allegations of corruption, misconduct, and abuse of force have continued to plague the city. For example, in July 2014, former New Orleans Mayor Ray Nagin was sentenced to 10 years in prison for various acts of bribery and corruption while in office. Likewise, based on a 10-month investigation by the Department of Justice (DOJ) and a subsequent scathing written report documenting unconstitutional conduct by the NOPD, in 2012 the City of New Orleans, the NOPD, and the DOJ entered into the "nation's most expansive Consent Decree" in an effort to force sweeping department-wide reform.[2] Despite recent reform efforts, the perceived legitimacy of the NOPD remains a challenge as scandals of police misconduct continue to be exposed (e.g., a damaging report released in November 2014 documented extensive failures in reporting and investigations conducted by the NOPD Special Victims Section). These scandals led to a continual questioning of the legitimacy of the NOPD (Tyler, 1990).

Despite such difficulties, between 2010 and 2012 specifically, government officials in New Orleans including the Mayor's Office, the NOPD, federal and local prosecutors, and federal law enforcement drew on promising strategies such as the GVRS and the Project Safe Neighborhoods (comprehensive gun violence-reduction approach—see McGarrell, Corsaro, Hipple, and Bynum, 2010) to build the interorganizational capacity necessary for strategic implementation. The city also supplemented the GVRS with the *CURE Violence* model (formerly *CeaseFire Chicago*) as part of the broader *NOLA for Life* murder reduction strategy (Skogan, Hartnett, Bump, and Dubois, 2009), which relied on violence interrupters and outreach workers to mediate conflicts between conflicting groups within the Central City area (City of New Orleans, 2013). Thus, the city relied on both a multiagency and a comprehensive problem-solving framework to address the persistent citywide patterns in violence.

As part of the GVRS problem identification phase (with a specific focus on the causes and correlates of lethal violence), law enforcement officials in New Orleans partnered with researchers to conduct a series of homicide incident reviews (the first beginning in June 2012) as well as gang audits to identify potential groups most prone to violence across the different police districts within the city. The incident reviews included NOPD officers; Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF) special agents; researchers from the Institute of Crime Science at the University of Cincinnati; and members of the

2. For more details, see nola.gov.

Cincinnati Police Department who had extensive experience with gang audits.[3] Information about violent street gangs was converted into actionable intelligence by organizing it along the following dimensions: (a) individual gang members, (b) geography, (c) social networks, and (d) participation in violence.

The working group identified 59 potential street gangs in six of the seven police districts within the city. Officials estimated that there were approximately 600–700 individual members within these gangs. Social network analyses indicated that a handful of gangs were diverse and at risk for violence via their social networks (such as active feuds or alliances with other gangs). Social network and geographic analyses supported officers' descriptions of the changing nature of groups and gangs in New Orleans. Officers described these groups as less likely to be hierarchical, intergenerational, structured gangs, and more likely to be loosely knit with continual changes in membership and affiliations. In addition, officers suggested that the structure and territorial nature of violent groups and gangs changed dramatically after Hurricane Katrina because of the displacement of low-income residents (see also Kirk, 2009) and the subsequent rebuilding phase, which separated group and gang members and disrupted traditional territorial boundaries (where applicable).

Loose gang network structures and corresponding definitional issues have previously served as a challenge within the City of New Orleans. For example, in a DOJ-sponsored review of homicides from 2009 to 2010, Wellford, Bond, and Goodison (2011) found that only 1.0% to 2.5% of all homicides in the city were officially classified as gang related. Wellford et al. (2011: 12) cited cases in which the gang units specified that small groups (three or four individuals) of unorganized young men often identified with geographic areas where they lived committed high levels of violence within the city; however, because they were not in "structured" and formalized "gangs" the NOPD at that time did not define such activity as gang related. In their conclusion, Wellford et al. suggested the use of homicide incident reviews would likely better unravel the network of loosely structured groups of offenders. In the homicide reviews conducted in preparation for the GVRS, the research team and NOPD officials placed a greater emphasis on identifying the loosely affiliated networks of offenders engaging in violence. Based on this more in-depth and comprehensive review, the current investigation identified 54.3% of all lethal incidents between January 1, 2010 through March 31, 2014 as group or gang member involved

3. The Cincinnati Initiative to Reduce Violence was a replication of the Boston Ceasefire strategy that began in 2007. Key officials from the enforcement team from the Cincinnati Police Department worked with the University of Cincinnati research team and the NOPD to explain their previous organizational experiences with the Cincinnati Initiative to Reduce Violence (see Engel, Baker, Tillyer, Eck, and Dunham, 2008), conduct several gang audits, and lead their homicide incident reviews. Additionally, David Kennedy and members from the National Network for Safe Communities played a key role by providing project oversight and guidance throughout the various stages of the strategy to the NOPD and City of New Orleans officials.

(GMI), indicating that the incident involved a group or gang member as a victim, suspect, or both.[4]

As part of the GVRS, officers within the NOPD suggested that the structure of groups and gangs in New Orleans was more fluid and less geographically based than in the past. Similar descriptions have been observed regarding the changing nature of group and gang affiliations in other cities as well (Engel et al., 2013; Kennedy, 2009b). Law enforcement officials most familiar with these groups and gangs provided detailed feedback to the criminal justice working group about gang participation in violence leading to a continually updated list of potential gangs to include in the call-ins. The identified (mostly loosely structured) gangs became the focus of a multipronged approach that included law enforcement, the threat of enhanced prosecution, and the use of social services. During the course of the strategy evaluated in this study, the NOPD conducted five separate offender notification sessions to deliver antiviolence messages to offenders associated with problematic gangs that were incarcerated or were on probation or parole (between October 2012 and March 2014). During these combined sessions, 158 individuals (representing 54 high-violence gangs) directly received communication that enhanced sanctions would follow any involvement in violence, and the notified offenders were asked to disseminate the message to other members. More specifically, the notified group and gang members were warned that the next murder or shooting committed by any individual associated with the notified gang would result in immediate and enhanced law enforcement scrutiny of the entire group for any criminal activity. Illustrations of previous gangs that were apprehended and facing rigid federal and state prison terms were shared to underscore the seriousness of the message.

In terms of organizational structure, a multiagency law enforcement task force (including local and federal partners) was created to track gang violence, review data sources and intelligence, and build criminal cases on violent gang members. Two newly created positions (i.e., program manager and criminal intelligence analyst) helped provide direction. In addition to the 158 individuals who attended the call-ins, six individuals were visited by police and received a personalized antiviolence message, which was referred to as a "custom notification" session (see Kennedy and Friedrich, 2014). Social service provisions were

4. GMI homicides were classified by the NOPD and researchers during the homicide incident reviews (see Azrael, Braga, and O'Brien, 2013). The name of the victim and suspect(s) (if known) as well as the totality of situational homicide characteristics were considered. Such characteristics included the following: location of the offense, suspected involvement of the victim in illicit acts preceding the homicide, manner and type of death, known characteristics of the victim, time of day, likely suspects, and other relevant characteristics of the incident. If the totality of the circumstances suggests that group members were involved in the incident, then it was coded as a GMI homicide unless evidence existed to the contrary. Each case was reviewed retrospectively for proper GMI determination when additional suspect information was gathered. The same team of NOPD officers and researchers was responsible for the final GMI classification of all homicides examined in these analyses; therefore, there are no concerns regarding coder inter-rater reliability.

presented to those individuals in attendance at the call-in sessions; 59 of the 158 individuals (37.3%) signed up for some type of social services, although only 25 of the 59 individuals actually participated in or received such services.[5]

To summarize, the process (or implementation) of the GVRS strategy in New Orleans is consistent with the overall model of the focused deterrence framework adopted in other cities (Braga and Weisburd, 2012; Kennedy, 2009a). Specifically, high-risk groups and gangs were identified through problem analyses, notified of future sanctions in call-in sessions, subjected to enhanced enforcement actions when antiviolence rules were broken, and provided access to social service opportunities.

Assessment of Impact: Two-Phase Analytical Approach

Assessing the potential impact of the New Orleans focused deterrence strategy presents a unique challenge because the intervention was implemented throughout all areas in the city that experienced high homicide and persistent gun-violence problems. Given the political and social climate that led to programmatic implementation, prioritization was placed on identifying and addressing citywide violence rather than on optimizing the evaluation design. We thus employ a two-phase methodological design. First, we compare the relative homicide rate change in New Orleans with cities that have been classified as having the most volatile and stable homicide rates (see McCall et al., 2011). Next, we examine changes in targeted violence within New Orleans by employing a standard interrupted time-series design that compares multiple targeted crime outcomes (i.e., homicides, gun homicides, gang homicides, and firearm assaults) in the postintervention period relative to preintervention trends after controlling consistent shocks and drifts in the longitudinal data to better isolate potential programmatic effects within the city (Cook and Campbell, 1979). We also include a comparative time-series analysis on nontargeted outcomes (i.e., overall violent crimes, property crimes, and nongang homicides) to assess whether the potential changes in targeted outcomes correspond with a more general trend in crime within the city. Finally, we include a series of sensitivity and placebo tests in both analytical phases and control for the potential influence of simultaneous strategies (i.e., *CURE Violence*) within New Orleans to rule out, where possible, the impact of confounding influences on the outcomes examined.

5. We note that the current study does not attempt to disentangle the various potential intervention mechanisms. Although the literature has framed the focused deterrence framework under the umbrella of “deterrence” (Kennedy, 2009a) as well “legitimacy” (Papachristos et al., 2007), it is possible that the increased use of social service provisions by high-risk offenders could increase levels of institutional engagement. It is important to consider that structural criminological research would suggest that enhanced institutional engagement could potentially lead to reductions in homicide (see McCall, Land, Dollar, and Parker, 2013). The Cincinnati GVRS evaluation did not find any significant relationship between service provisions and changes in city-level violence (Engel et al., 2013: 28). However, parsing out such mechanisms is both an analytical challenge and a vital next step in future evaluation research where interventions are guided by various theoretical frameworks.

Phase I: Homicide Rate Change in New Orleans Contrasted with Similar High-Trajectory Cities

Phase I is designed to assess whether New Orleans experienced a change in homicide above and beyond cities with highly comparable homicide rates. The City of New Orleans has maintained a persistently high rate of lethal violence since the 1970s (McCall et al., 2011). Prior research has demonstrated the benefits of using group-based trajectory analysis when attempting to achieve balance between cases and controls in observational (i.e., nonexperimental) settings (see Haviland and Nagin, 2005, 2007). Previous studies that have classified geographic units such as street segments, neighborhoods, and cities into different trajectory groups consistently illustrate that places with the highest *levels* of violence are also responsible for the largest peaks and valleys (i.e., *variability*) in violence, whereas moderate and lower trajectory classifications typically experience far fewer ebbs and flows in crime (Braga, Papachristos, and Hureau, 2010; Griffiths and Chavez, 2004; McCall et al., 2011; Weisburd, Bushway, Lum, and Yang, 2004).

As an initial step, we drew from work by McCall et al. (2011) that was based on a long-term trajectory analysis of homicide rates in U.S. cities from 1976 to 2005. McCall et al. identified 15 cities (including New Orleans) that had the most persistently high homicide rates, similar structural factors that predicted group classification, and vastly similar changes in homicide rates during a sustained period of time.[6] By restricting our comparison of changes in homicides in New Orleans with these previously validated high-trajectory homicide cities, we restricted our comparison to cities that are also more likely to experience similar shifts in their homicide rates over time. The homicide data examined in this analysis are for the years 2008 through 2013. We conducted a series of difference-in-difference Poisson regression models (with an offset exposure variable accounting for the annual population for each city—thus transforming the outcome into a homicide rate) based on Equation (1):

$$\begin{aligned}\log(\text{Homicides})_{it} &= \alpha + \text{I}(\text{NewOrleans})_{it}B_1 + \text{I}(\text{Treatment})_{it}B_2 + \text{I}(\text{NewOrleans})_{it} \\ &\quad \times \text{I}(\text{Treatment})_{it}B_3 + \log\big(\text{Population}\big)_{it} + \varepsilon_{it} \qquad (1)\end{aligned}$$

where $\log(\text{Homicides})_{it}$ denotes the homicide count for each city between 2008 and 2013 (which is transformed into a homicide rate via the natural logarithm with the inclusion of the population exposure variable on the right-hand side of the equation), $\text{I}(\text{New Orleans})_{it}$ is an indicator variable that equals 1 if the city is New Orleans and 0 otherwise, $\text{I}(\text{Treatment})_{it}$ is

6. The 15 cities that were classified as high-trajectory homicide rate cities (and thus were used as treatment and comparison cities in subsequent trend comparisons) were as follows: Atlanta, Georgia; Baltimore, Maryland; Birmingham, Alabama; Cleveland, Ohio; Dallas, Texas; Detroit, Michigan; Flint, Michigan; Gary, Indiana; Miami, Florida; New Orleans, Louisiana; Newark, New Jersey; Oakland, California; Richmond, Virginia; St. Louis, Missouri; and Washington, DC. All annual homicide data from 2008 to 2013 were obtained from the Federal Bureau of Investigation's Uniform Crime Reports (2013).

TABLE 1

Difference-in-Difference Model Estimates (New Orleans Compared with 14 High-Trajectory Homicide Cities)

	Model 1 Treatment = 2013		Model 2 Treatment = 2012		Model 3 Treatment = 2011	
Estimate	***b***	**(*SE*)**	***b***	**(*SE*)**	***b***	**(*SE*)**
Intercept	-8.276^{**}	(.011)	-8.290^{**}	(.012)	-8.287^{**}	(.014)
New Orleans	$.765^{**}$	(.034)	$.786^{**}$	(.039)	$.767^{**}$	(.045)
Treatment	–.012	(.027)	.035	(.021)	.018	(.020)
Difference-in-difference	$-.266^{**}$	(.090)	$-.191^{**}$	(.068)	–.088	(.064)

$^{**}p < .01$.

an indicator variable that equals 1 if the year is in the posttreatment period and 0 otherwise, and where $+\ \mathrm{I(New\ Orleans)}_{it} \times \mathrm{I(Treatment)}_{it}$ is the difference-in-difference estimator to examine the direct impact of the change in homicides in New Orleans compared with other highly chronic lethal violent U.S. cities.[7]

Model 1 in Table 1 illustrates that if the regression model corresponds with an intervention date that equals 2013 (i.e., the *first full year* that is in the true postintervention period), then the City of New Orleans experienced a statistically significant homicide rate decline above the average homicide rate change for the 14 highly comparable cities identified by McCall et al (2011). Specifically, the incident rate ratio is written as $e^{-0.266}$ or 0.766, which equates to –.23, or a 23% homicide rate decline that was specific and unique to New Orleans. However, true implementation began in late October 2012, and thus, we alter the intervention period to include both 2012 and 2013 as the postimplementation period (Model 2). Similarly, the results show a statistically significant decline unique to New Orleans that was 17.3% lower ($b = -.191$, standard error [SE] $= .068$) when compared with the homicide rate change in the 14 comparison sites. Although the magnitude of the effect via the point estimate is reduced (from –23% in Model 1 to –17% in Model 2), the analysis still shows a robust reduction in homicides that was unique for New Orleans in 2012–2013. Finally, as a sensitivity test, we model the postimplementation period in the analysis as 2011–2013 (Model 3), which thus includes a statistical implementation period one year longer than the true postimplementation period. The difference-in-difference estimate for

7. A key assumption of the difference-in-difference framework is the parallel trend assumption. Research by McCall et al. (2011) highlighted that New Orleans and the other 14 high-trajectory cities have a long criminological history with similar levels as well as comparable rates of homicide rate change. Additionally, graphical analyses illustrated two consistent trends. First, New Orleans typically had homicide rates that were on the high end of the distribution, and second, no noticeable differences were observed in the shifts in homicide rates between New Orleans, as most comparison sites during the preintervention period were examined in this study.

Model 3 was no longer statistically significant ($b = -.088$, $SE = .064$), which indicates that the relative homicide rate change in New Orleans was specific only for years 2012 and 2013 (i.e., the *true* postintervention period in New Orleans). In sum, the homicide rate in New Orleans experienced a decline that was unique when compared with other chronic, high-trajectory homicide cities.

Supplemental analyses to test potential regression to the mean. Although the previous analysis provides evidence that the homicide rate change was unique to New Orleans, the highly comparable control sites were based on a classification using city crime and structural data that ranged from 1976 to 2005 (McCall et al., 2011). To generate the closest possible city-level homicide rate trajectories, we next follow a comparative analysis procedure outlined by Haviland, Nagin, and Rosenbaum (2007: 250–251). Specifically, we modeled homicide rate data from 2009 to 2011 for all U.S. cities (>100,000 population as of the 2010 U.S. Census) since this 3-year period immediately preceded the GVRS implemented in New Orleans in late 2012. All group-based trajectory analyses (GBTAs) relied on the Proc Traj procedure in SAS version 9.1 (Jones, Nagin, and Roeder, 2001; SAS Institute Inc., Cary, NC). Latent growth curves were operationalized as annual homicide rates for the years 2009–2011 by a set number of trajectories. Following the model identification procedures outlined by Nagin (2005), linear models were found to be the most appropriate, which was anticipated because only three observational periods were used to estimate the trajectory models. The Bayesian information criterion (BIC) was also evaluated to select the appropriate number of trajectory groups. The BIC can be viewed as an approximate standardized model fit indicator because it penalizes when an increase in the number of trajectory groups (k) is estimated.[8] As shown in Table 2, most large U.S. cities were in the low homicide trajectory group ($n = 219$, 81.4%), which averaged four homicides per 100,000 from 2009 to 2011. A total of 43 cities (15.9%) were classified in the moderate trajectory group that had an average homicide rate of 16 per year. A small number of cities ($n = 7$, 2.7%) was classified in the high-trajectory group for the years 2009–2011 (i.e., the period that immediately preceded the 2012 New Orleans intervention), which averaged 39 homicides per 100,000. The cities in this trajectory include New Orleans along with Detroit, Baltimore, St. Louis, Newark, Flint, and Richmond (California).

We conducted a sensitivity difference-in-difference regression analysis to compare the shift in homicides in New Orleans with these six (immediate) high-trajectory cities. These

8. The BIC values clearly indicated the three-group solution was the most appropriate for city homicide rate trends. The three-group solution was identified as follows: BIC = −2341.48, where $N = 269$ (269 cities). In terms of model diagnostics, Nagin (2005) suggested that average posterior probabilities above .70 are acceptable, whereas values closest to 1.0 are ideal. The average posterior probability of group membership for each group was higher than 0.95 for each of the three groups identified. Nagin (2005: 89) also specifically indicated that the odds of a correction classification for each group should have an estimated value above 5.0 to achieve high assignment accuracy (i.e., low residual deviation). The lowest odds of a correction classification for any group were 18.93, which exceeds the acceptable diagnostic criteria.

TABLE 2

GBTA Model Estimates—Urban Homicide Rates (2009–2011)

Parameter	Low Group ($k = 1$)	Moderate Group ($k = 2$)	High Group ($k = 3$)
Group probability	0.989	0.959	1.000
Homicide rate 2009	4.58	16.53	39.45
Homicide rate 2010	4.30	16.41	37.13
Homicide rate 2011	4.30	16.31	40.25

TABLE 3

Difference-in-Difference Model Estimates (New Orleans Compared with Six High-Trajectory Homicide Cities)

	Model 1 Treatment = 2013		Model 2 Treatment = 2012		Model 3 Treatment = 2011	
Estimate	b	(SE)	b	(SE)	b	(SE)
Intercept	-7.888^{**}	(.041)	-7.915^{**}	(.036)	-7.929^{**}	(.031)
New Orleans	$.415^{**}$	(.082)	$.459^{**}$	(.096)	$.475^{**}$	(.127)
Treatment	.057	(.076)	.109	(.080)	.104	(.068)
Difference-in-difference	$-.375^{**}$	(.102)	$-.314^{**}$	(.149)	–.241	(.162)

$^{**}p < .01$.

six control sites were most comparable with New Orleans in terms of homicide rate levels for the 3-year period that immediately preceded the implementation of the intervention in 2012. The difference-in-difference findings displayed in Table 3 mirror those presented previously; specifically, New Orleans experienced a statistically significant 31.2% homicide rate divergence ($b = -.375$, $SE = .102$) when the intervention date in the model is set to year 2013 (Model 1). Likewise, Model 2 shows that New Orleans had a 26.9% statistically significant reduction ($b = -.314$, $SE = .149$) in homicides in the years 2012–2013 when compared with the control cities. Finally, the difference-in-difference estimate was not statistically significant in Model 3 ($b = -.241$, $SE = .162$), which indicates that the homicide rate departure in New Orleans did not occur in 2011—or before the intervention was actually implemented.

The combined evidence from both sets of models shows the homicide rate decline in New Orleans was unique in two important respects. First, the relative homicide rate change between the preintervention and postintervention periods in New Orleans was distinctive when compared with cities that followed similar homicide rate trajectories. Second, the multiple models used in this analysis illustrate the relative homicide rate change was greatest for New Orleans in 2013 (the first full year in the postintervention period), was marginally lower for New Orleans in 2012–2013 (the intervention was implemented in late 2012), and did not seem to exist in 2011 (the year prior to the New Orleans intervention). These combined results suggest that New Orleans experienced a distinctive change in homicides that corresponded with the implementation of its citywide GVRS strategy.

Phase II: Time-Series Analyses on Homicides and Firearm Assaults within New Orleans

To assess the programmatic impact *within* New Orleans more fully, we move to an interrupted time-series analysis that accounts for unique changes in additional types of targeted crime outcomes. The interrupted time-series design is appropriate when over-time data are analyzed to assess the degree to which a treatment shifts the trajectory of a single case over time (McCleary and Hay, 1980). Although any analytical design has limitations, time-series modeling can be enhanced in many ways. In the context of the current study, Morgan and Winship's (2007: 245–252) review of the original Boston Ceasefire DOJ report that relied on time-series analysis (see Braga et al., 2001) found the Boston evaluation was of "high quality" largely because of the use of supplemental analyses, which included the examination of multiple outcomes that were hypothesized to be influenced by the citywide intervention. Morgan and Winship (2007) specifically noted that the use of multiple within-city outcomes improved the rigor of the time-series design, which can bolster the case for causal assertion.

Research has suggested that the underlying dynamics that generate homicides, gun homicides, and gun assaults are powerfully influenced by gang and group conflicts (e.g., Braga et al., 2014), which are the same dynamics targeted by the GVRS. Therefore, we examined four key citywide monthly outcome variables as a way to triangulate possible program impacts:

(1) Overall homicides[9]
(2) Firearm-related homicides
(3) Firearm assaults[10]
(4) GMI homicides

The various violent crime incident data examined here were provided by the NOPD Crime Analysis Unit.[11]

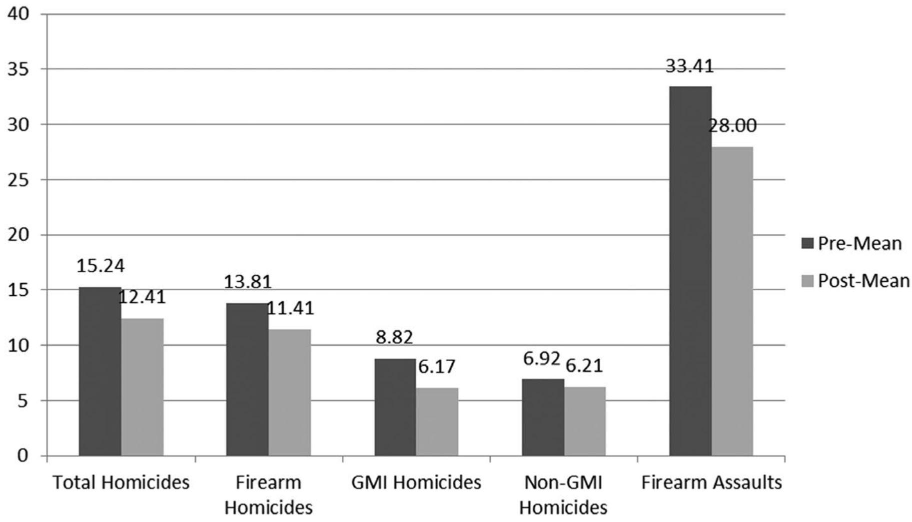
9. Homicides excluded police intervention shootings and those classified as self-defense or justifiable.

10. A change in the data reporting structure by the NOPD did not allow for an analysis of earlier shooting data prior to 2010; likewise, given the detailed nature of the GMI incident reviews, the research team was not confident that retrospective classifications of homicide incidents that occurred prior to 2010 would yield the same level of measurement validity as those examined between January 1, 2010 and March 31, 2014.

11. Officially reported crime incident data are not without limitations, such as citizen reporting biases (Warner and Pierce, 1993) and officer decisions to alter crime classifications and actions taken on

FIGURE 1

Mean Monthly Difference in Targeted Crime Outcomes Between Preintervention and Postintervention Periods



We use November 2012 as the period of implementation onset in our monthly preintervention and postintervention time-series comparisons given that the first pulling levers call-in meeting occurred on October, 25, 2012, when approximately 40 probationers and other high-risk prior offenders affiliated with gang networks were called into a session in an Orleans Parish courtroom. Prior evaluations of focused deterrence indicate that the strategy can have a "light-switch" (i.e., immediate and sustained) impact on homicide and gang-related offenses (Kennedy, 2006). Therefore, the analyses presented in this article are modeled to examine immediate and sustained changes in violent crime.[12]

Figure 1 shows the unconditional bivariate change in the various types of outcomes that the focused deterrence strategy was intended to impact (as well as the non-GMI homicides, which we use as a point of comparison). Specifically, we find that after implementation, the mean monthly count of total homicides decreased from 15.2 pretest monthly incidents to 12.4 posttest monthly incidents or by 18.6%. A similar percentage reduction (17.4%) in firearm-related incidents was observed during this same period—from 13.8 pretest mean

incidents reported to police (Black, 1970). Although it is important to consider the appropriate limitations of official incident level data, police incident reports have been widely used to assess trends and patterns of lethal gun violence (Blumstein and Rosenfeld, 1998) and previous focused deterrence initiatives (Braga and Weisburd, 2012).

12. Alternative specifications are examined in the sensitivity analyses.

monthly offenses to 11.4 posttest monthly incidents. The largest and most substantive reduction was specifically observed for GMI homicides; a 30.1% decline was detected (a change from 8.8 GMI homicides per month to 6.2 in the postintervention period). Comparatively, non-GMI incidents also declined but the reduction was much less pronounced (–10.3%) between the preintervention and postintervention periods (6.9 average monthly incidents to 6.2 postmonthly incidents). Finally, firearm assaults experienced a sizeable reduction from 33.4 per month to 28.0 per month (–16.2%).

Figure 2 displays the monthly counts of homicides (all types) and firearm assaults, which were distributed as event counts. These events do not approximate a normal, continuous distribution (King, 1988). Because crime rates are discrete events that can suffer from low counts during a specified time period, there are a variety of problems when analyzing such data by using ordinary least-squares regression estimation (Osgood, 2000). Most importantly, normal or symmetrical error distributions cannot be expected when crime counts are small because the error distribution becomes rapidly skewed. A similar assumption is required when using Autoregressive Integrated Moving Average time-series analysis (Box and Jenkins, 1976).

The conventional analytical approach in criminology for analyzing event counts, and in particular crime events, has been to rely on Poisson regression via maximum-likelihood estimation (Long, 1997; Osgood, 2000). The Poisson distribution as it relates to this analysis is expressed as follows:

$$P\left(Y_i = y_i | x_i\right) = \frac{\exp\left(-\lambda\right) \lambda^{y_i}}{y_i!} \tag{2}$$

where Y_i is a random variable representing a violent crime count (i.e., homicide event or firearm assaults in this sample), y_i is a count value that denotes the number of monthly events observed for a discrete time period, and λ_i represents different values in violent crime counts at distinct points (i.e., months) in time. To predict λ_i, we relied on the loglinear function of the following model:

$$ln\left(\lambda_i\right) = x_i^T \beta \tag{3}$$

where $x_i^T\ \beta$ is a linear combination of predictors for each case (i). When estimating the interrupted time-series models, this combination of measures included a postintervention variable (1 = November 2012 onward), trend measures that account for a general decline (i.e., fluctuations) in the time series, and monthly dummy variables to control for consistent seasonal shifts in the data.

It is important to note the conditional Poisson process assumes equidispersion between the expected mean and variance for the outcome variables modeled (Long, 1997). We reestimated each Poisson regression model by relying on the conditional negative binomial distribution because the overdispersion in the models that were estimated could lead to

FIGURE 2

Monthly Count of Targeted Crime Outcomes (Total Homicides, Firearm Homicides, GMI Homicides, and Firearm Assaults) Preintervention and Postintervention

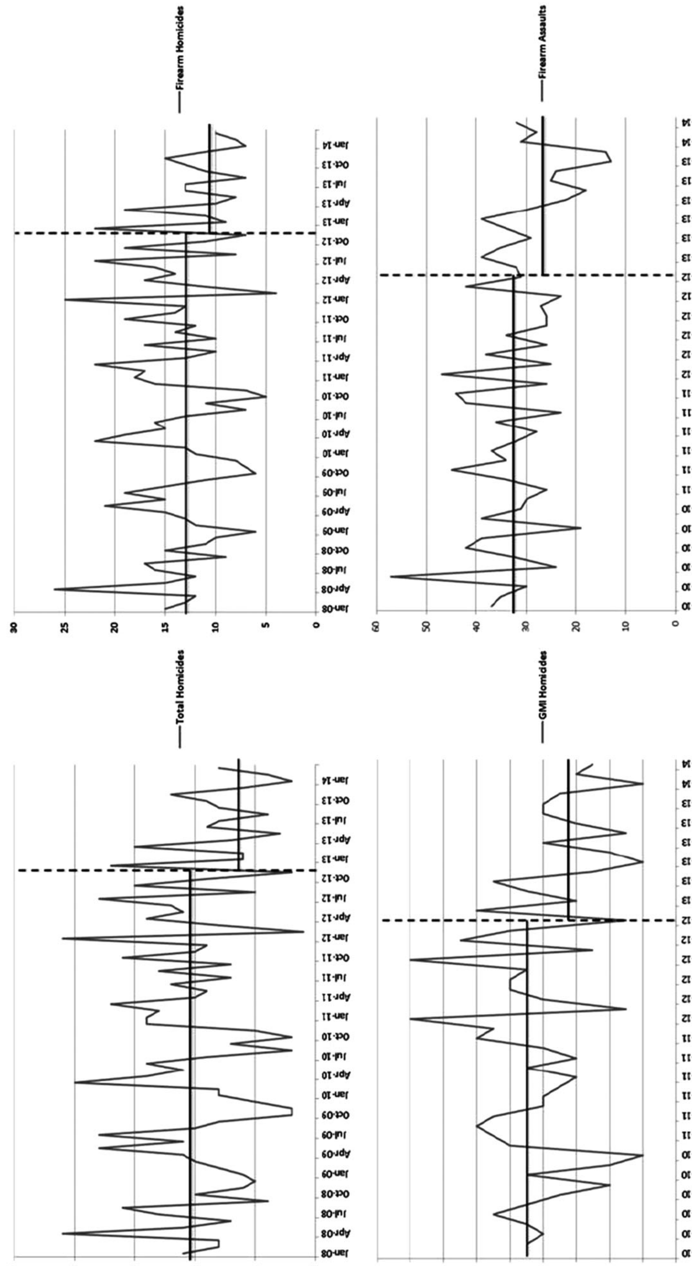


TABLE 4

Annual Changes in Homicides Relative to Changes in Overall Violent and Property Offenses (2006–2012)

Offense Type	2006	2007	2008	2009	2010	2011	2012*
Homicide	162	209	179	174	175	200	161
Percentage change	—	+29.01	−14.35	−2.79	+0.57	+14.28	2.42
Overall Violent Crime	2,225	3,451	2,869	2,614	2,593	2,748	2,240
Percentage change	—	+53.03	−16.68	−8.08	-0.83	+6.00	−0.04
Overall Property Crime	12,178	15,583	14,880	12,940	12,645	14,013	11,431
Percentage change	—	+27.96	−4.51	−13.04	−2.28	+10.81	−1.06

*The 2012 total only includes incidents between January 1, 2012 and December 31, 2012. The percentage change for 2012 is also relative to the total number of incidents between January 1, 2011 and October 31, 2011. In 2011, 165 homicides, 2,241 overall violent crimes, and 11,553 total property crimes occurred between January 1 and October 31.

biased statistical inferences (Hilbe, 2007; Osgood, 2000). In each outcome examined, we used the Kolmogorov–Smirnov goodness-of-fit test and found that none of the distributions examined were overdispersed. Thus, we display the results from the conventional Poisson regressions with monthly fixed-effects parameters as a way to control for omitted static influences on specified outcomes that were not included in our models (Allison and Waterman, 2002). We also include the Huber–White robust sandwich estimator to ensure the coefficient variances were robust to violations of homoscedastic error distributions. STATA 12.0 SE software (StataCorp, College Station, TX) was used in all analyses presented herein. However, as with any interrupted time-series estimation, we note that statistical inferences in the subsequent analyses should be tempered because of the limitations of the design (see McDowall, McCleary, Meidinger, and Hay, 1980: 7).

It is first necessary to assess whether potential changes in targeted crime outcomes, such as homicides, occurred simply as a result of a broader fluctuation in crime within New Orleans during the period of examination. Table 4 displays annual Uniform Crime Reports crime counts from 2006 to 2012, which serves as a 7-year preintervention window that illustrates the relative stability between annual changes in homicides with changes in overall violent crime (homicides, rapes, robberies, and assaults combined) and changes in property crime (burglaries, larcenies, and motor vehicle thefts). When examining year-by-year variations, the annual changes in homicides followed extremely similar patterns to changes in overall violent crime and property crime. The shifts (increases or decreases) were virtually identical among homicides, overall violent crimes, and property crimes in 6 of the 7 years from 2006 to 2012. In the year where there was a minor degree of relative variation (2010), crimes of all types were stable relative to the earlier baseline year (2009), with a difference less than two percentage points between each

TABLE 5

Poisson Regression Results and Percentage Change Estimates on Targeted Outcomes (January 1, 2008 to March 31, 2014)

	Total Homicides		Overall Violence		Overall Property	
Variable	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	−.191* (.085)	−17.38%	.128** (.035)	13.65%	.085** (.021)	8.87%
February	−.215 (.169)	19.50%	−.045 (.102)	−4.40%	−.151* (.061)	−14.01%
March	.161 (.168)	−16.99%	.086 (.090)	8.98%	−.020 (.041)	−1.98%
April	.170 (.166)	−18.29%	.075 (.067)	7.88%	−.020 (.040)	−1.98%
May	.030 (.166)	−2.63%	.100 (.079)	10.51%	.125* (.049)	13.31%
June	.051 (.141)	−4.70%	.023 (.066)	2.32%	.070 (.045)	7.25%
July	.132 (.164)	−13.31%	.085 (.077)	8.87%	.136* (.031)	14.65%
August	−.120 (.203)	12.10%	−.007 (.069)	−0.69%	.096* (.032)	10.08%
September	−.145 (.165)	13.32%	−.055 (.086)	−5.35%	.010 (.046)	1.00%
October	−.158 (.207)	14.61%	.002 (.072)	0.20%	.016 (.034)	1.61%
November	−.267 (.193)	23.73%	−.065 (.077)	−6.29%	−.051 (.040)	−4.97%
December	−.017 (.199)	2.73%	−.041 (.067)	−4.01%	.027 (.043)	2.73%
Intercept	2.69** (.152)	—	5.41** (.063)	—	6.48** (.049)	—

$^{*}p < .05.$ $^{**}p < .01.$

outcome examined. In short, from 2006 through almost all of 2012, homicides, overall violent crimes, and property crimes followed extremely similar patterns of change in New Orleans.

Multivariate Results

Moving to the multivariate framework, Table 5 presents the results of the interrupted time-series analyses that examine the impact of the GVRS on homicides while controlling for other important covariates (i.e., seasonal monthly shocks). The targeted gang and criminally active group members that were called into notification sessions were identified based on their participation in gun violence; thus, it was anticipated that the strategy would have the greatest impact on lethal violence. Comparative estimates are also provided for changes in overall violent crimes and property crimes, which were beyond the specific scope of the focused deterrence strategy. Unstandardized coefficients and standard errors are presented within the table, along with the estimated percentage change in the monthly incidents, which is expressed as incident rate ratios (IRRs). The IRRs are exponentiated coefficients given the use of the logarithmic transformation in the Poisson regression models and percentage changes ([IRR – 1] × 100) in the intervention estimates.

The findings presented in Table 5 indicate that the GVRS was associated with a statistically significant 17.4% decline ($b = -.191$, $SE = .085$) in the mean monthly number of homicides in the postintervention period relative to the preintervention period. Additionally, none of the monthly dummy variables were statistically significant, which suggests relative seasonal stability in homicide incidents in New Orleans. Comparatively, both overall violence and overall property crimes experienced statistically significant increases during the same period that homicides experienced a significant decline. A significant 13.6% increase in violent crimes and an 8.8% increase in property crimes occurred after November 2012. These findings, combined with the earlier year-by-year trends (displayed in Table 4), suggest that the New Orleans strategy had a crime prevention impact that was unique to homicides that occurred beyond chance, whereas overall Part I crimes significantly increased during the postintervention period.

Table 6 provides more precise details on the types of homicides and firearm-related outcomes that were impacted by the GVRS. Several prior focused deterrence evaluations showcase a consistent effect: The greatest and most sizeable decreases in homicides are those that are group and gang related, which is expected because the most chronically violent gangs are the specific target of the multiagency task force (see Braga et al., 2008; Corsaro and McGarrell, 2009; Engel et al., 2013). In New Orleans, GMI homicides significantly reduced by 32.1% in the postintervention period ($b = -.387$, $SE = .116$)—net of seasonal controls. Comparatively, non-GMI homicide incidents (i.e., lethally violent incidents that were considerably more likely to be domestically related and involve nonchronic offenders) did not experience a statically significant mean difference. Specifically, an 8.9% nonsignificant decline occurred in non-GMI homicides ($b = -.094$, $SE = .093$). When compared with a significant reduction in GMI homicides by 32.0% in the postintervention period, these results highlight that the driving force behind the overall homicide decline was the specific reduction in lethal violence that was group or gang involved.

Table 6 also illustrates that both lethal and nonlethal firearm-related incidents experienced similar changes that corresponded with the New Orleans GVRS. The mean number of monthly firearm-related homicides significantly declined by 16.3% ($b = -.178$, $SE = .092$). Likewise, the mean monthly number of nonlethal firearm assaults significantly

TABLE 6

Poisson Regression Results and Percentage Change Estimates on Targeted and Comparison Outcomes

	GMI Homicides[a]		Non-GMI Homicides[a]		Firearm Homicides[a]		Firearm Assaults[b]	
Variable	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	$-.387^{**}$ (.116)	−32.09%	−.094 (.093)	−8.97%	$-.178^{*}$ (.092)	−16.30%	$-.177^{**}$ (.067)	−16.22%
February	−.120 (.260)	−11.30%	$-.575^{**}$ (.216)	−43.72%	−.165 (.210)	−15.21%	−.136 (.117)	−12.71%
March	−.023 (.220)	−2.27%	$.446^{*}$ (.230)	56.20%	.169 (.205)	18.14%	−.033 (.112)	−3.24%
April	−.149 (.187)	−13.84%	.239 (.167)	26.99%	.217 (.203)	24.23%	.021 (.177)	2.12%
May	−.180 (.240)	−16.47%	−.121 (.243)	−11.39%	.031 (.201)	3.14%	−.097 (.146)	−9.24%
June	−.247 (.239)	−21.88%	−.121 (.216)	−11.39%	.100 (.175)	10.51%	−.207 (.110)	−18.69%
July	−.003 (.278)	−0.29%	$.381^{**}$ (.162)	46.37%	.144 (.197)	15.48%	−.224 (.146)	−20.06%
August	$-.570^{*}$ (.257)	−43.44%	−.037 (.160)	−3.63%	−.168 (.219)	−15.46%	−.207 (.145)	−18.69%
September	−.088 (.231)	−8.42%	−.121 (.243)	−11.39%	−.097 (.199)	9.24%	$-.495^{**}$ (.124)	−39.04%
October	−.213 (.249)	−19.18%	.001 (.335)	0.10%	−.153 (.247)	14.18%	.005 (.117)	0.50%
November	−.304 (.250)	−26.21%	−.055 (.265)	−5.35%	−.248 (.222)	−21.96%	−.173 (.183)	−15.88%
December	.165 (.218)	17.93%	.024 (.282)	2.43%	.022 (.231)	2.24%	$-.327^{*}$ (.157)	−27.39%
Intercept	2.31^{**} (.154)		1.89 (.159)		2.62^{**} (.173)		3.65^{**} (.099)	

[a] Time series: January 1, 2008 to March 31, 2014.
[b] Time series: January 1, 2010 to March 31, 2014.
$^{*}p < .05$. $^{**}p < .01$.

declined by 16.2% ($b = -.177$, $SE = .067$). Firearm assaults seemed to have more seasonal fluctuations (particularly during the late summer and fall months) than all other targeted offenses examined, as evidenced by the significance levels of the monthly dummy variables.

In an effort to assess whether changes in homicides were observed for the most “at-risk” groups for homicide victimization and to minimize concerns related to gang “definitional issues” (see Maxson, 1999), a series of time-series analyses is presented and distinguished by victim age and race demographics (given that such demographic classifications are not

TABLE 7

Poisson Regression Results and Percentage Change Estimates on Race and Age-Specific Homicides (January 1, 2010 to March 31, 2014)

	Black Male Victims 20–29 Years Old		Black Male Victims 30+ Years Old		All Other Victims 20–29 Years Old		All Other Victims 30+ Years Old	
Variable	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	−.311** (.104)	−26.72%	.100 (.129)	10.51%	−.012 (.337)	−1.19%	−.388 (.290)	−32.15%
February	−.234 (.243)	−20.86%	.213 (.360)	23.73%	−2.079* (.990)	−87.49%	-1.466* (.503)	−76.91%
March	−.097 (.219)	−9.24%	.593 (.342)	60.94%	−.287 (.549)	−24.94%	−.262 (.353)	−23.04%
April	−.030 (.277)	−2.95%	.380 (.348)	46.22%	.558 (.419)	74.71%	−.842* (.385)	−56.91%
May	−.085 (.212)	−8.15%	.213 (.381)	23.78%	−.317 (.463)	−27.16%	.111 (.326)	11.73%
June	−.030 (.238)	−2.95%	.300 (.360)	34.95%	−.135 (.395)	−12.63%	−.506 (.435)	−39.70%
July	.072 (.210)	7.46%	.618 (.375)	65.52%	−.540 (.567)	−41.73%	−.255 (.421)	−22.50%
August	−.207 (.299)	−18.69%	.257 (.376)	29.30%	−1.233 (.661)	−70.85%	−.373 (.362)	−31.13%
September	−.273 (.272)	−23.89%	.117 (.424)	12.41%	−.828 (.521)	−56.30%	−.506 (.356)	−39.71%
October	−.239 (.282)	−21.25%	.166 (.377)	18.05%	−1.926* (.977)	−85.42%	−.054 (.454)	−5.26%
November	−.225 (.252)	−20.15%	.100 (.384)	10.51%	−1.924 (.977)	−85.39%	−.784 (.487)	−54.34%
December	−.159 (.253)	−14.70%	.240 (.429)	27.12%	−.272 (.534)	−23.81%	−.431 (.411)	−35.01%
Intercept	1.89** (.188)		1.06* (.341)		.136 (.317)		.715 (.224)	

$^{*}p < .05$. $^{**}p < .01$.

subject to misclassification). Table 7 shows that homicides involving Black male victims between the ages of 20 and 29 years old experienced a statistically significant decline of 26.7% ($b = -.311$, $SE = .104$). This observed intervention effect was found only for victims with this race and age classification. No significant decline occurred in homicides that involved older Black male victims (30 years old or older), and there were no significant changes in homicides that included pooled White and Hispanic male and female victims, or Black female victims (i.e., all other victims) between 20 and 29, as well as 30 years old or older. Thus, the lone observed intervention effect among New Orleans homicides was for Black males between the ages of 20 and 29 years old.

Taken together, the analyses presented in this section consistently indicate that homicides experienced significant declines and that overall violence and overall property crimes experienced increases. Comparing trends among specific types of homicides, the observed significant reductions were specific only to GMI and firearm homicides, whereas non-GMI homicides (i.e., those that did not involve group and gang members) remained relatively stable during the period examined here. Finally, homicides that involved Black males between the ages of 20 and 29 years old were the homicides that experienced significant changes. These results indicate that targeted violent crime incidents had observed significant declines, whereas there was no evidence of a general reduction in overall crime or lethal violence that did not involve group or gang members.

Sensitivity and supplemental time-series analyses. A series of sensitivity tests was examined to determine whether the estimated intervention point estimates were robust against rival explanations.[13] We conducted several placebo and sensitivity tests related to the intervention date. The placebo tests were designed to assess the relationship between the timing of the intervention estimate and the change in the targeted outcomes. We randomly selected four different placebo intervention dates for the preintervention time series available across each targeted outcome. As shown in Table 8, no placebo test resulted in a statistically significant intervention point estimate among the various homicide models (i.e., total homicides, GMI homicides, and firearms homicides), which indicates the observed significant intervention effect on the various types of homicides was found only when the *modeled postintervention period* paralleled the *true postintervention period* in New Orleans. This would suggest that the observed intervention effect on homicides occurred immediately after the initial notification session in New Orleans, which took place in late October 2012. In terms of firearm assaults, the placebo postintervention model (i.e., placebo model 4, or a postintervention period of January 2012 onward) was statistically significant

13. We reestimated each model using negative binomial regression (to control for overdispersion), and the results mirrored those that were presented in this study in all meaningful ways. We also included a series of additional control variables such as linear and curvilinear trend measures, as well as annual dummy variables to absorb random annual fluctuations in violence. None of the linear, curvilinear, or use of dummy estimates changed the intervention estimates in any substantive way. We likewise examined each model for potential autoregressive processes because time-series data are likely to be influenced by closest proximity empirical values (McCleary and Hay, 1980). We did not find autoregressive processes to be an issue of concern based on the following results: First, we conducted Autoregressive Integrated Moving Average analyses on the preintervention time series, where we detected no statistically significant first- or second-order autoregressive parameters in the model identification stage. Second, we estimated first-order autoregressive parameters into each model that is presented in this article using the ARPOIS package available in STATA (Tobias and Campbell, 1998), where the results did not diverge substantively from those presented herein.

TABLE 8

Preintervention Time-Series Intervention Placebo Tests on Targeted Crime Outcomes (Total Homicides, GMI Homicides, Firearm Homicides, and Total Firearm Assaults)

	Total Homicides[a]		Firearm Homicides[a]		GMI Homicides[b]		Firearm Assaults[b]	
Variable	Coefficient (St. Error)	*p* Value	Coefficient (St. Error)	*p* Value	Coefficient (St. Error)	*p* Value	Coefficient (St. Error)	*p* Value
Placebo Model 1	−.092 (.074)	.695	−.047 (.081)	.559	−.039 (.144)	.787	.002 (.081)	.976
Placebo Model 2	.012 (.072)	.859	.002 (.078)	.976	−.030 (.110)	.780	−.093 (.113)	.196
Placebo Model 3	−.103 (.074)	.167	−.101 (.080)	.210	−.057 (.109)	.599	−.124 (.067)	.088
Placebo Model 4	−.095 (.078)	.219	−.086 (.084)	.306	−.173 (.119)	.144	−.195 (.058)	.001

Note. Monthly dummy variables used to capture systematic seasonal fluctuations are estimated in each placebo model (4 models × 4 outcomes = 16 in total).
[a]Time series: January 1, 2008 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = July 2009, 2 = February 2010, 3 = March 2012, 4 = May 2012).
[b]Time series: January 1, 2010 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = March 2010, 2 = February 2011, 3 = August 2011, 4 = January 2012).

(IRR = .822), which indicates that the reduction in nonlethal firearms violence occurred in midyear 2013.[14,15]

14. There are two potential interpretations of this finding for firearm assaults. One possibility is that firearm assaults oscillated in an unsystematic way between January 2012 and March 2014 and that the intervention estimate in the time-series models (despite the use of relevant and suitable control variables) is influenced by such nonsystematic fluctuations. The second possibility, which seems more plausible given the visual display of the data in Figure 2, is that the statistically significant reduction in the average number of monthly firearm assaults was not precisely associated with the specific timing of the intervention onset in the time-series models. In this instance, the movement of the postintervention period to different dates does not influence the mean reduction in firearm assaults because the observed decline was less consistent with the timing of the true postintervention period; rather, it was influenced by the large shift in the magnitude of firearm assaults at some point in the true postintervention period. Figure 2 shows that beginning in early summer 2013, firearm assaults declined from reasonably stable 30–40 incidents per month to 15–25 incidents per month. In short, the timing of the intervention in the model becomes less important because of the very large decrease in firearm assaults that occurred in 2013.
15. As a second set of sensitivity tests, we followed procedures used by Cook and MacDonald (2011) and Piehl et al. (2003) to assess whether model fit parameters and the intervention point estimate coefficients had the largest estimated effect sizes (and optimal model fit indices) at the time of the intervention, or in the postintervention period (i.e., potential lagged effects) relative to potential preintervention effects (i.e., lead effects). The results presented in Appendix A illustrate that for total homicides, GMI homicides, and firearm homicides, the Wald chi-square statistic was smallest at the

Finally, it is important to isolate unique programmatic effects where possible. Although the combination of GVRS (focused deterrence) and street workers for conflict mediation (*CURE Violence,* which was implemented in Central City) has been shown to impact crime in previous settings (Engel et al., 2013; see also Webster, Whitehill, Vernick, and Curriero, 2013), we specifically partitioned the estimated impacts of GVRS and *CURE Violence* within Central City as well as the remainder of the city (i.e., excluding Central City).[16] The results are displayed in Appendix B and show that the overall city (minus Central City) experienced statistically significant declines in both total homicides (–17.5%, $p < .05$) and GMI homicides (–32.9%, $p < .05$). Comparatively, although both total homicides and GMI homicides were also reduced in Central City, this specific area of New Orleans did not have statistically significant declines that corresponded with November 2012 onset date. Thus, as will be discussed in more detail, there was no indication that the observed reduction in homicides that corresponded with the citywide focused GVRS was driven by the supplemental *CURE Violence* strategy within Central City.

Discussion

The current study illustrates that focused deterrence strategies can have a significant impact even in the most challenging of contexts, which in the City of New Orleans included extremely high murder rates, political and police corruption, and a local culture seemingly more tolerant of violence. Furthermore, overall homicides in New Orleans significantly declined between 17% and 31% when compared with similar high-trajectory homicide cities. More refined interrupted time-series analyses within New Orleans show that significant reductions in violence were observed specifically for overall homicides (–17%), GMI homicides (–32%), homicides that involved young Black male victims (–26%), and both lethal and nonlethal firearms violence (–16%). A series of sensitivity tests and supplemental analyses provide more support that these observed intervention effects were robust, were unlikely to have been caused by extraneous circumstances (e.g., a general overall crime shift), and were consistent with the timing of the GVRS.

This study has two noteworthy limitations. First, the evaluation design was of secondary consideration to the implementation of the strategy given the widespread and persistent diffusion of violence within New Orleans. In an effort to overcome this limitation, we relied

point of the true intervention (November 2011). There seemed to be a 2-month lagged effect (lag – 2) for firearm assaults. Thus, there is no evidence the intervention estimates were influenced in any discernible way by preexisting (preintervention) declines.

16. The City of Baltimore implemented a conflict mediation-based program (*Safe Streets*) within the McElderry Park community. However, police districts within Baltimore also conducted offender notification sessions, which were shown to have a significant and direct relationship with the observed reduction in homicides and nonfatal shootings associated with *Safe Streets* (see Webster et al., 2013: 36). Webster et al. controlled for this relationship when examining the impact of the conflict mediation strategy within McElderry Park.

on a series of analytical models to assess (and isolate) program impact; this methodology is of moderate strength and would have been enhanced considerably with a stronger quasi-experimental or pure experimental design, such as the use of a place-based focus at the onset to use a matching or randomization process (see Braga and Weisburd, 2014). However, prior criminological research has illustrated that geographic locations that have the highest levels of crime are also more likely to experience greater variability in crime. By modeling the change in homicides in New Orleans against a set of approximately matched control cities, it was apparent the decline was significantly sharper for New Orleans during the period examined here than all other sites. Second, the current study does not allow us to disentangle which of the observed aggregate crime effects are associated with deterrence (Braga and Weisburd, 2012), incapacitation effects (Levitt, 1998), changes in perceived legitimacy (Papachristos et al., 2007), or enhancements in social services—which can lead to greater institutional engagement (McCall et al., 2013). More precise measures (particularly at the individual and group levels) that capture these regulatory theoretical principles through surveys and narratives with notified offenders and groups would enhance the literature considerably.

Limitations notwithstanding, this study contributes to the literature in several important ways. Our findings illustrate that it might be possible to alter the mindset of gang and criminally active group members in settings where retaliatory violence has been a common occurrence. As found in a growing body of literature, *gang and groups* and *individuals* who receive the focused deterrence message participate in fewer documented cases of gun violence (as both victims and offenders) and less overall crime (across multiple outcomes) after call-in sessions relative to highly comparable gangs and groups as well as individuals who do not receive the deterrent-based message (Braga et al., 2013, 2014; Papachristos and Kirk, 2015; Wallace et al., forthcoming). In short, it might be possible to alter in a tangible way persistent cultures of violence.

The findings presented in this study also suggest that outreach workers used to interrupt violence in one specific neighborhood had no independent effect that empirically corresponded with the reduction in violence in New Orleans. Such findings are similar with prior focused deterrence evaluations that have attempted to describe and examine the observable impact of street workers on violence (see Engel et al., 2013; Tillyer, Engel, and Lovins, 2012). Although not definitive, the results in this study add to the growing body of literature questioning the effectiveness of the use of street outreach workers rooted in the public-health model that are not connected to a larger criminal-justice–based violence-reduction initiative. As noted by Papachristos (2011), although the initial *Chicago CeaseFire* strategy showed mostly promising results, replications have shown less support for conflict mediators to combat serious lethal violence in alternative settings, such as in Pittsburgh (see Wilson and Chermak, 2011). Where conflict mediation has been implemented in settings that have shown changes in crime, it has corresponded with offender notification strategies that have been implemented in nearby geographic areas (e.g., *Baltimore Safe Streets*—see Webster et al., 2013) or occurred simultaneously with mediation strategies (e.g., Cincinnati—see Engel et al., 2013, and now New Orleans). Thus, the literature has suggested that conflict mediation might complement focused deterrence, or its influence could be conditional in unique social settings, but there is little evidence to indicate that conflict mediation can function reliably as a stand-alone strategy to reduce violence. Future replications and evaluations are needed in this area.

Finally, it should be recognized that policy transfer was greatly enhanced by the persistence and commitment to the strategy by political and police officials in New Orleans. The research team, staff, and officers associated with GVRS received clear direction and unwavering political and police support from the highest levels of government, including Mayor Landrieu and NOPD Superintendent Serpas. This provided a clear mandate for the City of New Orleans, empowered those associated with the GVRS team, and ultimately led to successful implementation and reduced violence. However, members from the New Orleans GVRS working group (and future potential replicating agencies) would be well advised to understand the potential toward deterioration of treatment in strategies that approach their second, third, and fourth years (where applicable). Often, treatment deterioration is driven by staffing changes, changes in the characteristics of the target population, some decline in enthusiasm among working group officials, or other systematic factors (Kennedy, 2011; see also Land, McCall, and Williams, 1992). As noted by Brunson et al. (2013), the deepest collaborative relationships in focused deterrence working groups occur most often between individuals and not institutions, and thus staffing changes can undermine the collaborative capacity necessary for sustainability. Thoughtful efforts to rejuvenate the focused deterrence model among the key providers are critical to long-term sustainability (Tillyer et al., 2012). Additionally, maintaining a clear focus on problem identification (gangs and groups) is vital to sustained success.

Factors such as defining gang violence can potentially undermine such momentum. Even after recommending, measuring, and training NOPD supervisors multiple times to use an expanded definition of gang-member involved violent incidents, the agency routinely reverted back to its standard (conservative) definition of gang involvement in violence, requiring multiple recoding of the violent incidents by the research team and ongoing consultation and technical assistance to refocus the NOPD on the individuals and gangs most responsible for violence in the city. We conclude by reiterating that researcher–practitioner partnerships are imperative not only for problem identification, implementing effective strategic approaches, resource management, and evaluation purposes, but also for helping law enforcement keep track as to which individuals and groups are driving a city's violent crime problems (see Kennedy, 2009b) and for maintaining programmatic sustainability. Each of these tasks is important for successful policy transfer and program implementation. Although researchers have long acknowledged that measurement matters for evaluation purposes, it should now be clear that measurement matters a great deal for programmatic implementation.

Appendix A: Time-Series Intervention Estimate Sensitivity Tests on Targeted Crime Outcomes (Total Homicides, GMI Homicides, Firearm Homicides, and Total Firearm Assaults)

	Total Homicides[a]		Firearm Homicides[a]		GMI Homicides[b]		Firearm Assaults[b]	
Variable	Coefficient (St. Error)	Model Wald Statistic	Coefficient (St. Error)	Model Wald Statistic	Coefficient (St. Error)	Model Wald Statistic	Coefficient (St. Error)	Model Wald Statistic
Lead + 2	−.166 (.086)	25.65	−.092 (.095)	29.24	−.251* (.124)	22.68	−.150* (.068)	35.99
Lead + 1	−.166 (.087)	25.65	−.153 (.092)	24.91	−.323** (.114)	29.02	−.157* (.067)	35.51
Intervention (T_0)	−.191* (.085)	25.43	−.178* (.092)	24.05	−.387** (.115)	18.35	−.177** (.067)	37.06
Lag − 1	−.171* (.090)	25.52	−.160* (.098)	22.31	−.353** (.115)	29.15	−.188** (.073)	35.38
Lag − 2	−.219** (.086)	26.57	−.228** (.091)	26.42	−.398** (.119)	34.41	−.213** (.076)	37.70

Note. Monthly dummy variables used to capture systematic seasonal fluctuations are estimated in each sensitivity model.

[a] Time series: January 1, 2008 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = July 2009, 2 = February 2010, 3 = March 2012, 4 = May 2012).

[b] Time series: January 1, 2010 to March 31, 2014, and random placebo preintervention and postintervention dates (1 = March 2010, 2 = February 2011, 3 = August 2011, 4 = January 2012).

*$p < .05$. **$p < .01$.

Appendix B: Impact on Targeted Violence Within Geographic Settings with Competing Strategic Interventions (January 1, 2008 to March 31, 2014)

	Total Homicides				GMI Homicides			
	City Minus Central City		Central City Only		City Minus Central City		Central City Only	
Variable	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
Intervention	−.193** (.008)	−17.55%	−.131 (.432)	−12.27%	−.400* (.115)	−32.96%	−.161 (.414)	−14.87%
February	−.188 (.179)	−17.13%	−1.386 (1.055)	−74.99%	−.097 (.266)	−9.24%	−1.648 (1.030)	−80.75%
March	.158 (.179)	17.11%	.223 (.740)	24.98%	−.047 (.224)	−4.59%	.693 (1.081)	99.97%
April	.181 (.177)	19.84%	−.148 (.634)	−13.75%	−.192 (.195)	−17.46%	.893 (1.033)	144.24%
May	.048 (.159)	4.91%	−.554 (.755)	−42.53%	−.192 (.244)	−17.46%	.199 (1.253)	22.01%
June	.048 (.177)	4.91%	.139 (.664)	14.91%	−.261 (.198)	−22.97%	.199 (1.253)	22.01%

	Total Homicides				GMI Homicides			
	City Minus Central City		Central City Only		City Minus Central City		Central City Only	
Variable	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100	Coefficient (St. Error)	(IRR-1) × 100
July	.162 (.176)	17.58%	−1.247 (1.036)	−71.26%	.017 (.238)	1.71%	−1.650 (1.043)	−80.79%
August	−.172** (.224)	−15.80%	.698 (.544)	100.97%	−.885** (.293)	−58.72%	1.991** (.924)	632.28%
September	−.158 (.170)	−14.62%	.362 (.712)	43.62%	−.192 (.209)	−17.47%	1.586 (1.089)	388.41%
October	−.158 (.231)	−14.62%	.139 (.666)	14.91%	−.261 (.285)	−22.97%	.892 (1.033)	144.00%
November	−.242 (.200)	−21.49%	−1.226 (1.048)	−70.65%	−.321 (.374)	−27.45%	.239 (1.292)	26.99%
December	−.025 (.194)	−2.46%	.160 (.759)	17.35%	.138 (.202)	14.79%	.932 (1.287)	153.95%
Intercept	2.70** (.147)		−.523 (.506)		2.29** (.187)		−1.548 (.899)	

*$p < .05$. **$p < .01$.

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