# Reducing Gang Violence Using Focused Deterrence: Evaluating the Cincinnati Initiative to Reduce Violence (CIRV)

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Research indicates that focused deterrence interventions are associated with violence reductions, although levels of success vary across sites. It is unknown if these strategies can produce sustained reductions over time, and if the variation in success is due to differences in program activities and dosages. This study provides a detailed description and evaluation of the Cincinnati Initiative to Reduce Violence (CIRV), a focused deterrence violence reduction strategy implemented in Cincinnati, Ohio. CIRV's organizational structure and enhanced social services were designed to address sustainability issues that threaten to undermine long-term success. Results from our pooled time series regression models indicate that two violent outcomes——group/gang-member involved homicides and violent firearm incidents——declined significantly following implementation. These declines were observed in both 24- and 42-month post-intervention periods, but not in comparison outcomes. Additional analyses, however, reveal that provision of social services was not responsible for the significant and sustained decline.

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## Introduction

Despite a national decline in gun violence beginning in the mid-1990s, some cities, including Cincinnati, Ohio, experienced a rise in homicides at the beginning of the last decade. From 1991 to 2000, Cincinnati averaged 41.3 homicides per year, a relatively low per capita rate compared to other large Ohio and regional cities. From 2001 to 2006, however, the annual average was 73.3, culminating in a modern-day high of 88 homicides in 2006. Nearly three-quarters of homicides during a one-year period (June 2006-June 2007) involved a known violent group member as either a victim or suspect (Engel, Baker, Tillyer, Eck, & Dunham, 2008). In April 2007, Cincinnati's political leadership partnered with law enforcement officials, academics, medical professionals, street advocates, and community and business leaders, to form the Cincinnati Initiative to Reduce Violence (CIRV, pronounced "serve"). CIRV is loosely modeled after Boston's *Operation Ceasefire* and employs a focused deterrence strategy to directly communicate consequences for violence to at-risk gang members (Braga, Kennedy, Waring, & Piehl, 2001).

Boston experienced a 63% decline in homicide among youths following the implementation of the strategy (Braga, Kennedy, Waring, and Piehl, 2001), which led many jurisdictions across the United States and beyond to try to replicate this success by implementing similar initiatives (Tillyer, Engel, & Lovins, 2010). The general success of these strategies was documented in a series of studies aimed at describing the impact of focused deterrence on violent crime problems. Some of these studies, however, lacked scientific rigor, bringing the empirical status of focused deterrence into question (Wellford, Pepper, & Petrie, 2005). More recently, several evaluations have set a new standard in methodological and statistical procedures (Braga, Pierce, McDevitt, Bond, & Cronin, 2008; McGarrell, Chermak, Wilson, & Corsaro, 2006; Papachristos, Meares, & Fagan, 2007). What has emerged is a growing body of research that suggests various focused deterrence approaches have been successful in reducing violence, at least in the short-term. A recent meta-analysis reaffirms a statistically significant programmatic impact across 11 studies examined (Braga & Weisburd, 2011). It is unknown, however, if these initiatives continued to reduce violence over longer periods of time *and* if the variation in success experienced across implementation sites corresponds with differences in program activities and dosage. In short, in-depth descriptions of the differences across initiatives, and a clear understanding of the issues surrounding implementation and sustainability are generally lacking in this literature.

The present study describes the implementation of CIRV in detail and evaluates its impact on gang-related homicides, while taking into account the existing literature on the methodological and statistical considerations associated with evaluating citywide interventions. We provide a thorough discussion of

the implementation of the initiative, which differed in several respects from other "pulling levers" initiatives. In addition to the traditional law enforcement components of focused deterrence strategies, the CIRV team focused heavily on developing a sound managerial structure and a comprehensive social service component, with the specific goal of sustainability in violence reduction. In our evaluation, we examine both short-term and long-term impact on violence, and further consider the specific aggregate effects of the social service component of the initiative. We conduct this evaluation after careful consideration of the methodological decisions and dilemmas faced when assessing the impact of these initiatives, and conclude with a discussion of the next steps in focused deterrence research.

## Gangs, Groups, and Violence

The problems associated with criminal gangs have been well established. For example, the link between gang membership and increased risk for violence (as offenders or victims) is clearly documented in the criminological literature (e.g. see Battin-Pearson, Hill, Abbott, Catalano, & Hawkins, 1998; Curry, Decker, & Egley, 2002; Esbensen & Huizinga, 1993; Loeber, Kalb, & Huizinga, 2001; Ozer & Engel, 2011; Rosenfeld, Bray, & Egley, 1999; Taylor, Peterson, Esbensen, & Freng, 2007; Thornberry, Krohn, Lizotte, Smith, & Tobin, 2003). Research generally demonstrates that gang membership and associated violence is increasing, as are the use of specialized law enforcement gang units, and anti-gang interventions (Decker, 2007; National Gang Intelligence Center, 2009).

While there is little debate regarding the correlation between gang membership and criminal offending/victimization, there has been a growing concern regarding the definition of "gangs" and gang member identification (Esbensen, Winfree, He, & Taylor, 2001; Sullivan, 2006). As noted by Kennedy (2009a, p. 711), scholars and practitioners alike have struggled with constructing a "legally compliant, conceptually sound, useful, and interjurisdictionally accepted model for gang databases." Others have demonstrated the widely varying gang classification criteria across states (Barrows & Huff, 2009) and local jurisdictions (Klein, 2009). This is increasingly problematic given the enhanced criminal penalties associated with documented gang status.

We note at the onset of our study that the terms "groups" and "gangs" were used interchangeably in Cincinnati (and also in our evaluation). Historically, there have been very few highly organized, intergenerational gangs with national affiliations in Cincinnati. Rather, violence tends to stem from loosely-knit social networks of individuals that hang together on the street and promote violence as a means of handling conflict (Engel & Dunham, 2009; Engel et al., 2008). These are the types of episodic groups that are typical in most mid-sized urban centers (Howell, 2007; Kennedy, 2009a). These groups often do not rise to the status of "gang" as typically defined by academics and

practitioners (e.g. they may not have hierarchical structures, initiation rituals, common tattoos, and colors; in fact many do not even have a name, but are only identified by the territory where they hang out). Many of these would not meet the legal qualifications necessary for enhanced penalty under the State of Ohio (2923.42 Ohio revised code); nevertheless, they contribute significantly to the violence problem in Cincinnati, and were the subjects for the violence reduction intervention.

Whether identified as violent groups or gangs, individuals affiliated in these networks have been the subject of scores of different anti-violence initiatives in the past several decades. These initiatives are typically implemented in large or medium-sized cities, and are designed to reduce gang violence, albeit by focusing on different aspects of the problem. For example, anti-gang strategies have included programs that focus on primary prevention, secondary prevention, intervention, and suppression (for review, Decker, 2003; National Gang Center, 2010). One gang violence reduction strategy——focused deterrence——has been implemented in many cities, and is growing in popularity among practitioners, politicians, community groups, researchers, and funding agencies (e.g. through Strategic Alternatives to Community Safety Initiative and Project Safe Neighborhoods) (Braga & Weisburd, 2011; Dalton, 2002). Focused deterrence strategies are grounded in the literature that shows a substantial portion of serious violence is committed by repeat offenders organized in groups or gangs (Kennedy, 1997; Tracy, Wolfgang, & Figlio, 1985).[1] Issues of respect and reputation underlie the violence that escalates within the street group context (Anderson, 1999; Bourgois, 2003). Although a single individual may perpetrate a violent act, the group dynamic shapes behavior, how individuals are received by their peers, and how they respond to those who disrespect them. The core aspects of these strategies are described below.

## Focused Deterrence Strategies

Focused deterrence strategies are generally comprised of law enforcement, service, and community partners who reject the violence in clear, direct ways. First, various law enforcement agencies are coordinated to create meaningful and predictable consequences for groups who engage in violence. This component of the strategy is referred to as "pulling levers," as law enforcement attempts to pull every lever legally possible following a violent incident (Braga et al., 2001; Kennedy, 1997). To respond to violent groups in a swift and predictable manner requires a coordinated effort among several agencies that prioritize group violence, share information, and develop comprehensive group-focused responses. Direct and accurate communication of the strategy

1. It is also important to note Cook, Ludwig, and Braga (2005) found that although high incidence rates of lethal violence were committed by groups of chronic offenders with prevalent, serious, and often violent criminal histories, a large portion of overall lethal violence is often committed by individuals with no prior arrests or convictions.

to the target population is of central importance to increase compliance (Kennedy, 1997). This is often done during "offender notification meetings" (also referred to as "call-ins" or "forums"), during which violent group members are warned that if any member of their group commits a homicide, the entire group will become the priority of law enforcement. In many ways, the focused deterrence strategy relies on the organization of these individuals in groups. Not only does the group structure serve as a communication vehicle, it is also a potential source of a control, as the group-focused enforcement strategy encourages groups to police themselves to avoid becoming the priority of a multi-agency enforcement effort (Tillyer & Kennedy, 2008).

Second, assistance for those who want to transition out of the violent lifestyle is often offered in the form of social and job services. The social service component of the strategy has been described in less detail in the literature and there appears to be considerable variation in the nature and extent of services available across intervention sites (Tillyer et al., 2010). Finally, key leaders within communities assist in the development of community engagement activities and strive to create a "moral voice" of the community by delivering a clear message of non-violence and rejecting the norms and narratives of the street which promote violence. In some cities, the pulling levers law enforcement component is the most prevalent form of intervention (e.g. Indianapolis), whereas other types of violence reduction initiatives focus more on community engagement.

In summary, focused deterrence strategies target "very specific behaviors by a relatively small number of chronic offenders who are highly vulnerable to criminal justice sanctions" and "directly confronts offenders and informs them that continued offending will not be tolerated and how the system will respond to violations of these new behavior standards" (Braga & Weisburd, 2011). Focused deterrence approaches have been used to reduce gang violence, open air drug markets, robberies, and most recently, domestic violence (Corsaro, Brunson, & McGarrell, 2010; Corsaro, Hunt, Hipple, & McGarrell, forthcoming; Kennedy, 2009a, 2009b). Most directly relevant for our purposes are comparisons to other jurisdictions that have used focused deterrence initiatives to reduce gang violence. Table 1 describes 11 evaluations of focused deterrence initiatives designed to reduce gang violence in eight cities and one national initiative. The evaluation methods differ in design, measurement, and statistical analyses, yet consistently demonstrate reductions in the crime problems of interest (e.g. homicides, gun assaults, violent crime, etc.) that range from 24 to 63%.

Other scholars, however, have raised concerns regarding these effectiveness claims, and have questioned whether focused deterrence approaches should be credited with violence reductions. For example, Fagan's (2002) review of gun homicides in Boston and other Massachusetts cities suggests a general downward trend in violence across locations that began prior to Boston's *Operation Ceasefire*. Likewise, Ludwig (2005) reexamined homicide data in Boston using a

**Table 1**  Summary of findings from focused deterrence violence reduction initiatives

| Author(s) | Location(s) | Research design and analysis | Results |
|---|---|---|---|
| Braga et al. (2001) | Boston, MA | Interrupted time series design comparing pre- and post-intervention months (Generalized Linear Models) | Significant decreases in citywide homicide victims ages 24 and younger (63%), citywide shots-fired citizen calls for service (32%), and gun assaults in one high activity police district (25%) |
| | | Nonrandomized quasi-experimental design comparing youth homicide trends (i.e. victims 24 and younger) in Boston to trends in 39 large US cities (Generalized Linear Models) | Four cities had significant changes in youth homicide; Boston's estimated effect was the largest |
| Papachristos et al. (2007) | Chicago, IL | Quasi-experimental design with near-equivalent control group (fitted linear growth curve models), with these PSN indicators as predictors:<br><br>• a dummy variable indicating group assignment<br>• percentage of gun offenders in the areas who attended a notification meeting<br>• number of federal prosecutions<br>• the person-month sentences of federal prosecutions<br>• the number of ATF gun seizures<br>• a composite index of each of these measures | 37% decline in quarterly homicide rates in treatment area<br><br>All PSN indicators except the number of federal prosecutors had a significant effect |
| | | Statistically controlled for beat-level deprivation, concentrated disadvantage, and residential instability | None of the PSN variables were associated with a decline in arrest for aggravated assaults or aggravated batteries |

**Table 1** (*Continued*)

| Author(s) | Location(s) | Research design and analysis | Results |
|---|---|---|---|
| Hunt, Sumner, Scholten, and Frabutt (2008) | High Point, NC | Mixed methodology (GIS "hotspot" analysis; qualitative narratives) | Perceived elimination of violence, drug markets, and disorderly conduct incidents among community resource officers and local residents that were interviewed |
| | | Descriptive comparison of drug crime trends (18 pre/post month examination period) | Immediate and sustained reduction (31%) in drug-related offenses in West End neighborhood |
| | | Descriptive comparison of violent crime trends (18 pre/post month examination period) | Significant reduction (37%) in violent crimes in West End neighborhood |
| McGarrell et al. (2006) | Indianapolis, IN | Comparison between pre- and post-intervention monthly homicide counts using six cities as controls (ARIMA) | Significant reduction (34.3%) in homicides; no other city experienced a significant change |
| Corsaro and McGarrell (2009) | | Comparison of gang and non-gang homicides (coefficient-difference tests comparing estimates from ARIMA models) | Gang homicides declined significantly more than non-gang homicides |
| Tita et al. (2005) | Los Angeles, CA (Boyle Heights Area) | Comparison between pre- and post-intervention in five police reporting districts that received intervention, using other police reporting districts in Boyle Heights as controls (descriptive) | 34% decline in violent crime in treatment area; 24% decline in control area |
| | | | Gang crime declined initially, but began to rise after intervention ended |
| | | | Gun crime declined by approximately 1/3 in both the treatment and control areas |
| Braga et al. (2006) | Lowell, MA | Comparison between pre- and post-intervention gun assaults and homicides (descriptive) | 24% reduction in gun assaults (from 49 to 37); 50% reduction in homicides (from 4 to 2) |

**Table 1** (*Continued*)

| Author(s) | Location(s) | Research design and analysis | Results |
|---|---|---|---|
| Braga et al. (2008) | | Comparison between pre- and post-intervention monthly gun assaults and gun homicides (Poisson regression controlling for linear trends, non-linear trends, seasonal effects, population trends, and violent crime trends; other major Massachusetts cities used as controls) | Significant reduction (27.8%) in gun assaults and gun homicides; monthly average decreased from 5.4 to 3.9 (no controls experienced a decline) |
| Kennedy and Braga (1998) | Minneapolis, MN | Interrupted time series design comparing pre- and post-intervention June, July, and August yearly totals of homicide victims (descriptive) | Reductions in homicide (exact numbers not reported) |
| | | Interrupted time series design comparing pre- and post-intervention yearly totals of homicide victims (descriptive) | Reductions in homicide (exact numbers not reported) |
| Braga (2008) | Stockton, CA | Comparison between pre- and post-intervention, with eight California cities as controls (negative binomial regression controlling for linear trends, non-linear trends, seasonal effects, and violent crime trends) | Significant reduction (34.5%) in citywide monthly gun homicides (from 2.9 to 1.9); none of the controls experienced a significant reduction |
| McGarrell et al. (2010) | US Federal Districts: Nationally implemented PSN initiative | Comparison of 82 treatment cities and 172 non-treatment cities, controlling for state incarceration rate, police density, concentrated disadvantage, and population density (Hierarchical Generalized Linear Models) | High dosage PSN treatment cities experienced significant reductions in violent crime (low dosage and treatment cities experienced no significant change) |

longer pre-intervention time frame and concluded that the reductions in homicides in Boston were similar to the reductions experienced in the 25 largest US cities during the same time frame. Rosenfeld, Fornango, and Baumer (2005) considered the homicides rates of the 95 largest US cities to examine the impact of New York City's *Compstat*, Boston's *Operation Ceasefire*, and Richmond's *Project Exile* on reductions in homicides. Rosenfeld et al.'s (2005) growth curve analysis demonstrated a reduction in youth homicides in Boston compared to other cities, but their model generally "did not find evidence supportive of a program impact on homicides trends for Boston's Operation Ceasefire" (p. 438). Yet, these authors also noted the possibility that their methods lacked sufficient statistical power to detect an intervention effect because of the small number of youth homicides.[2]

While the Boston evaluation received the most attention, focused deterrence strategies in Minneapolis, Stockton, East Los Angeles, and High Point were also evaluated. Some of these studies, however, lack the methodological and statistical rigor of the original Boston evaluation. Given the numerous uncontrolled factors that could influence violence, coupled with the lack of strong methodological designs, the National Academy Panel concluded that evidence on the effects of focused deterrence violence reduction strategies was scientifically weak (Wellford et al., 2005). As noted by Braga et al. (2008), however, more rigorous evaluations have been conducted in other cities since the National Academy Panel's conclusions that provide compelling evidence regarding the effectiveness of focused deterrence approaches. Specifically, the quasi-experimental evaluations in Indianapolis (Corsaro & McGarrell, 2009; McGarrell et al., 2006), Chicago (Papachristos et al., 2007), and Lowell (Braga et al., 2008) have set new standards for rigorous evaluation that have demonstrated program effects.

In addition to internal validity concerns, the external validity of focused deterrence research deserves careful consideration. Though the principles of focused deterrence have been documented in the literature several times, many studies lack specific details related to program activities and dosage. As Eck (2010) notes in a recent essay on policy relevance and external validity, policy is in the details; criminologists must describe the policies studied with sufficient clarity that they can be replicated. Eck (2010) draws on Shadish, Cook, and Campbell's (2002) five dimensions of external validity to demonstrate how researchers can make policy research more useful, arguing that researchers should describe the units, treatments, outcomes, settings, and mechanisms related to the research. That is, studies should clearly describe the cases they study, the operationalization of the intervention and dependent

2. Note, however, that Berk (2005) criticized Rosenfeld et al.'s (2005) approach as "assume-and-proceed statistics" (p. 455) and argued that they made multiple assumptions about their data that cannot be verified, and therefore "the statistical tools applied far outstrip current substantive knowledge" (p. 457).

variable(s), the context in which the treatment is applied, and the process by which the treatment produces the results (Eck, 2010).

Despite the methodological issues noted above, Braga & Weisburd's (2011) meta-analysis of 10 quasi-experimental evaluations and one randomized controlled trial demonstrates that "focused deterrence strategies are associated with an overall statistically significant, medium-sized crime reduction effect." These collective results from the focused deterrence impact evaluations are very promising and have spawned multiple replications across the country and around the world (National Network for Safe Communities [NNSC], 2011). One such inspired intervention was the CIRV in Cincinnati, Ohio.

## Cincinnati Initiative to Reduce Violence

CIRV was created in April 2007 following a sharp increase in fatal and non-fatal gun violence in Cincinnati. The initiative adopted a focused deterrence approach and aims to create and communicate meaningful consequences for gang violence to at-risk group members. CIRV's objective was clearly identified as the reduction of homicides and gun-related violence perpetrated by group members, and the initiative was organizationally designed at the onset to meet this goal. The CIRV team was concerned that any reductions in violence would be difficult to sustain over time (Kennedy, 2007). Therefore, in addition to the traditional law enforcement-based intervention, CIRV leaders also developed a system for sustainability through a comprehensive organizational design and an enhanced social service component. In this regard, CIRV was generally considered a model initiative, hosting visitors from other cities around the world. In addition, CIRV formally served as a model for other cities in the state of Ohio, and as a leadership city of the NNSC (Engel et al., 2009).

To enhance both institutionalization and sustainability, local business executives from the Procter & Gamble Co. assisted in the development of a comprehensive organizational structure that consisted of four Strategy Teams——law enforcement, services, community engagement, and systems——overseen by a Strategy/Implementation Team, which ultimately answered to a Governing Board. The Governing Board was comprised of high-ranking city officials and was responsible for providing resources to the initiative, as well as overcoming barriers that impeded success. The Strategy/Implementation Team——comprised of two co-chairs (who serve as the primary spokespersons of the initiative), the owners of each individual strategy team, expert consultants, and the executive director——was tasked with daily operations, including making key decisions, developing program strategies, securing resources, and monitoring results. Finally, the individual CIRV Strategy Teams (law enforcement, services, community engagement, and systems) were responsible for executing particular elements of the overall initiative, which included: (1) increasing the risks and costs of involvement in violence;

(2) providing alternatives to violence; and (3) changing community norms toward violence (Engel et al., 2008).

## Increasing the Risks and Costs of Involvement in Violence

The CIRV law enforcement (LE) team[3] was committed to organizing and sharing information to comprehensively respond to group-related violence. The team systematically identified individuals most at risk as perpetrators and/or victims of gun violence through group affiliations and activities. Systematic research with front-line law enforcement officers in 2007 provided a vivid picture of a hyperactive offender population in Cincinnati: approximately 0.3% of the city's population, with prior records averaging 35 charges apiece, were members of violent episodic groups (or in some cases, more structured gangs). A review of homicides revealed that these violent groups were associated with three-quarters of the city's homicides during a one-year period (Engel et al., 2009), and that an overwhelming majority of homicide victims were black (76%), male (81%), and killed by firearms (82%) (Engel et al., 2008).

From May 2007 to December 2010, multiple LE Team intelligence-gathering sessions identified a total of 2,431 individual members of violent groups within the city at some point during this time period. Most recently, in December 2010, there were 46 active violent groups identified with 1,761 known active members. Group members' ages ranged from 11 to 67, with an average age of 26.2 years.[4] The size of groups ranged from 3 to 172 identified participants, with an average of 35 members per group. Using statistical analyses, geographic mapping, and social network analyses, this population——most at risk to be victims and/or suspects in gun-related violence——was routinely tracked and shared with CIRV partners for strategic deployment of resources.

Similar to other focused deterrence strategies (e.g. Braga et al., 2001; Papachristos et al., 2007), CIRV used offender notification meetings (call-in sessions) to communicate the consequences of violence to a portion of the target population. Of the identified group members, approximately 20-25% were under court-ordered probation or parole at any given time and could be compelled to attend offender notification meetings held at the county and federal courthouses. From July 2007 through December 2010, there were 28 call-in

3. LE team members include: CPD, Hamilton County Sheriff's Office, Hamilton County Adult Probation, Ohio Adult Parole Authority, Hamilton County Prosecutor's Office, US Attorney's Office, and Bureau of Alcohol, Tobacco, Firearms, and Explosives, and supported by the Ohio State Attorney General's Office and the Ohio Office of Criminal Justice Services.
4. In Cincinnati, there are few known stand-alone juvenile gangs; rather the majority of violent groups in the city have both juvenile and adult members (Engel & Dunham, 2009); law enforcement officials believe that the adult members are able to exert pressure/influence over the younger members (also see Braga et al., 2008).

sessions held with 568 violent group members. These sessions ranged in size from 7 participants to 98, with an average of 20 individuals per session. Of the total identified violent group members in the city across the span of the initiative, 32.5% have attended at least one call-in session, and 43.4% of those attending have attended multiple sessions. Individuals were told at these meetings to share the message with their peers, but it is unknown if the violent group members who did not attend these sessions were made aware of the CIRV message. The CIRV team also conducted four offender notification meetings in prison settings with 168 offenders scheduled to be released to Cincinnati neighborhoods within six months. Finally, the LE Team completed 163 "home/street visits" to deliver the anti-violence messages to specifically identified high-risk violent group members under court-mandated supervision. This collaborative enhanced supervision technique was modeled loosely after Boston's *Operation Night Light* (Jordan, 1998; Reichert, 2002) and designed as a narrowly focused, short-term deterrent.

The deterrent effect of CIRV is presumably based in part on law enforcement's perceived credibility to the target population. The LE Team promised that if a homicide was connected to an individual that was a member of a group, they would use whatever legal means necessary to bring criminal charges against all group members (for involvement in any criminal activity). Targeted group enforcement was then communicated at the next call-in session (i.e. an example was made of one group, and others were told if they did not stop engaging in gun violence, they would be the next targeted group). During the 42-month evaluation period, 17 groups were targeted for law enforcement action and enhanced penalties based on their involvement in homicides and gun violence. This culminated in 318 physical custody arrests of 223 offenders for various felony and misdemeanor charges (some were arrested multiple times), along with 17 individuals indicted on federal charges. These arrests and charges were detailed at subsequent notification meetings to demonstrate the return on the promises made. In addition to these planned gang enforcement actions, identified group members were routinely arrested for other crimes. During the evaluation period, approximately 75% of all identified group members were arrested at least once, totaling 7,486 separate arrests.

## Providing Alternatives to Violence

The Services Team was designed to provide meaningful alternatives to violence for group members. The goals specific to this team included creating a life-change system that successfully engaged members of violence-prone groups and moved them to a gun violence-free, pro-social, and eventually, employment-based lifestyle. Comprised of a social service agency, employment agency, and CIRV Street Advocates, this team attempted to provide immediate, tailored services to those willing to step away from a life of violence.

Providing "would-be offenders" with alternatives to violence has been an inherent weakness of most violence-reduction focused deterrence approaches (Tillyer et al., 2010). Specific CIRV strategies for increasing alternatives to violence included: (1) providing services to offenders that address their criminogenic needs; (2) direct outreach, including coaching and mentoring; and (3) violence interruption activities in targeted neighborhoods. These strategies are described in greater detail below, but first we note issues surrounding client selection. A problem with the initial design of the Services Team was an inability to focus service delivery directly on the target population (violent group members). The "client" population for social services is self-selected. Due to credible security concerns, social service providers were not provided access to the law enforcement-generated list of violent group members. Services were initially provided to anyone who contacted the CIRV Street Advocates. Potential clients were notified of the streamlined social services available through a number of communication mechanisms, including notification at the call-in sessions; direct contact in their neighborhoods with street advocates; law enforcement personnel; community members; and marketing campaigns both within particular neighborhoods and citywide. By casting this wide net, many of the social services were provided to individuals who were not the intended targets of the initiative (i.e. group members). Eighteen months after initial implementation, a system for screening clients for likely involvement in violence (used prior to receiving services) was developed and used.

Initially, the CIRV Services Team focused on providing willing offenders with job-readiness training. Over time, members of the Services Team conceded that only providing job training to a highly violent, gang-involved, criminal population was unlikely to successfully modify behavior. In 2009, the team was restructured to deliver services that were believed to be more effective in reducing future criminal behavior by addressing four basic principles for effective intervention: risk, need, responsivity, and fidelity (Andrews & Bonta, 2003; Gendreau, 1996). Valid risk assessment tools, including a composite risk assessment (Ohio Risk Assessment System) and a violence screener (Violence Triage Tool), were used to identify those at the greatest risk for violence (Latessa, Lemke, Makarios, Smith, & Lowenkamp, 2010). These screening procedures were designed to aid the Services Team in the delivery of services to the targeted gang-involved individuals. The focus was expanded beyond employment to include targeting antisocial attitudes, peers, behaviors, and personality factors. A cognitive-behavioral treatment model was also adopted, which offered an opportunity to address offenders' antisocial attitudes while teaching new skills to effectively manage their environment (Dowden & Andrews, 2004). Monthly staff meetings monitored the services delivered and the progress made by offenders, and a centralized data collection process was created to track clients.

Direct outreach was provided by 14 CIRV Street Advocates. Loosely modeled after Boston and Chicago *Ceasefire* outreach workers, the advocates served

multiple purposes, including social work, case managers, and violence inter-vention. They were selected based in part on their personal experience in low-income, high-crime neighborhoods, and the criminal justice system. These experiences allowed them to connect one-on-one with those at increased risk for violence (Skogan, Hartnett, Bump, & Dubois, 2009). CIRV Street Advocates strived to provide immediate and tailored services to individuals choosing to leave the life of violence, support for victims' and offenders' families, and alternatives for high-risk individuals.

From July 2007 through December 2010, the Street Advocates assessed 622 clients who contacted them for services. Of these, 325 (52.3%) were referred to job training, 55 (8.8%) were provided intensive social services under the new system, and the remaining 242 (38.9%) were provided some type of undocumented informal counseling and mentoring by street advocates. Of the 325 clients referred to an employment agency to begin job-readiness training, 199 (61.2%) completed the training, and of those, 104 (52.3%) obtained their first job. The vast majority of clients were black (93.4%), male (93.2%), unemployed (84.1%), single (66.2%), with at least one child (53.1%), less than a high school education (60.8%), and a felony record (69.1%). Importantly, of the 622 clients assessed and provided some type of social services, only 138 (22.2%) were also identified by law enforcement as a member of a violent group. Therefore, the CIRV services component might be better described as providing social services to self-selecting violent group members and others (non-group members) at risk for criminal offending, whereas CIRV law enforcement efforts were concentrated directly on identified violent group members.

CIRV Street Advocates also focused on outreach activities related to vio-lence interruption, mediation, and spreading non-violence messages to the community. Similar to *Chicago Ceasefire*, violence interruption tactics included CIRV Advocates deployed to violence hot spots and funerals to assess gang-related conflicts and intervene prior to escalation. From January 2009 to December 2010, Street Advocates documented 75 incidents in which they believed imminent violence between two or more individuals had been dis-rupted through their intervention. Their specific methods were not docu-mented; however, they informally described providing to those individuals at high-risk of engaging in violence with suggestions for non-violent conflict reso-lution. Several cities have reported an assortment of problems with street workers, including high turnover, little traditional work experiences, difficul-ties in supervision and evaluation, inadequate training, and unsystematic responses to conflict situations (Skogan et al., 2009; Wilson, Chermak, & McGarrell, 2010). While it has been suggested that the work of street advo-cates is critical for violence reduction, it can also lead to devastating results if they are not properly selected, trained, and held accountable for their activi-ties (Wilson et al., 2010). The CIRV Street Advocate component also suffered from some of these problems, with three advocates arrested for criminal offenses while they were funded by the city, ultimately leading to the suspen-

sion of the Street Advocates in December 2010, and reduced funding for the overall initiative in January 2011.

## Changing Community Norms Regarding Violence

The CE Team was responsible for changing community norms regarding violence. The activities of this team were based on the principles of *Chicago Ceasefire* (Skogan et al., 2009), with specific objectives that included: forming relationships with individuals and organizations in affected communities to articulate norms and expectations; delivering the "moral voice" message that gun violence is not acceptable; and rejecting the norms and narratives of the street that promote violence. Team members represented various interests and groups who reject violence and work to rebuild communities.

Specific tactics were used to increase collective efficacy by empowering neighborhoods to mobilize and exert informal social control (Sampson, Raudenbush, & Earls, 1997). The CE Team used a variety of strategies to engage the community and documented the following activities from July 2007 to December 2010: 104 community trainings; 71 youth violence prevention programs; 377 outreach events; 325 shooting responses; attendance at 62 funerals and vigils; and 54 CIRV-specific community engagement activities. These community-based activities were directed toward residents in neighborhoods with the highest levels of gun violence, and on occasion, specific gang territories were directly targeted.

Collectively, CIRV sought to reduce violence through implementation of specific focused deterrence-based strategies and tactics at the individual, group, neighborhood, and citywide levels. As with many policy-oriented initiatives, CIRV presents a number of challenges for rigorous evaluation (Rosenfeld et al., 2005). First, the initiative was implemented citywide, which therefore precludes comparison to control areas without intervention. There were multiple units of analysis for the specific intervention points, requiring different types of evaluation designs. In addition, different components of the initiative were implemented simultaneously, making it difficult to interpret which components of the initiative had an effect. Finally, CIRV's focus on reducing group-member involved homicides limits our ability to compare results across cities. These obstacles, and our response to them, are described in greater detail below.

## Methodology

Our review of prior evaluations provides evidence that certain types of violent offenses appear to be specifically impacted by focused deterrence initiatives. Many specialized working groups have implemented suppression tactics, notification sessions, and enhanced prosecution efforts in specific geographic contexts within cities (Corsaro et al., 2010; Kennedy & Wong, 2009; Papachristos

et al., 2007; Tita, Riley, Ridgeway, & Greenwood, 2005), while others have targeted high-risk offenders that were networked within violent offending groups across large urban areas (e.g. gangs, groups selling illicit firearms, drug markets). In the latter instance, citywide initiatives seem to have the most significant influence on aggregated levels of youth gun homicides, lethal firearms incidents, and gang homicides (Braga et al., 2001, 2008; Corsaro & McGarrell, 2009; Papachristos et al., 2007) relative to other types of violent crime. In essence, the potential crime reduction benefits appear to be conditioned on the goals of each individualized program. Thus, choosing an appropriate research design and modeling potential changes in relevant outcome measures were primary considerations for the current study.

Sampson (2010, p. 498) recently illustrated that studies drawing upon the evidence-based framework should consider a number of factors, including: (1) the quality of empirical measures; (2) extensive theory related to the outcome; (3) modeling selection into treatment; and (4) the rigor of the empirical statistical model. Previous findings across different focused deterrence evaluations illustrate the unique dynamics that must be considered between utilizing the most rigorous evaluation design possible (e.g. experimental, quasi-experimental, time series design) and an examination of outcomes that are posited to be specifically influenced by a targeted crime reduction initiative. All prior pulling levers evaluations have relied on the use of aggregated indicators of crime that capture potential changes in either general forms of violence (e.g. homicides, assaults, robberies) or specific measures of youth, gun, and gang incidents (Braga & Weisburd, 2011; Lum, Koper, & Telep, 2010). If one assumes relative consistency between similar empirical measures across cities, reliance on general violent crime outcomes provides an opportunity to conduct cross-city trend comparisons in an effort to improve internal validity. Note, however, that Loftin and McDowall (2010) illuminated the validity limitations that continue to plague official sources of publicly available crime data. Conversely, specific types of violence that a well-crafted focused deterrence program may be designed to reduce are not as consistently available for cross-city comparisons, such as gang-related homicides (see Decker & Pyrooz, 2010).

Based upon these considerations, we utilized a two-phase analytical strategy that allowed us to balance these central concerns by first assessing the unique hypothesized effect that CIRV was designed to have on specific types of violent incidents (i.e. group-related homicides and citywide violent firearm offenses). In the first analytical phase, we relied upon a pooled time series cross-sectional design examining monthly citywide violent incidents between 2004 and 2010 that were the direct target of CIRV. We sought to diminish the potential for regression to the mean and history effects that often threaten the internal validity of longitudinal designs (Cook & Campbell, 1979; Piquero, 2005). More specifically, all models included controls for fluctuating trends as well as seasonal influences that could obscure the estimated intervention effects. We contend that if CIRV was responsible for reducing specific types of violence that were consistent with the model of the initiative (i.e. firearm incidents

and lethal gang violence), any such changes in these targeted violent crime outcomes should also be compared to relative deviations (i.e. ebbs and flows) in the more general forms of violent crime during the same period that were not the direct focus of the strategy. In addition, we examined whether CIRV had a significant, as well as a long-term impact on specified outcomes by modeling the post-intervention period at distinct points in time (i.e. 24 and 42 month follow-up periods). While prior initiatives have shown significant short-term promise, the purpose of the present study was to examine the potential for a sustainable effect.

In the second analytical phase, we examined the relationship between social service provisions with changes in targeted violent crime outcomes during the post-intervention period using fixed-effects regression analyses. In this set of models, the number of individuals entering CIRV services per month (logged in order to reduce skewness) was included as both a zero order (i.e. instantaneous) and a lagged cross-correlation function (i.e. at one, two, and six months to capture potential longer term lagged effects) in separate and distinct models to examine whether the CIRV services component was at least partially correlated, after controlling for monthly fixed effects, with targeted violent crime outcomes. A total of eight separate models were estimated using this approach (i.e. four distinct time periods across two targeted violent crime outcomes). Although prior research has examined the influence of notification sessions, enhanced prosecution, and gun seizures on gang-related violence (see Papachristos et al., 2007), there has been no empirical examination of the influence of social services that were provided to high-risk offenders. One fundamental goal of CIRV was to create an infrastructure of longevity and sustainability by relying upon extensive social service provisions. Thus, we examined whether the instantaneous and lagged effects of social services provided a potential long-term reduction in violent crime.

## Data

The data used here include several types of violent criminal offenses reported to the Cincinnati Police Department (CPD) over a seven-year period.[5] The offense data are aggregated into a monthly format from 1 January 2004 through 31 December 2010, which equates to three and a half years of pre-intervention (i.e. baseline) and post-intervention data. Each month's violent crime measure is operationalized as a composite variable, running from its first through its last day for all offenses during this period. The measure of social

5. Klinger and Bridges (1997) provide empirical support for the preferred use of criminal offense data relative to calls for service measures when examining specific types of violent and predatory crime due to systematic undercounting and reporting issues observed with the latter method.

service provisions that took place from July 2007 through December 2010 (i.e. the post-intervention period) is also documented by the CIRV CE Team.

## Treatment and Control Outcome Variables

To examine the specific outcomes hypothesized to be affected by CIRV, we modeled systematic changes in the following dependent variables: (1) gang-related homicide incidents and (2) violent firearm incidents. *Group/ Gang Member Involved (GMI) homicides* were operationalized as fatal crime incidents where at least one of the actors (i.e. victim or suspect), or the situational circumstances surrounding the event, indicated that gang members were involved.[6] A total of 291 GMI homicides occurred between 2004 and 2010, which equates to roughly 57.6% of all citywide homicide incidents.

When modeling changes in GMI homicides between pre- and post-intervention, we compared estimates in the monthly number of *non-GMI homicides*, which has been specifically used as a control outcome in prior focused deterrence evaluations (see Corsaro & McGarrell, 2009). While changes in structural factors such as disadvantage, instability, and population structure have been shown to correspond with homicide levels in general (see Land, McCall, & Cohen, 1990), research indicates these structural correlates do not distinguish gang and non-gang homicides in a meaningful way (Rosenfeld et al., 1999). Therefore, *non-GMI homicide* serves as a reasonable control outcome.

We also examined changes in *violent firearm offenses*, which measured the aggregated number of fatal and non-fatal shooting incidents where each

6. GMI homicides were classified by CPD officials based on the following criteria and processes. First, the name of the victim and suspect(s) (if known) were cross-checked with the routinely updated, official violent-group database. If either the victim or the suspect(s) were known group members, the homicide was coded as GMI. Second, if the victim was not a known group member and the suspect was unknown, the totality of situational homicide characteristics were considered, including: location of the offense; suspected involvement of the victim in illicit acts preceding the homicide; manner and type of death; demographic 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, 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 updated classification is reflected in the current operationalization of the GMI homicide measure, through December 2010. Third, if the victim and/or suspect were known but did not appear in the group database, *and* a review of the circumstances as documented above indicated that they were group members, the individuals were added to the gang database, and the incident was coded as a GMI homicide. A single CPD commander was responsible for the final GMI classification of all homicides examined in these analyses; therefore there are no concerns regarding coder inter-rater reliability. We also note that domestic-related homicides that include group members were included as GMI incidents. The GMI classification indicates that a group member was involved in the homicide, but does not necessarily indicate that the crime itself was group-related. This coding approach may differ from other jurisdictions, making interagency comparisons difficult. This inclusive classification, however, provides for a more conservative test of CIRV's likely impact on violence reduction.

incident represents a unique victim injury. One of the objectives of the CIRV strategy was to identify geographic "hotspots" with disproportionate levels of firearms violence in order to disentangle the networks of offenders responsible for street violence in these areas. There were 2,988 shooting victims in Cincinnati between 2004 and 2010. This included 415 firearm-related homicide incidents (13.8% of the overall shooting incidents). Unfortunately, we were unable to distinguish non-fatal firearm incidents that were GMI-related. Due to the large number of shooting incidents during this period (i.e. an average of over 35 per month), the CPD did not systematically record whether such gun-related offenses are gang-related, and the research team has since been unable to retrospectively classify the shooting incidents as gang-related with consistency and validity. Therefore, our examination of violent firearm incidents is designed to capture specific types of offenses that were likely to include high-risk offenders involved with violent gun and drug groups (Blumstein, 1995), as well as illicit street networks where firearm use is more likely to be encouraged (Elliott, 1994; Fagan, 2002). Similarly, when examining differences in violent firearm offenses in the time series models, we compared the estimates to changes in *non-shooting violent offenses* (i.e. non-shooting robberies and aggravated assaults).

## Independent Variables

The interrupted time series analyses included a specific indicator that was designed to differentiate the pre- and post-intervention period. The *post-intervention* measure was operationalized as a dummy variable where the months between January 2004 through June 2007 were defined as the pre-intervention period (i.e. value = 0), and all subsequent months from July 2007 through December 2010 were defined as the post-intervention period (i.e. value = 1). Offenders were summoned to the initial notification session in July 2007, which was the first month that group/gang members were made aware that law enforcement consequences had changed; that social services were readily and consistently available; and that community members would no longer tolerate gun violence.

From an analytical perspective, it is important to note that violent crime trends and longitudinal data in general experience changes, drifts, and seasonal deviations (Box & Jenkins, 1976). Piquero (2005) illustrated that policy-driven strategies that are implemented at a time when there is an overall "regression to the mean" are often inappropriately credited with impacting crime rates, while initiatives that correspond with increases in crime are generally perceived to be ineffective. In either case, the specific impact of the intervention itself is difficult to identify. It was important to control for general history effects and threats to internal validity (Cook & Campbell, 1979). We added both a *linear trend* variable and a *trend squared* variable to all models as a mechanism to account for linear trends and curvilinear inflections

**Table 2**  Descriptive statistics ($N = 84$ months)

|  | Mean | SD |  | Max |
|---|---|---|---|---|
| *Dependent variables* |  |  |  |  |
| GMI homicides | 3.46 | 2.09 | 0 | 9 |
| Non-GMI homicides | 2.55 | 1.65 | 0 | 8 |
| Violent firearm incidents | 35.57 | 10.21 | 10 | 59 |
| Non-shooting violent incidents | 183.54 | 46.93 | 98 | 280 |
| *Time varying independent variables* |  |  |  |  |
| CIRV services (Ln) | 2.54 | 0.62 | 1.10 | 3.81 |
| Post-intervention[a] | 0.50 | 0.50 | 0 | 1 |
| Trend | 42.50 | 24.39 | 1 | 84 |
| Trend squared | 2,394.17 | 2,139.77 | 1 | 7,056 |
| January | 0.08 | 0.28 | 0 | 1 |
| February | 0.08 | 0.28 | 0 | 1 |
| March | 0.08 | 0.28 | 0 | 1 |
| April | 0.08 | 0.28 | 0 | 1 |
| May | 0.08 | 0.28 | 0 | 1 |
| June | 0.08 | 0.28 | 0 | 1 |
| July | 0.08 | 0.28 | 0 | 1 |
| August | 0.08 | 0.28 | 0 | 1 |
| September | 0.08 | 0.28 | 0 | 1 |
| October | 0.08 | 0.28 | 0 | 1 |
| November | 0.08 | 0.28 | 0 | 1 |

[a]Based on full time series (January 2004-December 2010).

in the outcome measures, which were apparent in the bivariate graphs.[7] Similarly, we included *monthly dummy variables,* using December as the reference month, to account for seasonal effects (i.e. seasonal shocks) that consistently occurred during specific months of the year (Table 2).

## Analytical Strategy

We utilized Generalized Linear Modeling (GLM) pooled time series regression analysis to estimate the impact of the CIRV initiative over time. Ordinary Least Squares (OLS) regression models are inappropriate for analyzing violent crime counts since they do not follow a normal distribution (King, 1988). This same assumption of normality is required in the use of Autoregressive Integrated

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7. The trend variable was created as a sequential time measure from the start to the end of the time series data (i.e. our data ran from January 2004 (1) to December 2010 (84). The trend-squared variable was simply the trend variable squared (trend variable $\times$ trend variable) to account for potential quadratic changes in a given time series.

Moving Average (ARIMA) time series analysis (Box & Jenkins, 1976).[8] Therefore, each specific violent crime outcome examined in the time series models was estimated using Maximum Likelihood (ML) estimation. The Poisson distribution is written as:

$$P(Y_i = y_i|x_i) = \frac{\exp(-\lambda)\lambda^{y_i}}{y_i!}, \tag{1}$$

where $Y_i$ is a random variable representing a violent crime count; $y_i$ is a particular count value that denotes the number of monthly events observed for a discrete time period; and where $\lambda_i$ represents different values in violent crime counts at distinct points (i.e. months) in time. Given our focus was to model the systematic variation observed for $\lambda_i$ for each outcome, we relied upon the log-linear model:

$$\ln(\lambda_i) = x_i^\tau \beta, \tag{2}$$

where $x_i^\tau \beta$ is a linear combination of predictors for each case (*i*). When estimating the interrupted time series models, this combination of measures included a post-intervention variable, linear and curvilinear trend measures, and monthly dummy variables (as a means to control for seasonality).

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 re-estimated each Poisson regression model by relying on the conditional negative binomial distribution due to the fact that the overdispersion in the models that were estimated could lead to biased statistical inferences (Hilbe, 2007; Osgood, 2000). In our study, the negative binominal results were virtually identical to those estimated from the standard Poisson regressions (see Appendix). According to Berk and MacDonald (2008), overdispersion in a count distribution is quite complex and that omitted variable bias is most likely the major driving influence that leads to this type of empirical distribution. Berk and MacDonald (2008, p. 283) specifically contend that turning automatically to the negative binomial regression can lead to a false sense of security since the fundamental errors remain in the model. In this case, the observed overdispersion is likely facilitated by a lack of empirical measurement of important time varying influences such as changes in citywide disadvantage and population structure that have been shown to correspond with changes in violent crime rates across US cities over time (see Land et al., 1990). 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 & Waterman, 2002). However, we note that statistical inferences in the subsequent analyses should be tempered accordingly.

8. We note that initial ARIMA models were performed on the lagged outcome measures (to more closely approximate a normal distribution) in an effort to assess whether serial autocorrelation plagued empirical models. Along with results from the Augmented Dickey Fuller Unit Root Test, there was no significant evidence of temporal autocorrelation and thus we relied upon the GLM estimates (see McCleary & Hay, 1980).

## Results

As an initial step, we estimated the changes in the average number of violent crime incidents for both the hypothesized treatment outcomes as well as the comparison outcomes. Table 3 indicates the average number of GMI homicides declined from 3.8 to 3.0 per month. Conversely, non-GMI homicides actually experienced a slight increase (i.e. from 2.5 to 2.6 per month), which demonstrates a slight divergence between GMI and non-GMI homicides during this period. Finally, the total number of violent firearm incidents across the city experienced a modest reduction from 36.6 offenses per month to 34.4 offenses per month. Comparatively, violent crime incidents that did not involve the use of a firearm also experienced a major substantive decrease during this same

**Table 3**   Bivariate monthly average in target and control outcomes

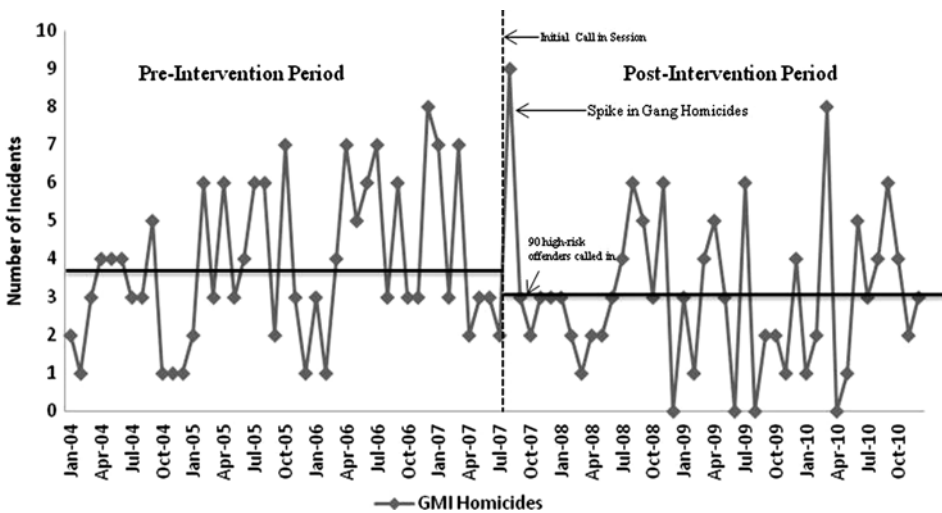| Violent offense | Number of offenses per month (pre-intervention) | Number of offenses per month (post-intervention) |
|---|---|---|
| *Treatment outcomes* | | |
| GMI homicides | 3.86 | 3.07 |
| Violent firearm incidents | 36.67 | 34.48 |
| *Comparison outcomes* | | |
| Non-GMI homicides | 2.50 | 2.60 |
| Non-shooting violent incidents | 214.79 | 152.29 |



**Figure 1**   GMI homicide trends.

time frame, which resulted in a decline from 214 non-shooting violent incidents per month to 152 incidents per month, on average.

Figure 1 shows the monthly number of GMI homicides between January 2004 and December 2010. We note that the "break" in the time series corresponds with the July 2007 intervention date (i.e. the first month during which the offender notification sessions occurred). The graph also illustrates that a spike in GMI homicides occurred one month after the onset of the initiative, which was followed by an immediate series of notification forums and a subsequent decline in this specific type of violence. The apparent reduction seemed to be relatively sustained for roughly one year.

Figure 2 shows the longitudinal pattern in violent firearm-related incidents during the same period. Similar to the graph displaying GMI homicides, violent firearm incidents experienced a stable increase between 2004 through 2006, with a subsequent reduction that was observed at different points between 2007 and 2008. Thus, the next step in the analysis was to examine whether these observed differences between the pre- and post-intervention periods materialized within a time series regression framework, controlling for other important factors.

## Time Series Regression Results

While the bivariate trend analyses were suggestive of a potential initiative impact, the estimated mean differences that have been examined to this point represent simple pre- and post-intervention reductions and do not control for general trends in the data, seasonality, and other confounding influences that are likely to create a regression toward the mean in the relevant crime outcomes.[9] In the following regression models, parameter estimates were expressed as incidence rate ratios (i.e. the change in the rate of an outcome based on a unit change in an independent variable), which are simply the exponentiated coefficients given the use of logarithmic transformation in ML estimation (Long & Freese, 2003).

Importantly, the tables display both a 24- and 42-month post-intervention period to examine whether potential intervention effects were sustained over an extended time period. However, since two models with distinct post-intervention periods were used to examine the potential initiative impact for each treatment and control outcome, the likelihood of observing at least one statis-

9. We also estimated the outcomes using ARIMA models (on the logged outcomes in an effort to more closely approximate a normal distribution) to assess whether first-order serial autocorrelation existed in the specified outcome measures. There was no evidence of a unit-root process (Augmented Dickey Fuller Unit Root Test was not statistically significant) and thus we did not need to control for serial autocorrelation.
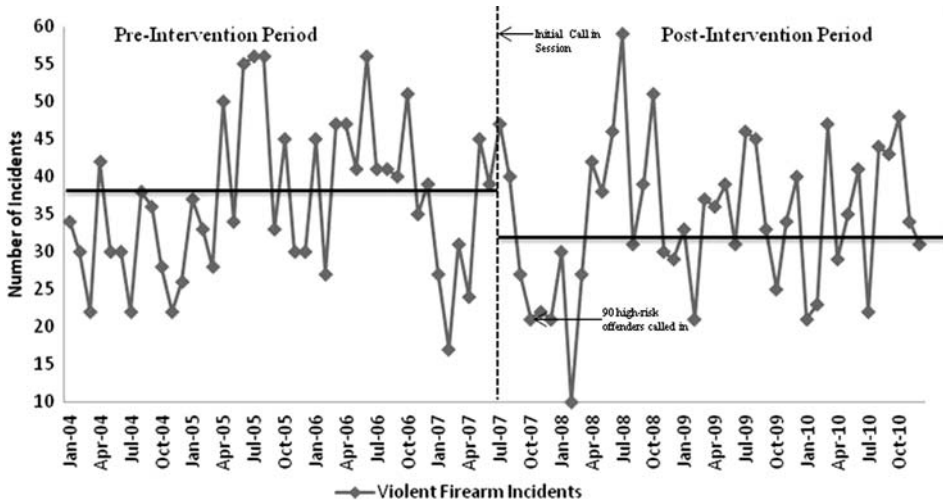
**Figure 2**   Violent firearm incident trends.

tically significant post-intervention effect increased nearly 9.8% (Shaffer, 1995).[10] Thus, we constrained the alpha levels using the Bonferroni correction where the actual *p*-value threshold was divided by the number of statistical tests ($\alpha/\eta$) in order to reduce the inflated error associated with multiple tests of the same underlying social process.[11]

Table 4 displays the pooled time series regression results designed to capture the impact assessment of the CIRV intervention strategy on GMI homicides while controlling for potential confounding influences in the time series data. More specifically, the post-intervention estimate can be interpreted as the mean change in GMI homicides between the pre- and post-intervention periods, centering on the date of the first call-in session in July 2007. The multivariate models indicated that specific summer months had higher levels of GMI homicides indicating consistent seasonality. After controlling for monthly seasonality and linear, as well as curvilinear trends in the data, we found that GMI homicides experienced a statistically significant decline ($\beta = -0.472$, SE = 0.275) of roughly 37.7% after 24 months. In addition, the statistically significant reduction in GMI homicides appear to be sustained after 42 months (i.e. 3.5 year follow-up period) given the incident rate ratio (IRR) of 0.586 ($p < .05$), which means GMI homicides were 58.6% in the post-intervention period what they were relative to the pre-intervention period, or a 41.4% reduction after 3.5 years post-implementation.

In terms of a comparison across outcomes, we examined whether similar changes were observed for non-GMI homicides between the pre- and post-intervention period after controlling trend and seasonal influences. The

10. This probability was derived from the following equation: $[1 - (1 - .05)^2] = 0.0975$, or roughly 9.8%.
11. Thus, the *p*-value associated with a 90% alpha level was as follows: ($.10/2 = p < .05$). Similarly, a 95% alpha threshold was observed as: ($.05/2 = p < .025$). For more details, see Shaffer (1995).

Table 4  Pooled time series homicide regression models

| Measure | Target outcome—GMI homicides | | | | | | Comparison outcome—non-GMI homicides | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 2 Year post-intervention | | | 3.5 Year post-intervention | | | 2 Year post-intervention | | | 3.5 Year post-intervention | | |
| | IRR | Coeff. | SE | IRR | Coeff. | SE | IRR | Coeff. | SE | IRR | Coeff. | SE |
| *Intercept* | – | 0.569 | 0.356 | – | 0.787 | 0.299 | – | 1.140 | 0.381 | – | 0.992 | 0.340 |
| Post-Intervention | 0.623* | −0.472 | 0.275 | 0.586** | −0.534 | 0.233 | 1.122 | 0.115 | 0.140 | 1.383 | 0.324 | 0.283 |
| *Controls* | | | | | | | | | | | | |
| Trend | 1.023 | 0.025 | 0.014 | 1.022* | 0.022 | 0.011 | 1.000* | 0.000 | 0.000 | 1.000 | 0.000 | 0.000 |
| Trend squared | 0.999 | −0.001 | 0.000 | 1.000* | 0.000 | 0.000 | 0.973 | −0.026 | 0.015 | 0.981 | −0.018 | 0.012 |
| January | 1.339 | 0.292 | 0.357 | 1.050 | 0.049 | 0.313 | 0.854 | −0.157 | 0.42 | 0.829 | −0.186 | 0.395 |
| February | 0.927 | −0.075 | 0.386 | 0.793 | −0.232 | 0.336 | 1.002 | 0.002 | 0.405 | 1.115 | 0.109 | 0.367 |
| March | 1.442 | 0.366 | 0.35 | 1.474 | 0.388 | 0.289 | 1.510 | 0.412 | 0.374 | 1.757* | 0.563 | 0.335 |
| April | 1.687 | 0.523 | 0.341 | 1.266 | 0.236 | 0.297 | 0.577 | −0.549 | 0.466 | 0.779 | −0.249 | 0.403 |
| May | 1.285 | 0.251 | 0.358 | 1.014 | 0.014 | 0.312 | 1.157 | 0.146 | 0.394 | 1.284 | 0.250 | 0.357 |
| June | 1.273 | 0.241 | 0.358 | 1.198 | 0.181 | 0.304 | 1.376 | 0.319 | 0.382 | 1.643 | 0.502 | 0.339 |
| July | 1.927* | 0.656 | 0.336 | 1.604* | 0.473 | 0.287 | 1.426 | 0.355 | 0.377 | 1.456 | 0.376 | 0.346 |
| August | 2.163** | 0.771 | 0.338 | 1.592 | 0.465 | 0.287 | 1.078 | 0.075 | 0.418 | 0.907 | −0.097 | 0.385 |
| September | 1.664 | 0.509 | 0.353 | 1.478 | 0.391 | 0.291 | 1.895** | 0.639 | 0.373 | 1.896** | 0.640 | 0.329 |
| October | 1.255 | 0.227 | 0.373 | 1.114 | 0.108 | 0.309 | 1.267 | 0.237 | 0.403 | 1.625 | 0.485 | 0.339 |
| November | 1.242 | 0.217 | 0.373 | 0.956 | −0.045 | 0.320 | 0.544 | −0.607 | 0.507 | 0.781 | −0.246 | 0.402 |
| *Model statistics* | | | | | | | | | | | | |
| Log likelihood | −130.39 | | | −167.23 | | | −114.63 | | | −144.70 | | |
| LR chi-square test (df) | 19.44 (14) | | | 21.02 (14) | | | 21.44 (14) | | | 32.12 (14) | | |

*Note.* IRR = incident rate ratio.

*$p$ < .10 (Bonferroni adjustment = .05).

**$p$ < .05 (Bonferroni adjustment = .025).

post-intervention coefficients in the non-GMI homicides models were quite divergent from GMI homicides in terms of direction, statistical significance, and estimated percentage changes ranging from a 12.2% non-significant increase after 24 months ($\beta = 0.115$, SE = 0.140) to a 38.3% non-significant increase after 42 months ($\beta = 0.324$, SE = 0.283). Thus, there is evidence that the significant reduction in GMI homicides occurred independent from changes in non-GMI homicides after the strategy unfolded.

Additional regression analyses revealed relatively stable statistically significant reductions in violent firearm incidents. Table 5 shows the estimates of firearm offenses in the post-intervention period relative to the baseline pre-intervention period, net of other time varying factors, after both 24 and 42 month post-intervention intervals. Violent firearm offenses experienced a statistically significant decline of roughly 22% after July 2007 in both follow-up periods. More specifically, the IRR of violent firearm incidents was 78.5 in the two year post-intervention period ($\beta = -0.241$, SE = 0.125) and was 78.0 in the three and a half year post-intervention period ($\beta = -0.247$, SE = 0.103). Thus, the magnitude (roughly 22%) of the statistically significant reduction in violent firearm offenses appeared to be continuous.

We also examined the change in violent non-firearm incidents, which declined substantially between 2004 and 2010. However, the magnitude of the overall decline in this control outcome appeared to be driven extensively by a general linear decline, as seen in Table 5. Once seasonal and trend controls were accounted for in the regression analyses, the post-intervention estimate that estimated the specific CIRV intervention effect was neither substantive (less than 5% of the variance explained) nor statistically significant in either the 24 month ($\beta = -0.010$, SE = 0.233) or 42 month follow-up period ($\beta = -0.049$, SE = 0.235).

The interrupted time series models demonstrated that targeted violent crime incidents in Cincinnati experienced a relatively stable decline of roughly 22-41% depending on the nature of the outcome measure and the post-intervention duration. These findings are consistent with results from prior focused deterrence evaluations (see Table 1). In addition, the post-intervention IRR was quite similar for GMI homicides, as well as violent firearm incidents in both the 24 and 42 month follow-up periods, indicating a potential sustained and long-term overall intervention effect. Given that the CIRV strategy relied specifically on social service provisions as a mechanism to achieve sustained violence reductions, we examined whether a relationship existed between the number of social service provisions extended to offenders with targeted outcome measures during the post-intervention period. Specifically, to further examine this sustained impact, we measured the total number of offenders that received CIRV-related social services (logged in order to reduce skewness) in a given month from July 2007 through December 2010. The log-linear equation reflects the association between the number of monthly social service pro-

**Table 5**  Pooled time series violent incident regression models

| Measure | Target outcome—violent firearm offenses | | | | | | Comparison outcome—non-shooting violent offenses | | | | | |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
| | 2 Year post-intervention | | | 3.5 Year post-intervention | | | 2 Year post-intervention | | | 3.5 Year post-intervention | | |
| | IRR | Coeff. | SE | IRR | Coeff. | SE | IRR | Coeff. | SE | IRR | Coeff. | SE |
| *Intercept* | – | 3.248 | 0.138 | – | 3.331 | 0.124 | – | 5.478 | 0.049 | – | 5.446 | 0.044 |
| Post-intervention | 0.785** | −0.241 | 0.125 | 0.780** | 0.247 | 0.103 | 0.980 | −0.010 | 0.233 | 0.951 | −0.049 | 0.235 |
| | | | | | | | | | | | | |
| *Controls* | | | | | | | | | | | | |
| Trend | 1.007 | 0.007 | 0.005 | 1.006 | 0.006 | 0.004 | 0.989** | −0.010 | 0.002 | 0.990** | −0.009 | 0.001 |
| Trend squared | 0.999 | −0.001 | 0.000 | 0.999 | −0.001 | 0.000 | 1.000 | 0.000 | 0.000 | 0.099 | −0.001 | 0.001 |
| January | 1.206 | 0.187 | 0.140 | 1.065 | 0.063 | 0.136 | 1.034 | 0.034 | 0.051 | 1.031 | 0.031 | 0.048 |
| February | 0.804 | −0.217 | 0.148 | 0.752* | −0.284 | 0.101 | 0.821** | −0.196 | 0.054 | 0.846** | −0.166 | 0.049 |
| March | 1.109 | 0.103 | 0.141 | 1.111 | 0.103 | 0.140 | 1.034 | 0.034 | 0.051 | 1.054 | 0.053 | 0.047 |
| April | 1.396** | 0.336 | 0.137 | 1.255* | 0.227 | 0.157 | 0.108 | 0.080 | 0.051 | 1.116** | 0.109 | 0.047 |
| May | 1.300* | 0.262 | 0.138 | 1.205 | 0.187 | 0.151 | 1.191** | 0.176 | 0.050 | 1.222** | 0.200 | 0.047 |
| June | 1.465** | 0.382 | 0.136 | 1.365** | 0.311 | 0.168 | 1.163** | 0.151 | 0.051 | 1.203** | 0.184 | 0.046 |
| July | 1.606** | 0.474 | 0.135 | 1.390** | 0.329 | 0.172 | 1.200** | 0.182 | 0.050 | 1.235** | 0.211 | 0.047 |
| August | 1.451** | 0.372 | 0.142 | 1.389** | 0.329 | 1.710 | 1.255** | 0.227 | 0.052 | 1.260** | 0.231 | 0.046 |
| September | 1.227 | 0.205 | 0.145 | 1.177 | 0.163 | 0.148 | 1.308** | 0.269 | 0.051 | 1.303** | 0.265 | 0.046 |
| October | 1.362** | 0.309 | 0.142 | 1.252 | 0.225 | 0.156 | 1.224** | 0.202 | 0.052 | 1.277** | 0.265 | 0.046 |
| November | 0.964 | −0.036 | 0.149 | 0.962 | −0.038 | 0.124 | 1.003 | 0.003 | 0.054 | 1.027 | 0.027 | 0.048 |
| | | | | | | | | | | | | |
| *Model statistics* | | | | | | | | | | | | |
| Log likelihood | −232.68 | | | −294.91 | | | −280.54 | | | −349.34 | | |
| LR chi-square test (*df*) | 40.13 (14) | | | 38.54 (14) | | | 127.93 (14) | | | 181.92 (14) | | |

*Note.* IRR = incident rate ratio.

*p < .10 (Bonferroni adjustment = .05).

**p < .05 (Bonferroni adjustment = .025).

**Table 6**  Independent estimates of the effects of social service provided to high-risk offenders (July 2007–December 2010)

| Measure | GMI homicides | | | Violent firearm offenses | | |
|---|---|---|---|---|---|---|
| | $\beta$ | SE | *p*-Value | $\beta$ | SE | *p*-Value |
| *Instantaneous effect* | | | | | | |
| CIRV services (Ln) | 0.077 | 0.198 | 0.696 | −0.014 | 0.084 | 0.866 |
| *One month lag* | | | | | | |
| CIRV services (Ln) | 0.070 | 0.189 | 0.709 | −0.090 | 0.087 | 0.301 |
| *Two month lag* | | | | | | |
| CIRV services (Ln) | −0.050 | 0.188 | 0.790 | −0.082 | 0.083 | 0.323 |
| *Six month lag* | | | | | | |
| CIRV services (Ln) | −0.203 | 0.201 | 0.310 | 0.051 | 0.079 | 0.520 |

*Note*. Monthly fixed-effects estimated but not displayed.

visions and either an instantaneous or a pre-specified lagged relationship with the targeted outcome measure(s).

Table 6 shows the results from a series (i.e. a total of eight separate models) of fixed-effects maximum likelihood regressions that relied on a pooled cross-section time series where the monthly violent crime counts between July 2007 and December 2010 were treated as distinct units of analysis, and a dummy variable for each monthly violent crime incident was present as a means to remove omitted variable bias given the additive effects of unmeasured factors (Allison & Waterman, 2002). Results revealed that there was no evidence of an instantaneous effect of social service on GMI homicides ($\beta = 0.077$, SE $= 0.198$, *t* ratio $= 0.38$) or on monthly violent firearm incidents ($\beta = -0.014$, SE $= 0.084$, *t* ratio $= 0.16$) in the post-CIRV intervention period. In addition, a number of lagged social services effects were specifically estimated (at one, two, and six months) to account for a potential delayed effect, and again there was no discernable relationship between social services provided to offenders and specific changes in targeted violent crime outcomes that had a sustained decline after the CIRV strategy unfolded.

## Discussion

Our research contributes to the growing body of focused deterrence evaluations by examining an in-city comparison group of outcomes; a prolonged post-intervention period to address potential long-term impact; and the aggregate impact of the social services component. Our evaluation demonstrates a significant reduction in group-member involved homicides in Cincinnati following the implementation of CIRV. This reduction, which was not observed in

non-GMI homicides, was significant even when controlling for potential con-founding influences. To a lesser degree, the implementation of CIRV was also associated with a decline in violent firearm incidents. Collectively, these results are consistent with the growing body of evidence that demonstrates significant reductions in violence following the implementation of focused deterrence policing strategies (Braga, 2008; Braga et al., 2001, 2008; Corsaro & McGarrell, 2009; McGarrell et al., 2006; Papachristos et al., 2007). These results also demonstrate sustainability, as GMI homicides experienced a statis-tically significant decline of 37.7% after 24 months, and a 41.4% reduction 42 months post-implementation.

The CIRV Team was initially concerned about the viability of long-term vio-lence reductions based on prior focused deterrence initiatives, and therefore focused heavily on implementation, model fidelity, and institutionalizing a process for sustainability. It was reasoned that focused deterrence approaches likely produced only short-term violence reductions due to one of two rea-sons. Either jurisdictions failed to continually implement their models over time, or focused deterrence initiatives (comprised primarily of law enforce-ment activities) only provide short-term suppression effects due to limitations in the model. First, it is possible that other jurisdictions (e.g. Boston) were unable to sustain their efforts because implementation ceased as new individ-uals entered leadership positions in key partner agencies. Given this possibil-ity, CIRV included the development of an organizational structure to withstand personnel changes in key leadership positions. After four years, this organizational structure has provided support for continued work in Cincinnati despite leadership turnover in every key partner agency and reductions in funding. Cincinnati continues to "do the work," and the CIRV Team remains operational and innovative in its approaches to reduce violence. Our results suggest that focused deterrence approaches can have stability over time if implemented properly and the organizational processes are institutionalized. Including business executives from Procter & Gamble Co. in the early planning phases of CIRV allowed the initiative to benefit from the lessons learned in the private sector. The strategic planning phase of new focused deterrence interventions should include a sustainability plan to address institutionaliza-tion and accountability challenges that inevitably arise with turnover in key leadership positions.

The second possibility is that, by their very nature, focused deterrence strat-egies cannot have a sustained impact on aggregated levels of violence without additional non-law enforcement tactics. Focused deterrence initiatives typically focus primarily on the "pulling levers" aspect, using law enforcement-based tactics designed to create actual (or perceived) changes in the criminal justice system's handling of chronic violent offenders. But when law enforcement's *new* response to gang violence quickly becomes routine, do offenders revert back to old behaviors? Many law enforcement efforts typically demonstrate short-term success, but long-term change remains elusive (Braga, 2005; Koper & Mayo-Wilson, 2006; Rosenbaum, 2007; Sherman, 1990). The CIRV team recog-

nized this possibility at the onset and reasoned that long-term change required investments in alternatives to violence, including the provision of meaningful social services and employment opportunities to would-be offenders. Nevertheless, our analyses suggest that the CIRV social service component likely did not contribute directly to the longevity in violence reductions.

It is possible that despite genuine efforts, the fidelity of the social service model was never fully achieved. As previously noted, although 622 offenders received some type of CIRV-sponsored social services during the 42-month period evaluated, only 55 clients (8.8%) actively engaged in the enhanced services program designed to address criminogenic needs. In addition, only 138 (22.2%) of these 622 individuals were identified by law enforcement as being associated with violent groups; the remainder were identified by CIRV Street Advocates (but not law enforcement) as being high-risk for engaging in violence. Street Advocates' violence assessments were conducted informally, on an ad hoc basis and without training, until empirically validated violence screening tools were introduced in January 2009 (18 months into the intervention period). As a result, it is likely that a substantial proportion of individuals who self-selected into the CIRV services component were not actively engaged in violence, and therefore provision of services to these individuals was unlikely to impact overall citywide trends in violence. A more fine-tuned analysis is needed to ascertain whether the social service component is promising when the appropriate population is targeted for services. Individual-level analyses that match gang/group members based on risk level and exposure to other CIRV-related activities are needed to determine whether this component of the strategy has the potential to reduce violence on a larger scale.

Furthermore, it is important to note that our analysis only examined the direct effect of the number of individuals engaged in social services on the violent outcomes of interest. The benefits of social service provisions to focused deterrence initiatives may actually be less tangible, but still substantively important. Examples include enhancing the legitimacy of law enforcement and the initiative in the community, which is necessary for engaging community partners and sustaining the initiative, as well as invalidating excuses for violence (Tillyer & Kennedy, 2008).

In summary, based on the reported sustained reduction in gang-related violence over the 42-month period examined, coupled with the null findings associated with the provision of social services, it appears likely that CIRV's organizational structure, rather than its social service component, is responsible for CIRV's prolonged success. The organizational structure articulated decision-making processes and division of labor, defining roles and responsibilities in terms of skills and resources, rather than specific individuals (Tillyer et al., 2010). This has allowed CIRV to remain operational, despite substantial turnover in CIRV Team membership. It is the continued implementation of the model that appears to account for the sustained reductions in violence in Cincinnati.

More broadly, however, our research raises important questions about focused deterrence initiatives. Here, it is useful to return to our earlier discussion of policy relevance and external validity (Eck, 2010). In our attempts to enhance the ability to judge the external validity of our study and improve its utility, we provided detailed descriptions related to the units, treatments, outcomes, and setting of CIRV. Differences in these dimensions of external validity likely contribute to the variation in success across sites. Where our evaluation comes up short——as do all focused deterrence studies to date——is in determining the specific mechanisms or processes by which CIRV reduces violence. While improved methodological techniques used across studies have provided more confidence in the assertion that focused deterrence initiatives reduce violence, only limited progress has been made in empirically establishing *why* these initiatives work. Research on such programs has been limited to within- and between-city pre-post comparisons, with the intervention measured as a dichotomous variable. Though informative in terms of the efficacy of the model, such methodological strategies have only begun to unpack why these initiatives are associated with reductions in violence. Questions remain in terms of the activities and dosages required to produce the desired outcome.

Recent theoretical work (Tillyer & Kennedy, 2008; Tillyer et al., 2010) has proposed numerous explanations consistent with existing criminological theory and research. Potential mechanisms include changing the rational calculus of offenders by increasing the certainty and severity of formal punishment or informal sanctions, such as ostracism by one's peers (Tillyer & Kennedy, 2008); addressing criminogenic needs through the availability of social services (Tillyer et al., 2010); increasing the legitimacy of the police (Tyler, 1990); and enhancing collective efficacy through community engagement (Sampson et al., 1997). In short, criminology offers a myriad of explanations for a relationship between these initiatives and violence reduction. To date, the underlying mechanisms and intervening processes set in motion by a focused deterrence initiative have received limited study. We propose the next step in focused deterrence research is to empirically examine potential mediating factors across multiple units of analysis (e.g. changes in perceived risk of punishment among individual offenders and dynamic macro-level influences linked to aggregate rates of violence). In addition to the theoretical implications, identifying the underlying mechanism has important policy implications for the dozens of cities that implement violence reduction initiatives grounded in focused deterrence principles. The results of our study provide a first step, demonstrating that provision of social services to would-be offenders did not result in any measurable direct impact on citywide violence trends.

Our study has additional limitations that should be noted. First, comparisons in outcome data for similar (i.e. balanced) large urban cities may improve the analyses and make this study more consistent with a rigorous quasi-experimental design and strengthen study validity (Cook & Campbell, 1979). However,

the specific dependent variables examined here (i.e. gang-member involved homicides and shootings) were selected because they were the primary focus of the initiative (see Engel et al., 2008). Suitable comparison data on these outcomes were: (1) not readily available; (2) available in cities that implemented similar initiatives; and/or (3) suffered extreme lags in data collection and processing.[12] As a result, we focused on strengthening the pre/post design to include a lengthy baseline and follow-up time series, and to control for relevant changes in trends, seasonal effects, and changes in other types of violence during the same period. Nevertheless, a more comprehensive quasi-experimental design would be preferred.

A second limitation of our study is that the GLM time series models used assume a one-group pre/post effect. In essence, this initial impact assessment does not test the specific rate of change in gang and gun violence that corresponded with the unique CIRV strategies (i.e. number of arrests, notification sessions, community and service provisions, etc.) across different time periods (i.e. weekly, monthly, lagged, and lead effects). This is true of most evaluation research that has generally found focused deterrence initiatives to be associated with reductions in violence. Despite these concerns, the current study provides an opportunity to make key contributions to the current state of scholarship within the focused deterrence, proactive policing, and evidence-based frameworks.

In conclusion, the present study suggests that focused deterrence initiatives *can* produce sustained reductions in violence over time. CIRV's organizational structure has proven important in several ways. Not only has it facilitated the ongoing implementation of the initiative, but it has ensured the ongoing data collection of all CIRV-related activities by holding all partners accountable. This has allowed us to enhance the external validity of our study by providing the "policy details," or the specific activities and dosages of CIRV. To that end, we have been able to take the first step in empirically examining one potential component——social service provisions——that might be responsible for the reductions in violence associated with such initiatives. Future focused deterrence research should move beyond simple dichotomous measures of program implementation to begin examining the various mechanisms that might account for the success in violence reduction.

12. We examined gang and firearm violence data from the Supplementary Homicide Reports (SHR) and the National Incident Based Reporting System (NIBRS). We found that only 56 of the 253 US cities (22.1%) with a population of 100,000 or more reported their data to the NIBRS system. Also, 2008 was currently the most recent update of the data available by NIBRS, which provides a limited post-intervention period. SHR data is available for over 90% of large US cities (also limited to 2008 follow-up), but are problematic (i.e. inconsistent) in terms of measurement of gang homicides (Decker & Pyrooz, 2010). While an examination of gun homicides would strengthen the design across cities, the emphasis of CIRV was not gun homicide specific. In addition, the Project Safe Neighborhoods (PSN) program was also in effect in large urban cities nationally during this period (McGarrell, Corsaro, Hipple, & Bynum, 2010).

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## Appendix: Negative Binomial Estimates

| Outcome[a] | 2 Year post-intervention | | | 3.5 Year post-intervention | | |
|---|---|---|---|---|---|---|
| | IRR | Coefficient | SE | IRR | Coefficient | SE |
| Target——GMI homicides | 0.623[*] | −0.472 | 0.281 | 0.586[**] | −0.534 | 0.233 |
| Comparison——non-GMI homicides | 1.122 | 0.115 | 0.350 | 1.383 | 0.324 | 0.284 |
| Target——violent firearm offenses | 0.785[**] | −0.241 | 0.125 | 0.780[**] | −0.247 | 0.103 |
| Comparison——non-shooting violent offenses | 0.980 | −0.010 | 0.247 | 0.951 | −0.049 | 0.283 |

*Note.* IRR = incident rate ratio.

[a]Monthly fixed effects dummy variables as well as trend measures estimated but not included in display.

*$p < .10$ (Bonferroni adjustment = .05).

**$p < .05$ (Bonferroni adjustment = .025).