

# Focused Deterrence Strategies and Crime Control

## An Updated Systematic Review and Meta-Analysis of the Empirical Evidence

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### Research Summary

*Focused deterrence strategies are increasingly being applied to prevent and control gang and group-involved violence, overt drug markets, and individual repeat offenders. Our updated examination of the effects of focused deterrence strategies on crime followed the systematic review protocols and conventions of the Campbell Collaboration. Twenty-four quasi-experimental evaluations were identified in this systematic review. The results of our meta-analysis demonstrate that focused deterrence strategies are associated with an overall statistically significant, moderate crime reduction effect. Nevertheless, program effect sizes varied by program type and were smaller for evaluations with more rigorous research designs.*

### Policy Implications

*The available empirical evidence suggests these strategies generate noteworthy crime reduction impacts and should be part of a broader portfolio of crime reduction strategies available to policy makers and practitioners. Investments still need to be made, however,*

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*to strengthen the overall rigor of program evaluations and improve our understanding of key program activities associated with observed crime reduction impacts.*

### **Keywords**

*Deterrence, problem-oriented policing, gang violence, repeat offenders, drug markets*

Focused deterrence strategies, also known as “pulling levers” policing programs, have been increasingly implemented in the United States and other countries to reduce serious violent crime committed by gangs and other criminally active groups, recurring offending by highly active individual offenders, and crime and disorder problems generated by overt street-level drug markets. These strategies are framed by an action research model that is common to both problem-oriented policing and public health interventions to reduce violence (Braga and Weisburd, 2015). Briefly, the aim of focused deterrence strategies is to change offender behavior by understanding underlying crime-producing dynamics and conditions that sustain recurring crime problems and by implementing an appropriately focused blended strategy of law enforcement, community mobilization, and social service actions (Kennedy, 2008, 2011). Direct communications of increased enforcement risks and the availability of social service assistance to target groups and individuals are defining characteristics of focused deterrence strategies.

The focused deterrence approach was pioneered as the “Operation Ceasefire” intervention in Boston, Massachusetts, to address an epidemic of gang homicide in the early-to-mid 1990s (Braga, Kennedy, Waring, and Piehl, 2001; Kennedy, Piehl, and Braga, 1996) and then was eventually tested in other jurisdictions (e.g., McGarrell, Chermak, Wilson, and Corsaro, 2006; Papachristos, Meares, and Fagan, 2007; Tita et al., 2004). Although the goal of focused deterrence strategies is to prevent crime by changing offender perceptions of sanction risk, other complementary crime prevention mechanisms seem to support the crime control efficacy of these programs (Braga and Kennedy, 2012; Kennedy, Kleiman, and Braga, 2017). These strategies are also intended to change offender behavior by mobilizing community action, enhancing procedural justice, and improving police legitimacy. To some observers, focused deterrence strategies hold great promise in reducing serious violence while improving strained relationships between minority neighborhoods and the police departments that serve them (Brunson, 2015; Meares, 2009).

In a now-dated Campbell Collaboration systematic review, ten quasi-experimental evaluations of the crime control impacts of focused deterrence programs were identified based on a search for eligible studies completed in 2010 (Braga and Weisburd, 2012a, 2012b). In that Campbell review, researchers found that focused deterrence strategies were associated with significant reductions in targeted crime problems. Although the authors concluded that the available evidence was highly supportive of crime reduction impacts (Braga and Weisburd, 2012a, 2012b), they also noted the absence of

randomized experiments and the fact that, in several of the included evaluations, weaker designs were used with nonequivalent comparisons. By drawing on the results of the original Campbell review and a growing body of evaluation evidence, Professor Kenneth Land (2015: 515) concluded that focused deterrence programs “work” in violent crime control and that policy makers should “let the focused deterrence and pulling levers programs roll with eternal vigilance.”

Recently, more cities have tested the focused deterrence approach to control gang violence, disorderly drug markets, and repeat offender problems. The National Network for Safe Communities, an applied research project of the John Jay College of Criminal Justice, provides support to some 84 U.S. cities who are implementing some version of a focused deterrence strategy.<sup>1</sup> A few other countries have started to test the approach. For instance, a focused deterrence program has been implemented targeting youth violence in Glasgow, Scotland (Deuchar, 2013). Police executives and other public officials in Eastern European and South American countries, such as Turkey and Brazil, have also explored the possibility of implementing focused deterrence strategies to control gang and group-related violence in their cities (National Network for Safe Communities, 2013).

The small number of studies and the preponderance of weaker evaluation designs, however, contribute to some healthy ongoing skepticism regarding the crime control benefits associated with focused deterrence programs among practitioners and crime policy scholars. Former New York City Mayor Rudy Giuliani criticized the “Boston Model” as not leading to lasting crime control gains in his 2001 farewell address (The New York Times Staff, 2001). In an article published in *The New Yorker*, well-respected deterrence scholar Professor Franklin Zimring is quoted as lamenting the lack of rigorous evaluations of focused deterrence programs and, when assessing the Boston experience, suggested, “Ceasefire is more of a theory of treatment rather than a proven strategy” (Seabrook, 2009: 37). Other criminologists seem unaware of the existing empirical evidence. For instance, in his 2013 summary of the crime prevention value of focused deterrence programs, former National Council on Crime and Delinquency president Barry Krisberg reported, “It certainly hasn’t been effective so far, and there is no information suggesting it is effective” (as interviewed by KTVU, 2013).

Given the growing popularity of focused deterrence programs and conflicting scholarly views on the crime reduction value associated with the approach, ongoing systematic review of rigorous program evaluations is necessary to keep policy and practice debates rooted in the most up-to-date and comprehensive scientific evidence. In this article, we present the findings of an updated Campbell Collaboration systematic review of 24 eligible program evaluations in which the effects of focused deterrence on crime are measured. In this updated review, more than twice as many eligible studies are considered when compared with its

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1. For a complete list of cities supported by the National Network for Safe Communities, go to [nnscommunities.org/impact/cities](http://nnscommunities.org/impact/cities) (last accessed August 30, 2017).

predecessor. Although the call for randomized controlled trials remains unanswered, the available empirical evidence base now comprises a greater share of more rigorous quasi-experimental evaluations in which matched comparison groups are used.

We begin by briefly describing three different types of focused deterrence strategies in the existing literature and how these strategies have been located within deterrence theory and other theoretical perspectives on crime control. We then describe the methods of our Campbell Collaboration systematic review and the results of our synthesis of the available empirical evidence. The available empirical evidence suggests these strategies generate noteworthy violence reduction impacts and should be part of a broader portfolio of crime reduction strategies available to policy makers and practitioners. But the effects of focused deterrence strategies vary by the target of the interventions. The strongest crime reduction impacts are generated by strategies designed to reduce serious violence by gangs and criminally active groups. Strategies designed to control continued criminal behavior by repeat offenders and to reduce crime and disorder associated with overt street drug markets generate much more modest crime prevention effects.

### **Focused Deterrence Strategies**

The focused deterrence approach is consistent with recent theorizing about police innovation, which suggests approaches that seek to both create more focus in application of crime prevention programs and expand the tools of policing that are likely to be most successful in controlling crime (Weisburd and Eck, 2004). Focused deterrence interventions are aimed at influencing the criminal behavior of individuals through the strategic application of enforcement and social service resources to facilitate desirable behaviors. These strategies are often framed as problem-oriented exercises where specific recurring crime problems are analyzed and responses are highly customized to local conditions and operational capacities. Kennedy (2006: 156–157) detailed the following key features of focused deterrence strategies:

- Selection of a particular crime problem, such as youth homicide or street drug dealing.
- Forming an interagency enforcement group, which often includes local police, probation, parole, state and federal prosecutors, and federal law enforcement agencies.
- Conducting research, usually by drawing heavily on knowledge from front-line enforcement personnel, to identify key offenders or groups of offenders and the context of their criminal behavior.
- Developing a special enforcement strategy to direct at identified key offenders or groups of offenders and influence the context of their offending by using any and all legal tools (known as “pulling levers”) available to sanction the targeted population.
- Matching enforcement actions with parallel efforts to direct social services and the moral voices of communities negatively affected by the targeted criminal behavior to those key offenders or groups of offenders.

- Communicating directly and repeatedly with the targeted criminal population to inform them of the heightened scrutiny they are being subjected to, what acts or “triggering events” (such as shootings) will get special attention, what increased enforcement and sanctions will follow, and what they can do to avoid increased attention. This message is often disseminated during a “forum,” “offender notification meeting,” or “call-in,” in which offenders are invited or directed (usually because they are on probation or parole) to attend these face-to-face meetings with law enforcement, social service providers, and representatives from the community.

In the earlier Campbell review, three basic kinds of focused deterrence programs were identified (Braga and Weisburd, 2012a, 2012b). The first type draws on the model of the Boston Operation Ceasefire experience during the 1990s (see Braga et al., 2001; Kennedy et al., 1996). This approach is focused on *gang and criminally active group violence reduction strategies*. It joins criminal justice agencies, social service organizations, and community members to engage directly with violent groups, communicate credible moral and law enforcement messages against violence clearly, make genuine offers of help for those who want it, and launch strategic enforcement campaigns against those who continue their violent behavior.

A second type of focused deterrence strategy is intended to reduce crime driven by street-level drug markets and is generally called a “drug market intervention” (DMI) program. DMI-focused deterrence strategies are used to identify street-level dealers, immediately apprehend violent drug offenders, and suspend criminal cases for nonviolent dealers (Kennedy, 2008). DMI strategies then bring together nonviolent drug dealers, their families, law enforcement and criminal justice officials, service providers, and community leaders for a meeting that communicates directly to offenders that their drug dealing has to stop, the community cares for them but rejects their conduct, services and job opportunities are available, and renewed dealing will result in the activation of the existing case (Kennedy and Wong, 2009). Finally, some focused deterrence programs are aimed at preventing repeat offending by *high-risk individuals*. In general, these strategies are used to address the most dangerous offenders with a wide range of legal tools, put offenders on formal notice that their “next offense” will bring extraordinary legal attention, and focus community “moral voices” on such offenders to set a clear standard that violence is unacceptable (Deuchar, 2013; Kennedy, 2008; Papachristos et al., 2007).

### **Theoretical Perspectives Supporting Focused Deterrence**

We think it important to focus on the theoretical mechanisms underlying focused deterrence at the outset. There is ample skepticism in the literature regarding “person-focused” approaches in policing (Weisburd, 2008). Such skepticism is rooted in evaluations of the standard model of policing dominant in the last century (National Research Council, 2004). In the standard model, the police focused on investigating and apprehending

offenders. But the results of studies examining the crime prevention effects of strategies such as rapid response to calls for service (e.g., see Spelman and Brown, 1984), and investigations of crime after its occurrence (e.g., see Eck, 2002), led scholars to conclude that generalized person-focused approaches were ineffective (National Research Council, 2004; Sherman et al., 1997; Telep and Weisburd, 2012; Weisburd and Eck, 2004). Even in the case of more focused interventions directed at individuals, and including focused deterrence policing, Weisburd and Eck (2004: 53) concluded that the evidence for effectiveness was “weak.”

Theory is important to provide a strong logic model for effectiveness, especially when drawing a conclusion on the basis of nonexperimental evidence of program impacts. The strong theoretical model for the effectiveness of focused deterrence adds weight to the empirical evidence that we present in this article. Even though focused deterrence programs vary, they share common prevention mechanisms that are believed to influence crime. Although we do not evaluate these mechanisms directly, findings from evaluations of the associated programs provide insight into the effectiveness of these prevention mechanisms, which in turn yields knowledge that can aid in designing effective programs (Ludwig, Kling, and Mullainathan, 2011).

### *Deterrence*

Deterrence theory suggests that crime can be prevented when the costs of committing the crime are perceived by the offender to outweigh the benefits (Gibbs, 1975; Zimring and Hawkins, 1973). Most discussions of the deterrence mechanism distinguish between “general” and “special” deterrence (Cook, 1980). General deterrence is the idea that the general population is dissuaded from committing crime when it sees that punishment necessarily follows the commission of a crime. Special deterrence involves punishment administered to criminals with the intent to discourage them from committing crimes in the future. Much of the literature evaluating deterrence has been focused on the effect of changing certainty, swiftness, and severity of punishment associated with certain acts on the prevalence of those crimes (Apel and Nagin, 2011; Nagin, 1998; Paternoster, 1987).

In addition to any increases in certainty, swiftness, and severity of sanctions associated with gun violence, focused deterrence strategies are intended to prevent crime through the advertising of the law enforcement strategy and the personalized nature of its application. The effective operation of general deterrence is dependent on the communication of punishment threats to relevant audiences. As Zimring and Hawkins (1973: 142) observed, “the deterrence threat may best be viewed as a form of advertising.” A key element of focused deterrence strategies involves the delivery of a direct and explicit “retail deterrence” message to a small target audience regarding what kind of behavior would provoke a special response and what that response would be. For instance, beyond the particular groups subjected to gang violence reduction interventions, the deterrence message was applied to a smaller

specific audience (e.g., all gang-involved youth in a particular city) rather than to a larger general audience, and it was operated by making explicit cause-and-effect connections between the behavior of the target population and the behavior of the authorities. Knowledge of what happened to others in the target population was intended to prevent further acts of violence by gangs in the jurisdiction.

The results of available research reveal that deterrent effects are ultimately determined by offender perceptions of sanction risk and certainty (Nagin, 1998). Durlauf and Nagin (2011: 40) observed that, “[S]trategies that result in large and visible shifts in apprehension risk are most likely to have deterrent effects that are large enough not only to reduce crime but also apprehensions,” and they identified focused deterrence strategies as having these characteristics. As described earlier, focused deterrence strategies are targeted on specific behaviors by a small number of chronic offenders who are highly vulnerable to criminal justice sanctions. The approach 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. Face-to-face meetings with offenders are an important first step in altering their perceptions about sanction risk (Horney and Marshall, 1992; Nagin, 1998). As McGarrell et al. (2006) suggested, direct communications and affirmative follow-up responses are the types of new information that may cause offenders to reassess the risks of continuing their criminal behavior.

In focused deterrence strategies, deterrent messages are framed to address the group context from which many crime problems emerge. The groups themselves can act as another internal communication vehicle for transmitting the actual sanction risk to other offenders. Sanctions for individual noncompliance are applied to groups; all communications to offenders focus on this group concept, with the thought that peer pressure will change individual and group behavior. As Braga and Kennedy (2012) described, meaningful enforcement actions and scrutiny by law enforcement agencies can leverage the rationality of group members to no longer encourage norms that provoke the outbreaks of violence. The citywide communication of the anti-violence message, coupled with meaningful examples of the consequences that will be brought to bear on groups that break the rules, can weaken or eliminate the “kill or be killed” norm as individuals recognize that their enemies will be operating under the new rules as well.

Changes in group norms and in objective risks associated with particular forms of misbehavior may, for example, make it more difficult to recruit peers for particular instances of co-offending. Ethnographic research findings on illicit gun markets in Chicago have shown that gangs’ assessment of the law enforcement responses to gun violence leads them to withhold access to firearms for younger and more impulsive members (Cook, Ludwig, Venkatesh, and Braga, 2007). DMI’s goal of fundamentally disrupting overt drug markets can greatly enhance the difficulty of drug dealing: when buyers no longer routinely “cruise” once-active markets, even a motivated street dealer may find it impossible to do business.

*Other Theoretical Perspectives*

Many scholars have suggested there are other complementary violence reduction mechanisms at work in the focused deterrence strategies described here that need to be highlighted and better understood (Braga, 2012; Brunson, 2015; Corsaro and Engel, 2015). In Durlauf and Nagin's (2011) article, their focus is on the possibilities for increasing perceived risk and deterrence by increasing police presence. Nevertheless, in the focused deterrence approach, the emphasis is not only on increasing the risks associated with offending, but it is also on decreasing opportunity structures for crime, deflecting offenders away from crime, increasing the collective efficacy of communities, and increasing the legitimacy of police actions. Indeed, program designers and implementers sought to generate large crime reduction impacts from the multifaceted ways in which this strategy influences targeted offenders (Kennedy, 2011).

Discouragement emphasizes reducing the opportunities for crime and increasing alternative opportunity structures for offenders (Clarke, 1997). In this context, situational crime prevention techniques are often implemented as part of the core pulling levers work in focused deterrence strategies (Braga and Kennedy, 2012). Extending guardianship, assisting natural surveillance, strengthening formal surveillance, reducing the anonymity of offenders, and using place managers can greatly enhance the range and the quality of the varying enforcement and regulatory levers that can be pulled on offending groups and key actors in criminal networks. The focused deterrence approach also is aimed at redirecting offenders away from crime through the provision of social services and opportunities. Treated individuals are offered job training, employment, substance abuse treatment, housing assistance, and a variety of other services and opportunities.

Sampson, Raudenbush, and Earls (1997) emphasized the capacity of a community to realize common values and regulate behavior within it through cohesive relationships and mutual trust among residents. They argued that the key factor determining whether crime will flourish is a sense of the "collective efficacy" of a community. A community with strong collective efficacy is characterized as having high capacities for collective action for the public good. The use of focused deterrence strategies enhances collective efficacy in communities by emphasizing the importance of engaging and enlisting community members in the strategies developed. Implementation of the High Point DMI strategy, for example, drew on collective efficacy principles by engaging family, friends, and other "influential" community members in addressing the criminal behaviors of local drug dealers (Kennedy and Wong, 2009).

Community-based action in focused deterrence strategies helps remove the justifications used by offenders to explain away their responsibility for the targeted behavior. In call-ins and on the street, community members effectively invalidate the excuses for criminal behavior by challenging the norms and narratives that point to racism, poverty, injustice, and the like. In Boston, for example, Black clergy challenged gang members who attempted



to use these excuses by countering that poverty, racism, and injustice were not linked to their decisions to fire shots in their neighborhoods and kill other young people who have experienced the same societal ills and life difficulties (Braga et al., 2001). Community members also work with law enforcement and social service agencies to (a) set basic rules for group-involved offenders such as “don’t shoot guns” and (b) alert the conscience of these offenders by appealing to moral values inherent in taking the life of another, causing harm to their neighborhood, or the pain that would be experienced by their mothers if they were killed or sent to prison for a long time in a far-away location (Kennedy, 2011).

Finally, use of the focused deterrence approach takes advantage of recent theorizing regarding procedural justice and legitimacy. The effectiveness of policing is dependent on public perceptions of the legitimacy of police actions (Tyler, 2004). Legitimacy is the public belief that there is a responsibility and obligation to accept and defer voluntarily to the decisions made by authorities (Tyler, 2006 [1990]). Findings from recent studies reveal that when procedural justice approaches are used by the police, citizens will not only evaluate the legitimacy of the police more highly, but they will also be more likely to obey the law in the future (Paternoster, Brame, Bachman, and Sherman, 1997; but see Nagin and Telep, 2017). Advocates of focused deterrence strategies argue that targeted offenders should be treated with respect and dignity (Kennedy, 2008, 2011), reflecting procedural justice principles. The Chicago Project Safe Neighborhood (PSN) strategy, for instance, was aimed at increasing the likelihood that the offenders would “buy in” and comply voluntarily with the pro-social, anti-violence norms being advocated by interacting with offenders in ways that enhance procedural justice in their communication sessions (Papachristos et al., 2007).

## Method

Our examination of the effects of focused deterrence strategies on crime followed the systematic review protocols and conventions of the Campbell Collaboration. Meta-analysis is a statistical method designed to synthesize empirical relationships across studies in a systematic review, such as the effects of a specific crime prevention intervention approach on criminal offending behavior (Wilson, 2001). Specialized statistical methods are applied in meta-analysis to analyze the relationships between findings and study features (Lipsey and Wilson, 1993; Wilson, 2001). The “effect size statistic” is the index used to represent the findings of each study in the overall meta-analysis of study findings and represents the strength and direction (positive or negative) of the relationship observed in a particular study (e.g., the size of the treatment effect found). The “mean effect size” represents the average effect of treatment on the outcome of interest across all eligible studies in a particular area, and it is estimated by calculating a mean that is weighted by the precision of the effect size for each individual study.

### *Criteria for Inclusion and Exclusion of Studies in the Review*

To be eligible for this review, interventions had to include the key components of a focused deterrence strategy as described earlier.<sup>2</sup> Randomized experimental and quasi-experimental (nonrandomized) designs that compared pre- and post-intervention measures were eligible for inclusion in this review, although we did not identify any randomized experiments in our search (Campbell and Stanley, 1963; Shadish, Cook, and Campbell, 2002). Eligible quasi-experiments used a comparison group or one-group-only interrupted time-series design that controlled for extraneous factors to analyze variations in crime trends pre- and post-intervention.<sup>3</sup> The units of analysis could be areas, such as cities, neighborhoods, or police beats, or individuals. Eligible studies had to include measurement of the effects of the focused deterrence intervention on officially recorded levels of crime at places or crime by individuals. Appropriate crime measures comprised crime incident reports, citizen emergency calls for service, and arrest data. Particular attention was paid to studies that included measurement of crime displacement effects and diffusion of crime control benefit effects (Clarke and Weisburd, 1994; Reppetto, 1976). All forms of displacement and diffusion reported by the studies were considered in the review.

### *Search Strategies for Identification of Studies*

Several strategies were used to perform an exhaustive search for literature fitting the eligibility criteria. First, a keyword search<sup>4</sup> was performed on 15 online abstract databases.<sup>5</sup> Second, we reviewed the bibliographies of past narrative and empirical reviews of literature in which

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2. Several scholars contacted during our original search for eligible studies suggested that the Hawaii Opportunity with Probation Enforcement (HOPE) randomized experiment (Hawken and Kleiman, 2009) fit within the general framework of focused deterrence strategies. We initially agreed that it was broadly similar to the Chicago PSN program included here, as both were focused on corrections populations. As such, HOPE appeared in the article version of our original review (Braga and Weisburd, 2012b). Nevertheless, after further consideration and following the recommendations of Campbell Collaboration peer reviewers, we eventually agreed that HOPE did not include the full range of program elements defined by our selection criteria (Kennedy, 2006: 156–157). As such, HOPE was not officially included in the final version of our Campbell review (Braga and Weisburd, 2012a). The HOPE evaluation and existing replication studies were also not included in this updated review.
  3. The previous iteration of this systematic review did not include studies with one-group-only interrupted time-series design designs. As will be shown, the updated review only identified one such study (Delaney, 2006).
  4. The following search terms were used: focused deterrence, deterring violent offenders, pulling levers AND police, problem-oriented policing, police AND repeat offenders, police AND gangs, police AND guns, gang violence prevention, strategic gang enforcement, crackdowns AND gangs, enforcement swamping, and drug market intervention.
  5. The following 15 databases were searched: Sociological Abstracts; Criminal Justice Abstracts; National Criminal Justice Reference Service (NCJRS) Abstracts; Educational Resources Information Clearinghouse (ERIC); Government Publications Office Monthly Catalog (GPO Monthly); Google Scholar; Proquest Dissertation and Theses A&I; West Law Next; Informit (includes CINCH); Web of Science Core Collection; Academic Search Premier; HeinOnline; Social Sciences Premium Collection; the Grey Literature database maintained by the Gottfredson Library at the Rutgers University School of Criminal Justice; and

the effectiveness of focused deterrence programs was examined (Braga, 2012; Kennedy, 2008; National Research Council, 2004, 2005). Third, we performed forward searches for works in which the original focused deterrence review (Braga and Weisburd, 2012a, 2012b) and seminal focused deterrence studies were cited (Braga et al., 2001; Kennedy et al., 1996; McGarrell et al., 2006; Papachristos et al., 2007). Fourth, we searched bibliographies of narrative reviews of police crime prevention programs (Braga, 2008a; Gravel, Bouchard, Descormiers, Wong, and Morselli, 2013; Koper, Woods, and Kubu, 2013; McGarrell et al., 2013; Petrosino et al., 2015; Sherman, 2002; Weisburd and Eck, 2004; Werb et al., 2011) and past completed Campbell systematic reviews of police crime prevention efforts (Bowers, Johnson, Guerette, Summers, and Poynton, 2011; Braga, Papachristos, and Hureau, 2014; Koper and Mayo-Wilson, 2012; Mazerolle, Soole, and Rombouts, 2006; Weisburd, Telep, Hinkle, and Eck, 2008). Fifth, we performed hand searches of leading journals in the field.<sup>6</sup> These searches were all completed between August 2015 and October 2015.

After finishing these searches and reviewing the studies, as described later, we e-mailed the list of studies meeting our eligibility criteria in December 2015 to leading criminology and criminal justice scholars knowledgeable in the area of focused deterrence strategies. These 100 scholars were defined as those who authored at least one study that appeared on our inclusion list, anyone involved with U.S. National Research Council (2004, 2005) reviews of police research and firearms research, and other leading scholars identified by the authors (available by request). This approach helped us identify unpublished studies that did not appear in conventional databases or other reviews. Finally, we consulted with an information retrieval specialist at the outset of our review and at points along the way to ensure that we used appropriate search strategies to identify the studies meeting the criteria of this review.<sup>7</sup>

### *Statistical Procedures and Conventions*

As a preliminary examination of the effects of focused deterrence strategies on crime, we used a vote counting procedure. In this rudimentary approach, the authors of each study metaphorically casts a vote for or against the effectiveness of treatment. Vote counting has been criticized because it relies on the individual inferences made by the researchers of each study, and it fails to take into account differences, for example, in the size of samples

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The Campbell Collaboration Social, Psychological, Educational and Criminological Trials Register (original review).

6. These journals were *Criminology*, *Criminology & Public Policy*, *Justice Quarterly*, *Journal of Research in Crime and Delinquency*, *Journal of Criminal Justice*, *Police Quarterly*, *Policing*, *Police Practice and Research*, *British Journal of Criminology*, *Journal of Quantitative Criminology*, *Crime & Delinquency*, *Journal of Criminal Law and Criminology*, and *Policing and Society*. Hand searches covered 1979–2015.
7. Ms. Phyllis Schultze of the Gottfredson Library at the Rutgers University School of Criminal Justice assisted with the initial abstract search and was consulted throughout on our search strategies.

across studies (see, e.g., Lipsey and Wilson, 2001). Nevertheless, vote counting provides a descriptive portrait of the conclusions reached study by study, and in cases like this one, where the researchers overall reach similar conclusions, it can strengthen confidence in the overall study findings.

In our closer examination of program effects, meta-analyses were used to determine the size, direction, and statistical significance of the overall impact of focused deterrence strategies on crime by weighting program effect sizes based on the variance of the effect size and the study sample size (Lipsey and Wilson, 2001). We used the standardized mean difference effect size (also known as Cohen's *d*; see Cohen, 1988; Rosenthal, 1994) and employed the Effect Size Calculator, developed by David B. Wilson and available on the Campbell Collaboration's website, to calculate standardized mean difference effect sizes for reported outcomes in each study. We then used Biostat's Comprehensive Meta Analysis Version 2.2 to conduct the meta-analysis of effect sizes.

One problem in conducting meta-analyses in crime and justice is that investigators often do not prioritize outcomes examined. This is common in studies in the social sciences in which authors view good practice as demanding that all relevant outcomes be reported. The lack of prioritization of outcomes in a study, however, raises the question of how to derive an overall effect of treatment. For example, the reporting of one significant result may reflect a type of "creaming" in which the authors focus on one large and significant finding and ignore the less positive results of other outcomes. But authors commonly view the presentation of multiple findings as a method for identifying the specific contexts in which the treatment is effective. When the number of such comparisons is small, and therefore unlikely to affect the error rates for specific comparisons, such an approach is often valid.

The meta-analysis was used to examine program effects via three approaches. The first is conservative in the sense that it combines all reported outcomes into an overall average effect size statistic. The second represents the largest effect reported in the studies, and gives an upper bound to our findings. It is important to note that in some of the studies with more than one outcome reported, the largest outcome reflected what authors thought would be the most direct program effect. Finally, we present the smallest effect size for each study. This approach is the most conservative and likely underestimates the effect of focused deterrence on crime. We use it here primarily to provide a lower bound to our findings.

## Findings

Search strategies in the systematic review process generate a large number of citations and abstracts for potentially relevant studies that must be closely screened to determine whether the studies meet the eligibility criteria (Farrington and Petrosino, 2001). The screening process yields a much smaller pool of eligible studies for inclusion in the review. Combined with the results from the original review, the search strategies produced 62,541

distinct abstracts. The contents of these abstracts were reviewed for any suggestion of an evaluation of focused deterrence interventions: 473 distinct abstracts were selected for closer review, and the full-text reports, journal articles, and books for 131 of these abstracts were acquired and carefully assessed to determine whether the interventions involved focused deterrence strategies and whether the studies were randomized controlled trial designs or nonrandomized quasi-experimental designs. Twenty-four eligible studies were identified and included in the updated review:

1. Operation Ceasefire in Boston, Massachusetts (Braga et al., 2001)
2. Indianapolis Violence Reduction Partnership in Indianapolis, Indiana (McGarrell et al., 2006)
3. Operation Peacekeeper in Stockton, California (Braga, 2008b)
4. Project Safe Neighborhoods in Lowell, Massachusetts (Braga, Pierce, McDevitt, Bond, and Cronin, 2008)
5. Cincinnati Initiative to Reduce Violence in Cincinnati, Ohio (Engel, Corsaro, and Tillyer, 2010)
6. Operation Ceasefire in Newark, New Jersey (Boyle, Lanterman, Pascarella, and Cheng, 2010)
7. Operation Ceasefire in Los Angeles, California (Tita et al., 2004)
8. Operation Ceasefire in Rochester, New York (Delaney, 2006)
9. Project Safe Neighborhoods in Chicago, Illinois (Papachristos et al., 2007)
10. Drug Market Intervention in Nashville, Tennessee (Corsaro and McGarrell, 2009a)
11. Drug Market Intervention in Rockford, Illinois (Corsaro, Brunson, and McGarrell, 2009)
12. Drug Market Intervention in High Point, North Carolina (Corsaro, Hunt, Hipple, and McGarrell, 2012)
13. Drug Market Intervention in Peoria, Illinois (Corsaro and Brunson, 2013)
14. Operation Ceasefire II in Boston, Massachusetts (Braga, Hureau, and Papachristos, 2014)
15. Community Initiative to Reduce Violence in Glasgow, Scotland (Williams, Currie, Linden, and Donnelly, 2014)
16. Group Violence Reduction Strategy in Chicago, Illinois (Papachristos and Kirk, 2015)
17. Group Violence Reduction Strategy in New Orleans, Louisiana (Corsaro and Engel, 2015)
18. No Violence Alliance in Kansas City, Missouri (Fox, Novak, and Yaghoub, 2015)
19. Project Longevity in New Haven, Connecticut (Sierra-Arevalo, Charette, and Papachristos, 2015)
20. Drug Market Intervention in Roanoke, Virginia (Saunders, Kilmer, and Ober, 2015)
21. Drug Market Intervention in Montgomery County, Maryland (Saunders, Kilmer, et al., 2015)

**T A B L E 1**

**Characteristics of Eligible Focused Deterrence Evaluations ( $N = 24$ )**

Characteristic	<i>n</i>	Percent
Country		
United States	23	95.8
Other (Scotland)	1	4.2
City Population		
Small (<200,000 residents)	8	33.3
Medium (200,000–500,000 residents)	6	25.0
Large (>500,000 residents)	10	41.7
Study Type		
Quasi-experiment with matched comparison group	12	50.0
Quasi-experiment with nonequivalent comparison group	9	37.5
Quasi-experiment with no comparison group (ITS)	3	12.5
Intervention Type		
Gang/group violence	12	50.0
Individual crime	3	12.5
Drug market	9	37.5
Displacement and Diffusion		
Measured displacement/diffusion	5	20.8
Did not measure displacement/diffusion	19	79.2
Publication Type		
Peer-reviewed journal	15	62.5
Grey literature	9	37.5
Published report	2	8.3
Unpublished report	7	29.2
Completion Year		
2001–2004	2	8.3
2005–2008	5	20.8
2009–2012	5	20.8
2013–2015	12	50.0

22. Drug Market Intervention in Guntersville, Alabama (Saunders, Kilmer, et al., 2015)

23. Drug Market Intervention in Seattle, Washington (Saunders, Kilmer, et al., 2015)

24. Drug Market Intervention in Ocala, Florida (Saunders, Kilmer, et al., 2015)

*Characteristics of Eligible Focused Deterrence Studies*

The 14 newly identified studies represent a large increase in eligible studies (140%) over the 10 evaluations considered in the previous systematic review. Table 1 is a summary of the characteristics of the 24 selected studies. In the selected studies, focused deterrence interventions were examined that were implemented in small, medium, and large cities. A focused deterrence program implemented in a jurisdiction outside the United States (Glasgow, Scotland) was only evaluated in one study. More than one third ( $n = 9$ , 37.5%)

of the eligible studies were acquired through “grey literature” sources<sup>8</sup> at the time the review of abstracts was completed.<sup>9</sup> All 24 evaluations were released after 2000, and half were completed after 2013. Researchers in half of the studies evaluated the crime reduction effects of focused deterrence strategies on serious violence generated by street gangs or criminally active street groups. In nine studies, the authors evaluated strategies focused on reducing crime driven by street-level drug markets (Guntersville, High Point, Montgomery County, Nashville, Ocala, Peoria, Roanoke, Rockford, and Seattle), and in three, they evaluated crime reduction strategies that were focused on individual repeat offenders (Chicago, Glasgow, and Newark).

All eligible studies were quasi-experimental designs aimed at analyzing the impact of focused deterrence strategies on crime. Half of the evaluations were quasi-experimental designs with near-equivalent comparison groups created through matching techniques. The Los Angeles evaluation comprised a quasi-experimental design that included both nonequivalent and matched comparison groups; for the Los Angeles study, we only included the effects from the more rigorous matched comparison group analysis in our meta-analysis. Nine evaluations (37.5%) used quasi-experimental designs with nonequivalent comparison groups (Boston, Cincinnati, Indianapolis, Lowell, Nashville, New Haven, New Orleans, Rockford, and Stockton). The comparison units used in these evaluations were selected based on naturally occurring conditions, such as other cities or within-city areas that did not receive treatment, rather than through careful matching to ensure comparability with treatment units. Three studies (12.5%) comprised one-group-only interrupted time-series designs (Kansas City, Peoria, and Rochester). Table 2 is a brief summary of the treatments, units of analysis, research designs, and results reported by the 24 eligible studies.<sup>10</sup>

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8. The grey literature is a term applied to sources of information that are not commercially published and is typically composed of technical reports, working papers, government and agency reports, and conference proceedings. Wilson (2009) has argued that there is often little difference in methodological quality between published and unpublished studies, suggesting the importance of searching the grey literature.
  9. During the development of this report, the New Haven study was accepted for publication at *Crime & Delinquency* and the Roanoke study was accepted for publication at *Journal of the American Statistical Association*.
  10. Table 2 summarizes the characteristics of the key focused deterrence evaluation identified through the varied search processes. Five evaluations had companion quasi-experimental analyses that supported the program impact conclusions presented here: Boston Ceasefire I (Piehl, Cooper, Braga, and Kennedy, 2003), Boston Ceasefire II (Braga, Apel, and Welsh, 2013), Chicago PSN (Wallace, Papachristos, Meares, and Fagan, 2016), High Point DMI (Corsaro, 2013), and Indianapolis (Corsaro and McGarrell, 2009b). In addition to the Corsaro et al. (2012) evaluation, the RAND Corporation completed an independent evaluation of the High Point DMI using a synthetic control quasi-experimental design (Saunders, Lundberg, Braga, Ridgeway, and Miles, 2015). The findings of the RAND evaluation revealed a slightly stronger impact of the DMI program on targeted outcomes. The authors found that in the year after a DMI, calls for service decreased 16% and violent crimes decreased 34%, on average, compared with synthetic control markets. They also found no evidence of statistically significant crime displacement or diffusion effects after a DMI was implemented.

T A B L E 2

Eligible Focused Deterrence Evaluations (N = 24)

Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Operation Ceasefire</b> Boston, Massachusetts Braga et al. (2001)	Strategy focused on reducing serious violence by street gangs 24-month post-intervention period (June 1996–May 1998) No threats to integrity of treatment noted during program implementation	Citywide intervention Outcome measures included monthly counts of citywide youth homicide incidents, citywide gun assault incidents, citywide shots fired calls for service, and youth gun assault incidents in one high-risk district	Nonequivalent quasi-experiment comparing youth homicide trends in Boston relative to youth homicide trends in 39 other U.S. cities and 29 New England cities Count-based regression models controlling for trends and seasonal variations used to estimate impact of intervention on time series	Statistically significant 63% reduction in youth homicides, 25% reduction in gun assaults, 32% reduction in shots fired calls for service, and 44% reduction in youth gun assaults in one high-risk district Displacement/diffusion effects not measured
<b>Indianapolis Violence Reduction Partnership</b> Indianapolis, Indiana McGarrell et al. (2006)	Strategy focused on reducing serious violence by street gangs 27-month post-intervention period (April 1999–June 2001) No threats to integrity of treatment noted during program implementation	Citywide intervention Outcome measure was the monthly count of citywide homicides	Nonequivalent quasi-experiment comparing homicide trends in Indianapolis relative to homicide trends in six cities selected based on population and Midwestern location ARIMA models controlling for trends and seasonal variations used to estimate impact of intervention on time series	Statistically significant 34% reduction in total homicide Displacement/diffusion effects not measured
<b>Operation Peacekeeper</b> Stockton, California Braga (2008b)	Strategy focused on reducing serious violence by street gangs 65-month post-intervention period (September 1997–December 2002) No threats to integrity of treatment noted during program implementation	Citywide intervention Outcome measure was the monthly count of citywide gun homicides	Nonequivalent quasi-experiment comparing gun homicide trends in Stockton relative to gun homicide trends in eight cities selected based on population and California location Count-based regression models controlling for trends and seasonal variations used to estimate impact of intervention on time series	Statistically significant 42% reduction in gun homicide Displacement/diffusion effects not measured

(Continued)



TABLE 2

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Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Project Safe Neighborhoods</b> Lowell, Massachusetts Braga et al. (2008)	Strategy focused on reducing serious violence by street gangs 39-month post-intervention period (October 2002–December 2005) No threats to integrity of treatment noted during program implementation	Citywide intervention Outcome measure was the monthly count of fatal and nonfatal gun assault incidents	Nonequivalent quasi-experiment comparing gun assault trends in Lowell relative to gun assault trends in the State of Massachusetts and eight Massachusetts cities selected based on population, demographics, and yearly numbers of gun assaults Count-based and maximum-likelihood regression models controlling for trends and seasonal variations used to estimate impact of intervention on time series	Statistically significant 44% reduction in gun assault incidents Displacement/diffusion effects not measured
<b>Cincinnati Initiative to Reduce Violence</b> Cincinnati, Ohio Engel et al. (2010)	Strategy focused on reducing serious violence by criminally active street groups 37-month post-intervention period (October 2007–September 2009) No threats to integrity of treatment noted during program implementation	Citywide intervention Outcome measures were the monthly counts of citywide group member-involved and nongroup member-involved homicides	Nonequivalent quasi-experiment comparing group-member-involved homicide trends relative to nongroup-member-involved homicides Count-based regression models controlling for trends and seasonal variations used to estimate impact of intervention on time series	Statistically significant 35% reduction in group member-involved homicides Displacement/diffusion effects not measured
<b>Operation Ceasefire</b> Newark, New Jersey Boyle et al. (2010)	Violence reduction strategy targeting individual gang members described as a “hybrid” between the Boston Ceasefire pulling levers strategy and the Chicago Ceasefire streetworker program 85-week post-intervention period (May 11, 2005–December 31, 2006) No threats to integrity of treatment noted during program implementation	Intervention implemented in two square mile area that experienced elevated levels of gun violence Outcome measure was the weekly number of gunshot wound incidents	Near-equivalent quasi-experiment comparing gunshot wound trends in the targeted area relative to gunshot wound trends in a comparison area selected based on similar levels of gun violence, geographic size, and demographic characteristics ARIMA models controlling for trends and seasonal variations used to estimate impact of intervention on time series Used dual kernel density spatial analyses to examine the distribution of gunshot wound hot spots around target and comparison zones before and after the intervention was implemented	No statistically significant reduction in gunshot wound victims in target zone The results of the displacement/diffusion analysis were inconclusive

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T A B L E 2

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Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Operation Ceasefire</b> Los Angeles, California Tita et al. (2004)	Strategy focused on reducing serious violence by criminally active street groups Six-month post-intervention period (October 2000–February 2001) Evaluation team reported that integrity of the treatment was undermined as a result of a lack of commitment to the strategy by working group members and the unintended consequences of a police corruption scandal	Intervention was implemented in a target area within the Boyle Heights neighborhood of Los Angeles Outcome measures were monthly counts of violent crime incidents, gang crime incidents, and gun crime incidents	Quasi-experimental evaluation used two nonequivalent comparisons: (the target area relative to the remainder of Boyle Heights; Boyle Heights relative to the surrounding larger Hollenbeck community) and one near-equivalent comparison (Census block groups matched via propensity score analyses) A variety of regression-based models were used to estimate the impact of the intervention on the distribution of monthly counts of the key outcome variables for six month pre-intervention, four month suppression, and two month deterrence time periods Examined immediate spatial displacement and diffusion effects in 11 Census block groups surrounding targeted Census block groups and gang crime committed by nontargeted gangs that were “socially tied” to targeted gangs	In Boyle Heights, gang crime decreased significantly compared with other regions of Hollenbeck during the suppression period of the intervention, and violent, gang, and gun crime all decreased significantly in the deterrence period In the five targeted police reporting districts, violent crime decreased significantly in comparison with the rest of Boyle Heights in the suppression and the deterrence periods, and gang crime decreased significantly in the suppression period In the Census block groups overlapping the targeted reporting districts, violent crime decreased significantly compared with the matched blocks Analyses suggested strong diffusion of crime control benefits into Census block groups immediately surrounding targeted area and a reduction in gang crime associated with the “socially tied” gangs
<b>Operation Ceasefire</b> Rochester, New York Delaney (2006)	Strategy focused on reducing serious violence by street gangs and criminally active groups 15-month post-intervention period (October 2003–December 2004) The treatment was undermined as a result of problems with inter-agency communication, limited enforcement actions, and inadequate delivery of the deterrence message	Citywide intervention Outcome measures were monthly counts of homicide, gun assault 1 <sup>st</sup> degree, and gun robbery 1 <sup>st</sup> degree, with a sub-analysis on black male victims ages 15–30 for each outcome	One-group-only interrupted time series evaluation comparing citywide outcome trends; pre- and post-intervention Multiple regression models controlling for trends, seasonal variations, and lagged intervention effects, as well as changes in economic conditions and policing behavior, to estimate the impact of the intervention on the time series	Statistically significant 25% reduction in homicide involving black male victims ages 15–30 and 27% reduction in gun robbery involving black male victims ages 15–30 at one, three, and four month lags No significant reduction in total homicide and total gun violence, as well as gun assault involving black male victims ages 15–30 Displacement/diffusion effects not measured

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T A B L E 2

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Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Project Safe Neighborhoods</b> Chicago, Illinois Papachristos et al. (2007)	Gun violence reduction strategy comprised of four interventions: (1) increased federal prosecutions for convicted felons carrying or using guns, (2) lengthy sentences associated with federal prosecutions, (3) supply-side firearm policing activities, and (4) social marketing of deterrence and social norms messages through offender notification meetings	Intervention was implemented in two adjacent policing districts that experienced very high levels of homicide  Outcome measures were monthly and quarterly counts of homicides, gun homicides, gang homicides, and aggravated assault and battery incidents	Quasi-experimental evaluation comparing trends in targeted policing districts to trends in near-equivalent policing districts matched via propensity score analysis  Hierarchical generalized linear growth curve regression models used to estimate impact of intervention on time series	Statistically significant 37% reduction in total homicides reported in targeted police districts  Statistically significant reductions in gun homicides and aggravated assaults in targeted districts also reported  No statistically significant reduction in gang homicides in targeted police districts  Displacement/diffusion effects not measured
<b>Drug Market Intervention</b> Nashville, Tennessee Corsaro and McGarrell (2009a)	Strategy focused on reducing crime driven by street-level drug market  14-month post-intervention period (March 2008–April 2009)  No threats to integrity of treatment noted during program implementation	Intervention was implemented in the McFerrin Park neighborhood of Nashville  Outcome measures were monthly count of violent crime incidents, property crime incidents, illegal drug possession incidents, illegal drug equipment incidents, and total calls for service	Nonequivalent quasi-experimental design comparing trends in the intervention neighborhood to trends in the remainder of Davidson County  ARIMA models controlling for trends and seasonal variations used to estimate impact of intervention on time series  Examined immediate spatial displacement and diffusion effects in areas contiguous to the targeted neighborhood	Statistically significant 55% reduction in illegal drug possession offenses, 37% reduction in drug equipment offenses, and 28% reduction in property crimes reported in targeted neighborhood  No significant decreases reported in violent crime incidents and total calls for service  Analyses suggested significant diffusion of crime control benefits into contiguous areas
<b>Drug Market Intervention</b> Rockford, Illinois Corsaro et al. (2009)	Strategy focused on reducing crime driven by street-level drug market  14-month post-intervention period (May 2007–June 2008)  No threats to integrity of treatment noted during program implementation	Intervention was implemented in the Delaney Heights neighborhood of Rockford  Outcome measures were monthly count of violent crime incidents and nonviolent crime incidents	Nonequivalent quasi-experimental design comparing trends in the intervention neighborhood to trends in the remainder of Rockford  Hierarchical generalized linear growth curve regression models used to estimate impact of intervention on time series	Statistically significant 22% reduction in nonviolent offenses  No significant decreases reported in violent offenses  Displacement/diffusion effects not measured

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T A B L E 2

Continued

Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Drug Market Intervention</b> High Point, North Carolina Corsaro et al. (2012)	Strategy focused on reducing crime driven by street-level drug market 60-month post-intervention period from the year of the first implementation site (January 2004–December 2008) No threats to integrity of treatment noted during program implementation	Intervention implemented in four neighborhoods Outcome measure was the annual count of violent crime	Quasi-experimental evaluation comparing Census blocks within the target area with matched comparison groups via propensity score analyses Count-based panel regression models with difference-in-difference estimators and place-based and time-varying fixed effects at the census block level Examined immediate spatial displacement and diffusion effects in 59 adjacent Census blocks	Statistically significant 14% reduction in violent crime reported in target area Analyses suggested a nonsignificant increase in violent crime in areas adjacent to target neighborhoods
<b>Drug Market Intervention</b> Peoria, Illinois Corsaro and Brunson (2013)	Strategy focused on reducing crime driven by street-level drug market 13-month post-intervention period (November 2009–December 2010) Evaluation team reported that integrity of the treatment was undermined as a result of a lack of citizen involvement in and community awareness of the intervention	Intervention was implemented in one neighborhood that had a disproportionately high number of crimes Outcome measures included monthly counts of violent crime, property crime, drug and disorder crime, and total calls for service	One-group-only interrupted time series evaluation comparing trends pre- and post-intervention for the target neighborhood ARIMA models controlling for trends and seasonal variations used to estimate impact of intervention Telephone surveys with residents in target area to determine their familiarity with the intervention and their perceived changes in neighborhood crime and disorder over the previous six months	No statistically significant relationship with violent crime, property crime, drug/disorder crime, or total calls for service Displacement/diffusion effects not measured
<b>Operation Ceasefire II</b> Boston, Massachusetts Bagia, Hureau, and Papachristos (2014)	Strategy focused on reducing serious violence by street gangs 48-month post-intervention period (January 2007–December 2010) No threats to integrity of treatment noted during program implementation	Citywide intervention targeted 19 gangs over the study period Outcome measures included quarterly counts of victim gang-involved shootings, suspect gang-involved shootings, and total gang-involved shootings	Quasi-experimental evaluation comparing trends for treated gangs to trends for untreated gangs matched via propensity score analyses Negative binomial growth curve regression models with differences-in-differences estimators controlling for trends and seasonal variations to estimate the impact of intervention on time series Displacement/diffusion effects measured for untreated “socially connected” gangs	Statistically significant 31% reduction in total gang-involved shootings, 35% reduction in suspect gang-involved shootings, and 27% in victim gang-involved shootings among targeted gangs Statistically significant 24% reduction in total gang-involved shootings and 27% suspect gang-involved shootings for vicariously treated gangs relative to matched comparison gangs (Bagia et al., 2013)

(Continued)

TABLE 2

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Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Community Initiative to Reduce Violence</b> Glasgow, Scotland Williams et al. (2014)	Strategy designed to reduce physical violence and weapon carrying by gang youth 35-month post-intervention period (October 2008–October 2011) No threats to integrity of treatment noted during program implementation	Intervention implemented in two police divisions corresponding to the area of Glasgow Outcome measures include annual counts of violent crime, nonviolent crime, physical violence crime, and weapon carrying crime	Quasi-experimental design comparing trends for one and two-year cohorts of targeted youth to matched comparison youth Conditional fixed-effects Poisson regression models including a group-time period interaction term used to estimate impact of intervention	Statistically significant 65% and 84% reductions in weapon carrying among 1 and 2-year targeted cohorts Displacement/diffusion effects not measured
<b>Group Violence Reduction Strategy</b> Chicago, Illinois Papachristos and Kirk (2015)	Strategy focused on reducing serious violence by street gangs 12-month post-call-in evaluation period No threats to integrity of treatment noted during program implementation	Citywide intervention that targeted 149 gang factions Outcome measures include the number of victimization, offending, and total shooting involvement for each faction	Quasi-experimental design post-intervention shooting counts for treated gangs relative to post-intervention shooting counts for untreated gangs matched via propensity score analyses Difference-of-group means Z-test comparison	Statistically significant 32% reduction in shooting victimization among targeted gangs relative to matched comparisons Marginally significant 23% reduction in total shooting involvement among targeted gangs relative to matched comparisons Displacement/diffusion effects not measured
<b>Group Violence Reduction Strategy</b> New Orleans, Louisiana Corsaro and Engel (2015)	Strategy focused on reducing serious violence by street gangs and criminally active groups 17-month post-intervention period (November 2012–March 2014) No threats to integrity of treatment noted during program implementation	Citywide intervention Outcome measures include monthly counts of overall homicides, overall violent crime, overall property crime, firearm-related homicides, firearm assaults, gang-member involved homicides, and nongang-member involved homicides	Nonequivalent quasi-experimental evaluation comparing homicide trends in New Orleans to 14 comparable cities and 6 high-trajectory cities Difference-in-difference count regression models used to compare homicide trends in New Orleans to nonequivalent controls with counterfactual tests	Statistically significant 17% reduction in total homicides, 32% reduction in gang-member-involved homicides, 17% reduction in firearm homicides, and 17% reduction in nonfatal firearm assaults No statistically significant relationship with nongang-member-involved homicides Displacement/diffusion effects not measured

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T A B L E 2

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Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>No Violence Alliance</b> Kansas City, Missouri Fox et al. (2015)	Strategy focused on reducing serious violence by street gangs and criminally active groups	Citywide intervention	One-group-only interrupted time series evaluation	Statistically significant homicide reductions of 40% at one month, 34% at three months, and 29% at six months
	12-month post-intervention period Early implementation was plagued by poor leadership and communication, which delayed full implementation until nearly one year after the originally intended start day and those problems have been rectified	Outcome measures include monthly counts of homicide and aggravated assault with a firearm	used to compare citywide trends in the pre-intervention period to 1-month, 3-month, 6-month, and 12-month post-intervention time periods	Statistically significant gun-involved aggravated assault reductions of 19% at one month and 14% at three months No statistically significant relationship with homicide at 12 months or gun-involved aggravated assault at 6 months and 12 months Displacement/diffusion effects not measured
<b>Project Longevity</b> New Haven, Connecticut Sierra-Arevalo et al. (2015)	Strategy focused on reducing serious violence by street gangs and criminally active groups	Citywide intervention	Nonequivalent quasi-experimental design comparing shooting trends in New Haven to a similar city (Hartford, Connecticut)	Statistically significant 37% reduction in total shootings and homicides and 73% reduction in group-member involved homicides and shootings
	18 month post-intervention period (November 2012–April 2014) No threats to integrity of treatment noted during program implementation	Outcome measures were the monthly counts of citywide total fatal and nonfatal shootings; group-member-involved shootings; and nongroup-member-involved shootings	ARIMA models controlling for trends and seasonal variations used to estimate the impact of the intervention on the time series	Displacement/diffusion effects not measured
<b>Drug Market Intervention</b> Roanoke, Virginia Saunders, Kilmer, et al. (2015)	Strategy focused on reducing crime driven by street-level drug market	Intervention was implemented in the Hurt Park and Melrose-Rugby neighborhoods	Quasi-experimental evaluation comparing trends in the targeted neighborhood to trends in comparison neighborhoods matched via synthetic control methods	In the Hurt Park neighborhood, statistically significant 30% reduction in total crime at 3 months, 19% at 6 months, 28% at 9 months, and 23% at 12 months; statistically significant 45% reduction in property crime at 6 months, 57% at 9 months, and 50% at 12 months, as well as significant 24% reduction in violent crime at 3 months and 29% at 9 months
	12-month post-intervention period (beginning December 2011 for Hurt Park and January 2013 for Melrose-Rugby) The treatment was undermined as a result of a history of poor police-community relations and lack of faith among residents in police despite the program's efforts to engage the community	Outcome measures were 12-month counts of total crime, violent crime, property crime, and drug crime	Negative binomial regression models controlling for trends were used to estimate the impact of the intervention on the time series	In the Melrose-Rugby neighborhood, statistically significant 15% reduction in violent crime at 3 months and 34% at 6 months Displacement/diffusion effects not measured

(Continued)

TABLE 2

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Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Drug Market Intervention</b> Montgomery County, Maryland Saunders, Kilmer, et al. (2015)	Strategy focused on reducing crime driven by street-level drug market 12-month post-intervention period (March 2011–February 2012) The treatment was undermined as a result of a lack of community engagement	Intervention was implemented in the Damascus Gardens one-square-block apartment complex Outcome measures were 12-month counts of total crime, violent crime, property crime, and drug crime	Quasi-experimental evaluation comparing trends in the targeted neighborhood to trends in comparison neighborhoods matched via synthetic control methods Negative binomial regression models controlling for trends were used to estimate the impact of the intervention on the time series	No statistically significant reduction in total crime, violent crime, property crime, or drug crime Displacement/diffusion effects not measured
<b>Drug Market Intervention</b> Guntersville, Alabama Saunders, Kilmer, et al. (2015)	Strategy focused on reducing crime driven by street-level drug market 12-month post-intervention period (December 2011–November 2012) The treatment was undermined as a result of a lack of citizen involvement, as well as community distrust for police and faith leaders involved in the intervention	Intervention was implemented in the Lakeview neighborhood Outcome measures were 12-month counts of total crime, violent crime, property crime, and drug crime	Quasi-experimental evaluation comparing trends in the targeted neighborhood to trends in comparison neighborhoods matched via synthetic control methods Negative binomial regression models controlling for trends were used to estimate the impact of the intervention on the time series	No statistically significant reduction in total crime, violent crime, property crime, or drug crime Displacement/diffusion effects not measured
<b>Drug Market Intervention</b> Seattle, Washington Saunders, Kilmer, et al. (2015)	Strategy focused on reducing crime driven by street-level drug market 12-month post-intervention period (beginning December 2009 for 23 <sup>rd</sup> Street and January 2013 for International District) No threats to integrity of treatment noted during program implementation	Intervention was implemented in the areas of the 23 <sup>rd</sup> Street Corridor and International District Outcome measures were 12-month counts of total crime, violent crime, property crime, and drug crime	Quasi-experimental evaluation comparing trends in the targeted neighborhood to trends in comparison neighborhoods matched via synthetic control methods Negative binomial regression models controlling for trends were used to estimate the impact of the intervention on the time series	In the International District, statistically significant 15% reduction in total crime at 3 months and 6 months; statistically significant 8% reduction in property crime at 3 months and marginally significant 17% reduction at 6 months; statistically significant 53% reduction in violent crime at 3 months and marginally significant 40% reduction at 6 months, 34% reduction at 9 months, and 29% reduction in drug crime at 3 months and marginally significant 17% reduction at 6 months In the 23 <sup>rd</sup> Street Corridor neighborhood, no statistically significant reduction in total crime, violent crime, property crime, or drug crime Displacement/diffusion effects not measured

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T A B L E 2

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Study	Treatment	Units of Analysis	Research Design	Crime Outcomes
<b>Drug Market Intervention</b> Ocala, Florida Saunders, Klimer, et al. (2015)	Strategy focused on reducing crime driven by street-level drug market 12-month post-intervention period (beginning November 2009 for Second Chance and October 2010 for First Avenue) No threats to integrity of treatment noted during program implementation	Intervention was implemented in the "Second Chance" neighborhood and the First Avenue housing project Outcome measures were 12-month counts of total crime, violent crime, property crime, and drug crime	Quasi-experimental evaluation comparing trends in the targeted neighborhood to trends in comparison neighborhoods matched via synthetic control methods Negative binomial regression models controlling for trends were used to estimate the impact of the intervention on the time series	No statistically significant reduction in total crime, violent crime, property crime, or drug crime for either intervention site Displacement/diffusion effects not measured



None of the eligible studies were randomized controlled trial designs aimed at evaluating the crime control impacts of focused deterrence programs. The lack of randomized controlled trials is concerning; well-implemented randomized studies provide the strongest evidence of the causal impacts of programs or practices. Nonetheless, the findings of our review show that, in recent years, program evaluators have increasingly used more rigorous quasi-experimental designs with matched comparison groups to estimate focused deterrence impacts. The previous iteration of this Campbell review (Braga and Weisburd, 2012a, 2012b) found that in only 30% (3 of 10) eligible studies were quasi-experimental designs with matched comparison groups. In contrast, 64.3% (9 of 14) of the newly identified studies in this updated review comprised these more rigorous controlled designs. Although randomized experiments are sorely needed, the trend toward quasi-experimental designs with higher levels of internal validity suggests reviewers can have more confidence in study findings on the effects of focused deterrence programs on crime.

The evolution of the rigor of the quasi-experimental evaluation techniques is evidenced by the differing approaches used to evaluate separate implementations of the well-known Boston Operation Ceasefire strategy in the 1990s and then in the mid-2000s. The U.S. Department of Justice (DOJ)-sponsored evaluation of the impact of Operation Ceasefire in the 1990s used a nonrandomized quasi-experimental design to compare youth homicide trends in Boston with youth homicide trends in other major cities in the United States and large New England cities (Braga et al., 2001; noted here as Boston Ceasefire I). The within-Boston program impact assessment was supplemented by analyses of Ceasefire's effect on the monthly number of citywide gun assault incidents, citywide shots-fired calls for service, and youth gun assault incidents in one high-risk policing district. Count regression models, controlling for secular trends, seasonal variations, Boston youth population trends, Boston employment rate trends, robbery trends, adult homicide trends, and youth drug arrest trends, were used to estimate the effect of Ceasefire on the outcome variables. The impact of Ceasefire was estimated using a dummy variable to represent the commencement of the treatment time period. As noted in Table 2, the Boston Ceasefire I intervention was associated with a 63% decrease in youth homicides that was distinct from youth homicide trends in the comparison cities.

The Boston Ceasefire I evaluation has been reviewed by several researchers, and the relationship between program implementation and the subsequent trajectory of youth homicide in Boston during the 1990s has been closely scrutinized (see Fagan, 2002; Ludwig, 2005; Morgan and Winship, 2007; Rosenfeld, Fornango, and Baumer, 2005). The U.S. National Research Council's (2005) Committee to Improve Research Information and Data on Firearms ultimately concluded that the Ceasefire I evaluation was compelling in associating the intervention with the subsequent decline in youth homicide. Nevertheless, the Committee also suggested that many complex factors affect youth homicide trends, and that it was difficult to specify the exact relationship between the focused deterrence intervention and subsequent changes in youth offending behaviors. Although the Ceasefire

I evaluation controlled for existing violence trends and certain rival causal factors, there could be complex interaction effects among these factors not measured by the evaluation that could account for some meaningful portion of the decrease. The evaluation was not a randomized controlled experiment. As such, the use of the nonrandomized control group research design cannot rule out these internal threats to the conclusion that Ceasefire was the key factor in the youth homicide decline.

Braga, Hureau, et al. (2014) conducted a rigorous quasi-experimental evaluation of a reconstituted Boston Ceasefire program implemented during the mid-2000s in response to a growing gang violence problem (noted here as Boston Ceasefire II). Propensity scores were used to match treated Boston gangs with untreated Boston gangs who were not connected to the treated gangs through rivalries or alliances. Differences-in-differences estimators in growth-curve regression models were used to assess the impact of Ceasefire II by comparing gun violence trends for matched treatment gangs relative with matched comparison gangs during the 2006 through 2010 study period. In the Ceasefire II evaluation, Braga et al. reported that total shootings involving directly treated gangs were reduced by 31% relative to total shootings involving comparison gangs. It is important to note that the findings from the Ceasefire II evaluation yielded a much more conservative violence reduction estimate when compared with program impacts reported in the Ceasefire I quasi-experimental evaluation.

When the Maryland Scientific Methods Scale (Sherman et al., 1997) is used as a standard, the Ceasefire I impact evaluation is considered a “Level 3” (on a five-level scale) evaluation and regarded as the minimum design that is adequate for drawing conclusions about program effectiveness. These designs rule out many threats to internal validity such as history, maturation/trends, instrumentation, testing, and mortality. Nevertheless, as Farrington, Gottfredson, Sherman, and Welsh (2002) observed, the main problems of Level 3 evaluations center on selection effects and regression to the mean as a result of the nonequivalence of treatment and control conditions. The Ceasefire II evaluation would be considered a “Level 4” evaluation as it measured outcomes before and after the program in multiple treatment and control condition units. These types of designs have better statistical control of extraneous influences on the outcome and, relative to lower level evaluations, deal with selection and regression threats more adequately.

In five studies (20.8%), researchers examined possible crime displacement and diffusion of crime control benefit impacts that may have been generated by the focused deterrence interventions. The High Point DMI, Nashville DMI, Newark Ceasefire, and Los Angeles Ceasefire evaluations tested whether areas proximate to treatment locations experienced changes in crime levels. The Los Angeles Ceasefire and Boston Ceasefire II evaluations examined whether the focused deterrence intervention influenced the criminal behavior of gangs socially connected to targeted gangs through rivalries and alliances.

Potential threats to the integrity of the treatment were noted in seven studies (29.2%). For instance, Tita et al. (2004) reported that the Los Angeles intervention was not fully

implemented as planned. The implementation of the Ceasefire program in the Boyle Heights neighborhood of Los Angeles was negatively affected by the well-known Ramparts LAPD police corruption scandal and a lack of ownership of the intervention by the participating agencies. During the initial implementation of the Kansas City No Violence Alliance group violence reduction strategy, Fox et al. (2015) reported a concerning lack of leadership and poor communication among partnering agencies; these issues were eventually addressed as the intervention continued to be implemented. Similarly, the Rochester Ceasefire group violence reduction strategy was negatively impacted by problems with interagency communication that led to limited enforcement actions and inadequate delivery of the deterrence message to targeted groups (Delaney, 2006). DMI programs in Guntersville, Montgomery County, Peoria, and Roanoke were noted to suffer from a lack of community involvement in the targeted areas (Corsaro and Brunson, 2013; Saunders, Ober, Kilmer, and Greathouse, 2016).<sup>11</sup>

#### *Vote Counting Analysis of the Main Effects of Focused Deterrence Strategies on Crime*

In 5 of the 24 evaluations of focused deterrence strategies, researchers did not report at least one noteworthy crime reduction effect associated with the approach (Table 2). In all 12 studies in which the impacts of focused deterrence strategies on violence by gangs and criminally active groups were evaluated, researchers reported at least one statistically significant crime control impact associated with program implementation. Although a non-statistically significant reduction in gunshot wound victimization in the target zone was noted, researchers in the evaluation of Newark's Operation Ceasefire did not report any statistically significant crime prevention benefits generated by focusing on individual violent gang members. In the other four studies, researchers did not report any noteworthy crime control impacts for the DMI programs implemented in Guntersville, Montgomery County, Ocala, and Peoria.

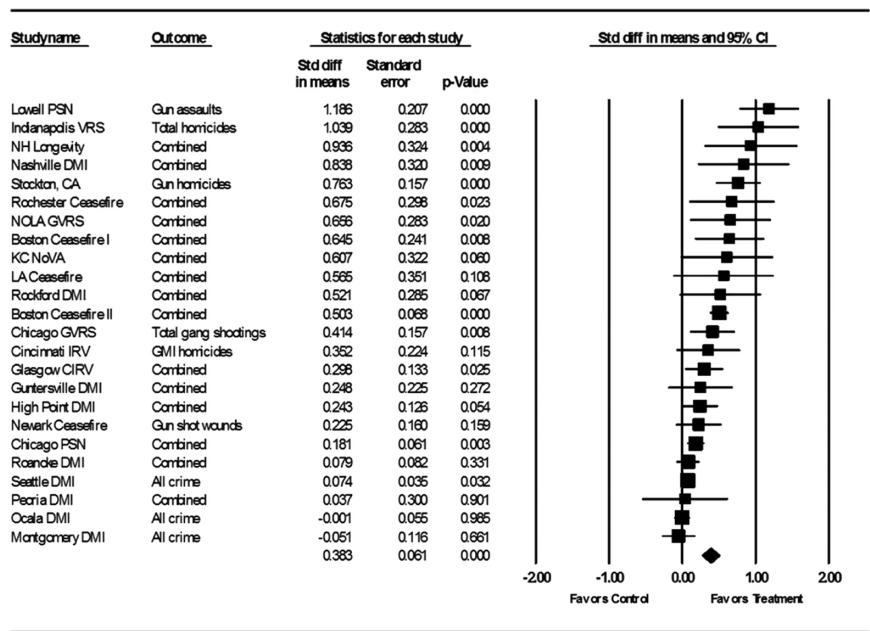
To test the statistical significance of the observed vote counting distribution of crime reduction effects reported by the 24 eligible studies, we used an application of the binomial distribution (Blalock, 1979). This nonparametric test was designed to examine the probabilities of getting an observed proportion of successes from a population

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11. The National Network for Safe Communities raised concerns to the RAND Corporation over the treatment fidelity of the DMI programs that were sponsored by the U.S. Bureau of Justice Assistance and implemented under the guidance of a technical assistance team from Michigan State University. One key concern centered on the absence of reconciliation efforts between police and affected communities on perceived harms associated with prior drug control tactics. Reconciliation is viewed as a critical component of developing the necessary community support needed to exert informal social control over drug sellers in targeted overt drug markets. Further concerns involved unclear definitions of the drug market areas to be targeted for intervention, a lack of opportunity and resources available to targeted dealers with banked cases, and other implementation issues (Personal communication with David Kennedy on February 25, 2017; Memorandum on "DMI Integrity" from David Kennedy to Beau Kilmer and Mark Kleiman, November 17, 2015).

FIGURE 1

Mean Effect Sizes for Study Outcomes



Note. Random effects model,  $Q = 122.568$ ,  $df = 23$ ,  $p < .000$ .

of equal proportions of successes and failures. In 19 of the 24 studies (79.2%), researchers reported noteworthy crime reductions associated with the focused deterrence approach. According to the observed binomial distribution, this result was statistically significant (exact binomial two-tailed probability = .0002). The results from this simple test suggests that focused deterrence strategies generate significant crime control impacts.

Meta-Analysis of the Main Effects of Focused Deterrence Strategies on Crime

Our meta-analyses of the effects of focused deterrence programs on crime included all 24 eligible studies. Using the mean effect criterion for the eligible studies, the forest plot in Figure 1 shows the standardized difference in means between the treatment and control or comparison conditions (effect size) with a 95% confidence interval. Because the studies vary in their contexts and approaches, which is indicated by a significant  $Q$  statistic ( $Q = 122.568$ ,  $df = 23$ ,  $p < .05$ ), we used a random effects model to estimate the overall mean effect size. The results of the meta-analysis of effect sizes suggests a statistically significant effect in favor of focused deterrence strategies. The overall effect size for these studies was .383 ( $p < .05$ ). This is below Cohen's (1988) standard of .50 for a medium effect

size. Nonetheless, the overall effect size is large compared with those of assessments of interventions in crime and justice work more generally (see Lipsey, 2000; MacKenzie and Hickman, 1998; Weisburd, 1993; Weisburd et al., 2008).

Most authors reported effect sizes that favored treatment conditions over control conditions (91.7%, 22 of 24), with the Ocala and Montgomery County programs reporting nonsignificant and tiny negative sizes ( $-.001$  and  $-.051$ , respectively). The authors of the Lowell (1.186), Indianapolis (1.039), and New Haven (.936) studies reported the largest statistically significant effect sizes, whereas those of the Seattle study (.074) reported the smallest statistically significant effect size. As described earlier, we conducted additional meta-analyses of the largest and smallest effect sizes reported for each study.<sup>12</sup> For the largest effect size meta-analysis, the overall standardized mean difference effect size was medium (.577,  $p < .05$ ). For the smallest effect size meta-analysis, the overall standardized mean difference effect size was modest (.262,  $p < .05$ ).

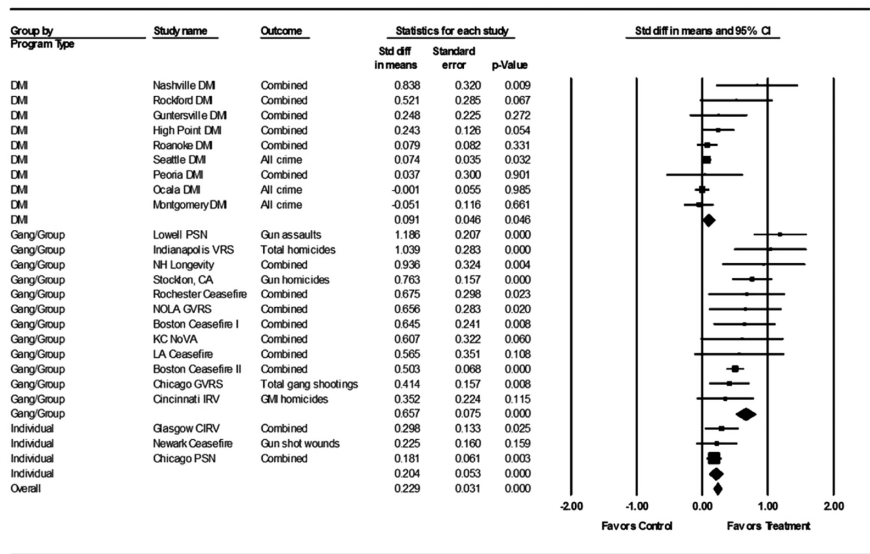
**Program type as effect size moderator.** Focused deterrence strategies have been directed at reducing crime by street gangs and criminally active groups, overt drug markets, and high-risk individuals. These programs represent differing applications of focused deterrence strategies to control distinct types of problems. The inclusion of moderator variables, such as program and research design types, helps to explain and understand differences across studies in the outcomes observed (Lipsey, 2003). Figure 2 presents a random effects model examining the mean effect sizes for the three different program types. It is important to note that the  $Q$ -statistic associated with the between-group variation was large and statistically significant ( $Q = 90.949$ ,  $df = 2$ ,  $p < .05$ ), which suggests that program type was influential in determining effect sizes. The gang/group intervention programs were associated with the largest within-group effect size (.657,  $p < .05$ ), followed by the high-risk individuals programs (.204,  $p < .05$ ) and the drug market intervention (DMI) programs (.091,  $p < .05$ ). When program type was included as a moderator, through the meta-analysis, we estimated a more modest overall effect size (.229,  $p < .05$ ).

The smaller mean effect size associated with the DMI programs was influenced by the noteworthy share of programs with reported threats to the integrity of the focused deterrence treatment. Not surprisingly, DMI programs that were implemented with higher treatment fidelity generated larger overall crime reduction impacts. As mentioned earlier, four of the nine (44.4%) eligible DMI programs suffered from implementation difficulties centered on securing the necessary community involvement in targeted drug market areas (Guntersville, Montgomery County, Peoria, and Roanoke). When treatment integrity was included as an effect size moderator for the nine DMI studies, programs with noted implementation issues had a smaller non-statistically significant mean effect size (.053). In contrast, the mean effect

12. Random effects models were used to estimate the overall standardized mean effect sizes. For the largest effect size meta-analysis,  $Q = 152.740$ ,  $df = 23$ ,  $p < .05$ . For the smallest effect size meta-analysis,  $Q = 109.537$ ,  $df = 9$ ,  $p < .05$ .

FIGURE 2

Mean Effect Sizes for Study Outcomes by Program Type



Note. Random effects model, total between-group heterogeneity,  $Q = 90.949$ ,  $df = 2$ ,  $p < .000$ .

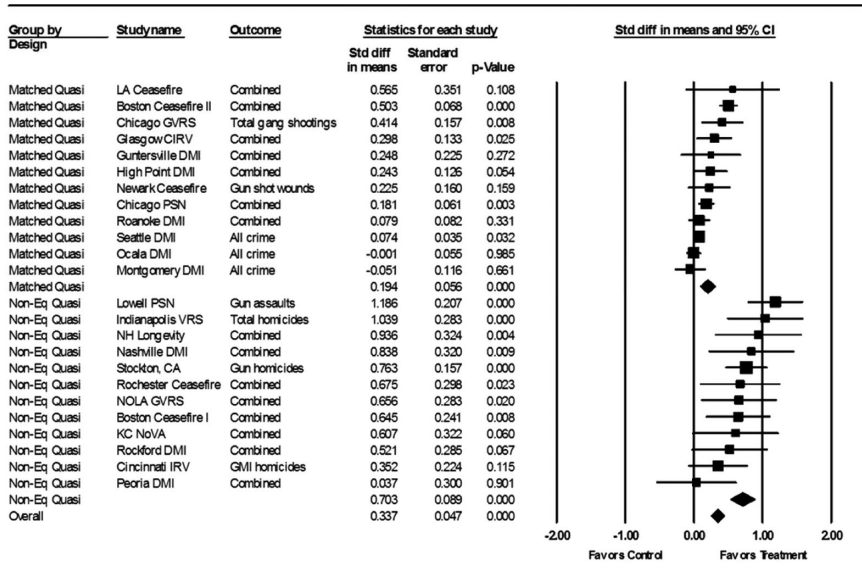
size for DMI programs without implementation difficulties suggested a modest, statistically significant crime reduction impact (.184,  $p < .05$ ).

**Research design as effect size moderator.** The meta-analysis in the previous iteration of the Campbell systematic review estimated a larger overall mean effect size (.604) relative to the current meta-analysis (.383). This difference is primarily a result of the greater prevalence of more rigorous quasi-experimental designs with higher levels of internal validity among the studies included in the current systematic review. It is well known among social scientists that program evaluations with more rigorous research designs tend to result in null effects. As Peter H. Rossi stated in his Iron Law of Evaluation, “The expected value of any net impact assessment of any large scale social program is zero” (1987: 3). And as he posited in his Stainless Steel Law of Evaluation, “The better designed the impact assessment of a social program, the more likely is the resulting estimate of net impact to be zero” (Rossi, 1987: 3). Given the important distinction in methodological quality between the nonequivalent and matched quasi-experimental studies, we examined research design as a moderator variable.

Figure 3 presents a random effects model examining the two different classes of quasi-experimental designs included in this review. It is important to note that the  $Q$ -statistic associated with the between-group variation was large and statistically significant

FIGURE 3

## Mean Effect Sizes for Study Outcomes by Design Type



Note. Random effects model, total between-group heterogeneity,  $Q = 57.117$ ,  $df = 1$ ,  $p < .000$ .

( $Q = 57.117$ ,  $df = 1$ ,  $p < .05$ ), which suggests that research design was influential in determining effect sizes. In this analysis, the nonequivalent quasi-experimental designs were associated with a much larger within-group effect size ( $.703$ ,  $p < .05$ ) relative to the matched quasi-experimental designs ( $.194$ ,  $p < .05$ ). When research design type was included as a moderator, the results of the meta-analysis revealed a more modest overall effect size ( $.337$ ,  $p < .05$ ).<sup>13</sup> Although the biases in quasi-experimental research are not clear (e.g., Campbell and Boruch, 1975; Wilkinson and Task Force on Statistical Inference, 1999), the findings from recent reviews in crime and justice demonstrate that weaker research designs often lead to more positive outcomes (see Weisburd, Lum, and Petrosino, 2001; Welsh, Peel, Farrington, Elffers, and Braga, 2011).

13. These findings are almost identical to the previous iteration of the Campbell focused deterrence systematic review. In the prior meta-analysis, the nonequivalent quasi-experimental designs had an effect size =  $.766$  ( $p < .05$ ), the matched quasi-experimental designs had an effect size =  $.196$  ( $p < .05$ ), and when research design type was included as a moderator, the overall effect size =  $.312$  ( $p < .05$ ). Nevertheless, as noted earlier, the current review has a larger share of matched quasi-experimental designs relative to nonequivalent quasi-experimental designs. As such, the overall mean effect size estimated in the current meta-analysis is smaller ( $.383$ ) compared with that of the original review ( $.604$ ).

### *Publication Bias*

Publication bias, generally defined as the concern that the collection of studies easily available to a reviewer represents those studies most likely to have statistically significant results, presents a strong challenge to any review of evaluation studies (Rothstein, 2008). The credibility of a review arguably depends more heavily on the collection of studies reviewed than on which statistical methods of synthesis are used (Wilson, 2009). Similar to the problem of a biased study sample leading to biased results in an individual study, a biased collection of studies will potentially lead to biased conclusions in a systematic review (Rothstein and Hopewell, 2009). As reported earlier, our search strategies were designed to mitigate the potential effects of publication bias on our analyses. Indeed, it is encouraging that more than one third of the eligible studies were acquired through grey literature sources.

Like many systematic reviews, we used the trim-and-fill procedure in our meta-analyses to explore whether publication bias might be affecting the results and to estimate how the reported effects would change if the bias were to be removed (Duval, 2005; Duval and Tweedie, 2000). The diagnostic funnel plot is based on the idea that, in the absence of bias, the plot of study effect sizes should be symmetric about the mean effect size. If there is asymmetry, the trim-and-fill procedure imputes the missing studies, adds them to the analysis, and then recomputes the mean effect size. Trim-and-fill procedures do suffer from some well-known limitations that could result in the underestimation or overestimation of publication bias (Rothstein, 2008; Simonsohn, Nelson, and Simmons, 2014).<sup>14</sup> Nonetheless, this approach does provide reviewers with a well-understood measure of the possible influence of bias on their meta-analytic results.

A visual inspection of the resulting funnel plot indicated some asymmetry with more studies with a large effect and a large standard error to the right of the mean than to the left of the mean. The trim-and-fill procedure determined that nine studies should be added to create symmetry. The funnel plot with imputed studies is presented in Figure 4. These additional studies modestly altered the mean effect size estimate. The mean random effect decreased from 0.383 (95% CI [0.264, 0.503]) to 0.215 (95% CI [0.098, 0.332]). Indeed, the 95% confidence intervals (CIs) overlap, which suggests that the mean effect sizes may be the same.

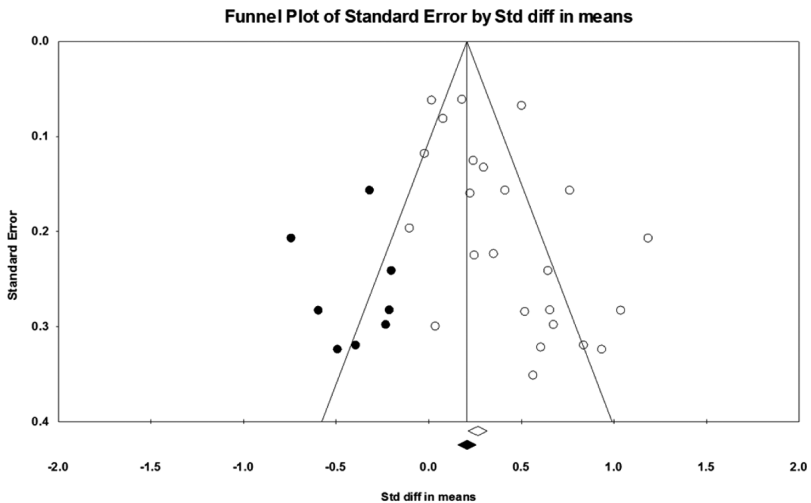
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14. As discussed by Rothstein (2008: 69), the trim-and-fill procedure is based on the notion that, in the absence of bias, a funnel plot of study effect sizes will be symmetric about the mean effect. If there are more small studies on one side than on the other side of the bottom of the funnel plot, there is concern that some studies may have been censored from the meta-analysis. The trim-and-fill approach imputes the missing studies, adds them to the analysis, and then recomputes the mean effect size. The most notable limitation is that in this approach, the observed asymmetry is assumed to be a result of publication bias rather than of true differences in the results of the small studies compared with those of the larger ones.



**FIGURE 4**

### **Funnel Plot for All Eligible Studies With Imputed Studies From Trim-and-Fill Analysis**



*Notes.* Empty circles are the original studies. Filled-in circles indicate imputed studies from the trim-and-fill analysis.

#### ***Displacement and Diffusion Effects***

In five studies, researchers conducted six tests of possible crime displacement and diffusion of crime control benefits associated with the evaluated focused deterrence programs (Table 2). These studies included gang/group violence reduction strategies (Boston II, Los Angeles), DMI programs (High Point, Nashville), and individual repeat offender strategies (Newark). In two of the four studies that included measurement of whether crime levels were impacted in areas immediately proximate to treatment areas, the authors reported noteworthy diffusion of crime control benefits associated with the focused deterrence intervention (Nashville, Los Angeles); none reported significant crime displacement effects into surrounding areas.

In two focused deterrence studies, researchers investigated the existence of displacement and diffusion effects on the criminal behavior of gangs that were socially connected to targeted groups. The Los Angeles intervention targeted two rival gangs operating out of the same area (Hollenbeck). Criminal activity (e.g., violent, gang, and gun crimes) was substantially reduced among the two gangs over a 6-month pre-post period. Slightly larger reductions in these crimes were evident among four nontargeted rival gangs in surrounding areas during the same time period. Part of the explanation for the diffusion effects may rest with fewer feuds between the targeted and nontargeted gangs. Tita et al. (2004) also speculated that diffusion effects may have been influenced by social ties among the targeted

and rival gangs. This seemed to be especially the case for gang crimes involving guns. In a companion paper to the main effects program evaluation, Braga et al. (2013) found that the Boston Ceasefire II strategy also created spillover deterrent effects onto other gangs that were socially connected to targeted gangs through rivalries and alliances. Total shootings involving these “vicariously treated” gangs decreased by 24% relative to total shootings involving matched comparison gangs.

## Discussion

The results of our review support the position that focused deterrence strategies do generate noteworthy crime control impacts. In 19 of the 24 eligible studies, researchers reported that the implementation of the evaluated program was associated with a statistically significant crime reduction effect on a targeted crime problem. The results of our meta-analysis of effect sizes suggests a statistically significant, moderate overall mean effect in favor of focused deterrence strategies. When these second-order effects were measured, focused deterrence programs did not result in significant crime displacement impacts. Rather, focused deterrence programs tended to generate diffusion of crime control benefits that extended into proximate areas and socially connected groups that did not receive direct treatments. These findings, in combination with the strong theoretical literature supporting the mechanisms of focused deterrence, provide solid support for the adoption of such programs by police agencies.

The strongest crime reduction impacts were associated with focused deterrence programs designed to reduce serious violence generated by ongoing conflicts among gangs and criminally active groups. Even when the integrity of the treatment applied was considered, DMI programs generated the smallest crime reduction impacts associated with the three different kinds of focused deterrence strategies. Given the large body of research findings that have shown the ineffectiveness of many police crime prevention efforts (Visser and Weisburd, 1998), the overall crime reduction impact generated by DMI programs is still noteworthy. Nevertheless, the smaller crime control benefits suggested by the findings of our meta-analysis are different from the effectiveness claims made from early applications of the approach. For instance, when reflecting on several short term, simple pre-test versus post-test, one-group-only comparisons of violent crime incidents in treated areas in High Point (NC), Kennedy and Wong (2009: 43) suggested “that it may be possible to close overt community drug markets and substantially reduce violent and drug-related crime.” In their more rigorous quasi-experimental evaluation of the High Point DMI, Corsaro et al. (2012) suggested a more modest 14% reduction in violent crime incidents associated with the approach.

It is interesting to note that these findings follow those that have been generated in studies of developmental prevention. After summarizing systematic reviews in this area, Farrington, Ttofi, and Lösel (2016) reported that programs focused on higher risk youth are more likely to be successful. In correctional evaluations, the importance of focusing on

high-risk offenders has also been a key element predicting program success (e.g., Andrews and Bonta, 2006). Our finding that the largest impacts are found for programs focused on the most violent offenders fits what has been observed in treatment programs more generally.

More than half of the eligible studies included in this updated systematic review were completed after the original Campbell review (Braga and Weisburd, 2012a, 2012b). Unfortunately, none of the newly identified studies responded to the original review's call that the next generation of focused deterrence program evaluations must shed some much needed light on the theoretical mechanisms underlying focused deterrence policing. Nearly all of the focused deterrence program evaluations included in this review could be described as "black box" evaluations where it is uncertain which program elements were most important in generating observed crime reduction effects.

Accordingly, even though we have a strong logic model for predicting positive outcomes in focused deterrence programs, we have little knowledge of which of the mechanisms underlying that model have the strongest impacts on outcomes. Deterrence certainly remains a key element to understanding why focused deterrence policing works. Nonetheless, it seems particularly important to assess how elements of procedural justice and collective efficacy influence program outcomes. In recent years, there has been growing concern not just about whether policing impacts on crime but also on how it affects communities. The President's Task Force on 21st Century Policing (2015), for example, identified public trust in the police to be the "first pillar" of policing. Although we do not have robust evidence on the mechanisms underlying prevention in most focused deterrence evaluations, the Chicago PSN quasi-experiment resulted in providing encouraging evidence for prevention mechanisms that would enhance public evaluations of legitimacy (Papachristos et al., 2007). The findings from the Chicago PSN evaluation show that direct communications with offenders in a procedurally just manner in the context of maintaining an enforcement environment enhances program effectiveness. This suggests potential for focused deterrence policing to be implemented in ways that are likely to increase legitimacy among offenders. We need more studies aimed at examining this and other potential mechanisms that may improve community outcomes.

None of the new studies were rigorous randomized controlled trial designs aimed at evaluating the crime reduction impacts of focused deterrence programs. This continues to be a key weakness in drawing conclusions about focused deterrence programs. Nevertheless, the findings from the updated review reveal that the quality of quasi-experimental evaluations of focused deterrence strategies has improved greatly over time. Contemporary quasi-experimental evaluations of focused deterrence strategies tend to include sophisticated statistical matching techniques, panel designs, and higher powered statistical models. Future evaluations of focused deterrence programs targeting repeat offenders and drug markets could be further strengthened by drawing on existing randomized experimental designs such as those used in the Hawaii Opportunity with Probation Enforcement (HOPE)

evaluation (Hawken and Kleiman, 2009), Jersey City Drug Market Analysis Program evaluation (Weisburd and Green, 1995), and other randomized experimental evaluations. It would be considerably more complicated to use randomized experimental designs to evaluate gang and criminally active group focused deterrence programs given that these interventions are intentionally aimed at generating spillover effects that could contaminate control gangs, groups, and areas. Braga and Weisburd (2014) suggested that multisite cluster randomized trial designs could be used to conduct more rigorous evaluations of gang and group violence reduction strategies.<sup>15</sup>

It is problematic, however, to implement programs that falsely raise citizen expectations of large violent crime reductions and dramatic changes in the quality of residential life in neighborhood suffering from persistent drug and violent crime problems. As Phil Cook suggested (2012: 162), “the quest for a miracle cure for crime and violence sometimes leads to an early or excessive embrace of an unproven technology.” It is much more prudent to take a skeptical approach to policy interventions until a portfolio of proven practices has been developed. The available focused deterrence evaluation evidence suggests that the approach does indeed reduce crime. Nevertheless, as the quality of program evaluations continue to improve, the impacts of focused deterrence programs seem to be much more modest relative to the large violence reduction and quality-of-life improvements described in earlier accounts (e.g., Braga et al., 2001; Kennedy and Wong, 2009).

The existing empirical evidence suggests that “person-focused” policing interventions associated with the standard model of policing, such as programs designed to arrest and prosecute repeat offenders, were not effective in controlling crime (National Research Council, 2004). In contrast, the evaluation evidence reviewed here leads us to conclude that focused deterrence strategies, designed to change offender behavior through a blended enforcement, social service and opportunity provision, and community-based action approach, are effective in controlling crime. Other key programmatic elements include strategic analyses of targeted crime problems and a well-developed communications plan designed to make targeted offenders understand the new regime that is being imposed on them.

The results reported in the available literature further reveal that focused deterrence strategies, especially DMI programs, may be difficult to implement and that these

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15. Cluster randomized experiments represent a variation of the classic randomized controlled trial design in which clusters (groups) of subjects, rather than individual subjects, are randomly allocated to treatment and control conditions. This design allows for better control of treatment “contamination” across individual subjects. In the case of gang violence, this contamination is the stable unit treatment valuation assumption (SUTVA) problem generated by social connections among gangs. In a multisite cluster randomized trial, clusters of subjects are randomly allocated to treatment and control conditions in two or more sites. Randomly allocating distinct clusters of gangs connected by rivalries and alliances to treatment and control conditions limits the treatment contamination problem. Researchers in each participating city would need to identify gang conflict and alliance networks and apply social network analysis techniques to specify distinct socially connected cliques of gangs. They would also need to track shootings by specific gangs during pre-intervention and post-intervention time periods in participating cities.

challenges can undermine their crime control efficacy in certain jurisdictions. It is important to recognize that successful focused deterrence programs follow a deliberate strategy development process rather than the simple adoption of tactics applied in other jurisdictions. Consistent with its problem-oriented policing roots, the adoption of the focused deterrence framework requires local jurisdictions to conduct careful upfront research on the nature of targeted crime problems to customize a response to identified underlying conditions and dynamics that fits both local community contexts and the operational capacities of criminal justice, social service, and community-based agencies. The successful implementation of focused deterrence strategies requires the establishment of a “network of capacity” consisting of dense and productive relationships among these diverse partnering agencies (see Braga and Winship, 2006). Cities without robust networks in place have found it difficult to implement and sustain focused deterrence strategies.

Comparative research on applications of focused deterrence strategies in other countries is also needed to determine whether these violence reduction policies and practices can be transferred to settings outside U.S. urban environments. Experiences in Glasgow, Scotland, suggest that the approach may be beneficial in addressing serious youth violence problems in other Western countries (Deuchar, 2013). Nevertheless, implementation in more challenging global environments, such as Turkey and Brazil (National Network for Safe Communities, 2013), represent strong tests for the focused deterrence approach. Many questions need to be answered. For instance, is it possible to develop a network of capacity that could mobilize communities to complement law enforcement efforts to control the violent behaviors of drug gangs in severely disadvantaged favelas of Rio de Janeiro? When we draw on the positive experiences in developing such capacities in violent disadvantaged neighborhoods in the United States, it seems possible. Indeed, the flexible problem-solving framework undergirding focused deterrence strategies suggests that the approach can be appropriately tailored to varying urban contexts. At this point in time, the potential violence reduction efficacy of these approaches in other countries is mainly based on speculation rather than on empirical facts and practical experience. Experimentation with focused deterrence strategies to control crime problems beyond U.S. settings, however, is clearly warranted by the available scientific evidence.

## Conclusion

Focused deterrence strategies are a recent addition to the existing scholarly literature on crime control and prevention strategies. Although the evaluation evidence needs to be strengthened with rigorous randomized experimental field trials, and more developed study of the theoretical mechanisms underlying its impacts, our review suggests that jurisdictions suffering from gang violence, overt drug markets, and repeat offender problems should add focused deterrence strategies to their existing portfolio of prevention and control interventions. The existing evidence suggests these new approaches to crime prevention and control generate noteworthy crime reductions. At the same, however, jurisdictions looking

to implement focused deterrence programs need guidance on the key operational elements of these varied approaches. As evaluation evidence and practical experience continues to accumulate, a premium must be placed on identifying these complementary crime control mechanisms and on isolating their impacts on targeted crime problems.

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