The Effects of "Pulling Levers" Focused Deterrence Strategies on Crime

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Campbell Systematic Reviews

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Abstract

BACKGROUND

A number of American police departments have been experimenting with new problem-oriented policing frameworks to prevent gang and group-involved violence generally known as the "pulling levers" focused deterrence strategies. Focused deterrence strategies honor core deterrence ideas, such as increasing risks faced by offenders, while finding new and creative ways of deploying traditional and non-traditional law enforcement tools to do so, such as directly communicating incentives and disincentives to targeted offenders. Pioneered in Boston to halt serious gang violence, the focused deterrence framework has been applied in many American cities through federally sponsored violence prevention programs. In its simplest form, the approach consists of selecting a particular crime problem, such as gang homicide; convening an interagency working group of law enforcement, social-service, and community-based practitioners; conducting research to identify key offenders, groups, and behavior patterns; framing a response to offenders and groups of offenders that uses a varied menu of sanctions ("pulling levers") to stop them from continuing their violent behavior; focusing social services and community resources on targeted offenders and groups to match law enforcement prevention efforts; and directly and repeatedly communicating with offenders to make them understand why they are receiving this special attention. These new strategic approaches have been applied to a range of crime problems, such as overt drug markets and individual repeat offenders, and have shown promising results in the reduction of crime.

OBJECTIVES

To synthesize the extant evaluation literature and assess the effects of pulling levers focused deterrence strategies on crime.

SELECTION CRITERIA

Eligible studies had to meet three criteria: (1) the program had to have the core elements of a pulling levers focused deterrence strategy present; (2) a comparison group was included; (3) at least one crime outcome was reported. The units of analysis had to be people or places.

SEARCH STRATEGY

Several strategies were used to perform an exhaustive search for literature fitting the eligibility criteria. First, a keyword search was performed on an array of online abstract databases. Second, we reviewed the bibliographies of past narrative and empirical reviews of literature that examined the effectiveness of pulling levers focused deterrence programs. Third, we performed forward searches for works that have cited seminal focused deterrence studies. Fourth, we searched bibliographies of narrative reviews of police crime prevention efforts and past completed Campbell systematic reviews of police crime prevention efforts. Fifth, we performed hand searches of leading journals in the field.

DATA COLLECTION AND ANALYSIS

For our ten eligible studies, we complete a narrative review of effectiveness and a formal meta-analysis of the main effects of these programs on reported crime outcomes.

MAIN RESULTS

Based on our narrative review, we find that nine of the ten eligible evaluations reported statistically significant reductions in crime. It is important to note here that all ten evaluations used nonrandomized quasi-experimental designs. No randomized controlled trials were identified by our search strategies. Our meta-analysis suggests that pulling levers focused deterrence strategies are associated with an overall statistically-significant, medium-sized crime reduction effect.

CONCLUSIONS

We conclude that pulling levers focused deterrence strategies seem to be effective in reducing crime. However, we urge caution in interpreting these results because of the lack of more rigorous randomized controlled trials in the existing body of scientific evidence on this approach.

1 Background

Deterrence theory posits that crimes 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 focuses on the effect of changing certainty, swiftness, and severity of punishment associated with certain acts on the prevalence of those crimes (see, e.g. Apel and Nagin, 2011; Blumstein, Cohen, and Nagin, 1978; Cook, 1980; Nagin, 1998; Paternoster, 1987).

In recent years scholars have begun to argue that police interventions provide an effective approach for gaining both special and general deterrence against crime. A series of experimental and quasi-experimental studies have shown that the police can be effective in preventing crime (Braga, 2001, 2005; Skogan and Frydl, 2004; Weisburd and Eck, 2004), and that such crime prevention benefits are not offset by displacement of crime to areas near to police interventions (Braga, 2001; Weisburd et al., 2006). Durlauf and Nagin have drawn from this literature to argue that "increasing the visibility of the police by hiring more officers and by allocating existing officers in ways that heighten the perceived risk of apprehension consistently seem to have substantial marginal deterrent effects" (2011: 14). Indeed, they conclude that crime prevention in the U.S. would be improved by "shifting resources from imprisonment to policing" (2011: 9-10).

A recent innovation in policing that capitalizes on the growing evidence of the effectiveness of police deterrence strategies is the "focused deterrence" framework, often referred to as "pulling-levers policing" (Kennedy, 1997, 2008). Pioneered in Boston as a problem-oriented policing project to halt serious gang violence during the 1990s (Kennedy, Peel, and Braga, 1996), the focused deterrence framework has been applied in many U.S. cities through federally sponsored violence prevention programs such as the Strategic Alternatives to Community Safety Initiative and Project Safe Neighborhoods (Dalton, 2002). Focused deterrence strategies honor core deterrence ideas, such as increasing risks faced by offenders, while finding new and creative ways of deploying traditional and non-traditional law enforcement tools

to do so, such as directly communicating incentives and disincentives to targeted offenders (Kennedy, 1997, 2008).

In its simplest form, the approach consists of selecting a particular crime problem, such as youth homicide; convening an interagency working group of law enforcement, social-service, and community-based practitioners; conducting research to identify key offenders, groups, and behavior patterns; framing a response to offenders and groups of offenders that uses a varied menu of sanctions ("pulling levers") to stop them from continuing their violent behavior; focusing social services and community resources on targeted offenders and groups to match law enforcement prevention efforts; and directly and repeatedly communicating with offenders to make them understand why they are receiving this special attention (Kennedy, 1997, 2006). The focused deterrence approach is also consistent with recent theorizing about police innovation, which suggests that approaches that seek to both create more focus in application of crime prevention programs and that expand the tools of policing are likely to be most successful (Weisburd and Eck, 2004).

In the United States, without the support of a formal evaluation, Boston's Operation Ceasefire pulling levers strategy was hailed as an unprecedented success (see, e.g. Butterfield, 1996; Witkin, 1997). These claims followed a surprising large decrease in youth homicide after the strategy was fully implemented in mid May 1996. However, more rigorous examinations of youth homicide in Boston soon followed. A U.S. Department of Justice-sponsored quasi-experimental evaluation of Operation Ceasefire that the intervention was associated with a 63% decrease in monthly number of Boston youth homicides (Braga et al., 2001). Other examinations of violence trends in Boston, however, were more skeptical of the violence prevention value of Boston's approach (see, e.g., Fagan, 2002; Rosenfeld et al., 2005).

Since the publication of the Boston Ceasefire evaluation, several evaluations of similar focused deterrence strategies implemented in other U.S. cities, such as Chicago (Papachristos et al., 2007), Los Angeles (Tita et al., 2003), and Indianapolis (McGarrell et al., 2006), have been published. These evaluations reported similar noteworthy crime reduction effects associated with the focused deterrence approach. The basic principles of the focused deterrence approach have also been applied to overt drug market problems (Kennedy, 2009) and repeat offending by substance-abusing probationers (Hawken and Kleiman (2009) with positive crime prevention gains reported.

Given the growing popularity of pulling levers focused deterrence strategies in the United States, as well as the conflicting views on its crime prevention value, a systematic review of the empirical evidence on the effects of focused deterrence interventions on crime is necessary to assess the value of this approach to crime prevention.

2   Objectives

The objective of this review is to synthesize the existing published and non-published empirical evidence on the effects of pulling levers focused deterrence strategies on crime and to provide a systematic assessment of the preventive value of this approach. Often, these new approaches represent a specific application of deterrence strategies within a problem-oriented policing framework (Kennedy, 2006; Braga, 2008a; see also Goldstein, 1990; Eck and Spelman, 1987). In short, many focused deterrence strategies use the iterative problem-oriented policing process (scanning, analysis, response, and assessment) to frame an interagency response to deter groups of chronic offenders from continuing their ongoing violent conflicts. More recently, however, these strategic applications of focused deterrence principles have been applied to a wider range of problems such as overt drug markets (Kennedy, 2009). This review will include this wider range of focused deterrence strategies.

3 Methods

3.1 INCLUSION AND EXCLUSION CRITERIA

3.1.1 Types of Studies

Only studies that used comparison group designs involving before and after measures were eligible for the main analyses of this review. In several well-known pulling levers focused deterrence evaluations (e.g. Braga et al., 2001; McGarrell et al., 2006), the comparison group experienced routine modern police responses to crime that includes a blend of traditional police responses (e.g., random patrol, rapid response, and ad-hoc investigations) and opportunistic community problem-oriented responses. While strategic interventions developed from community and problem-oriented policing initiatives may be present in certain control areas, none of the comparisons implemented focused deterrence strategies to address crime problems. The comparison group study had to be either experimental or quasi-experimental (nonrandomized) (Campbell and Stanley, 1966; Cook and Campbell, 1979).

3.1.2 Types of Units of Analysis

Pulling levers strategies attempt to influence the criminal behavior of individuals through the strategic application of enforcement and social service resources to facilitate desirable behaviors. However, existing reviews of the crime prevention value of pulling levers strategies have noted that published evaluations only report aggregated measures of underlying levels of criminal behaviors in targeted areas (Wellford et al., 2005). For example, in Boston, the pulling levers intervention targeted violent behavior among gang-involved offenders. The evaluators measured behavioral change among gang-involved offenders by examining city-wide trends in aggregated measures of serious violence (Braga et al., 2001).

3.1.3 Types of Interventions

To be eligible for this review, interventions had to be identified as a focused deterrence strategy. As described by Kennedy (2006: 156-157), pulling levers operations have tended to follow this basic framework:

- Selection of a particular crime problem, such as youth homicide or street drug dealing.

- Pulling together an interagency enforcement group, typically including police, probation, parole, state and federal prosecutors, and sometimes federal enforcement agencies.
- Conducting research, usually relying heavily on the field experience of front-line police officers, to identify key offenders – and frequently groups of offenders, such as street gangs, drug crews, and the like – and the context of their behavior.
- Framing a special enforcement operation directed at those offenders and groups of offenders, and designed to substantially influence that context, for example by using any and all legal tools (or levers) to sanction groups such as crack crews whose members commit serious violence.
- Matching those enforcement operations with parallel efforts to direct services and the moral voices of affected communities to those same offenders and groups.
- Communicating directly and repeatedly with offenders and groups to let them know that they are under particular scrutiny, what acts (such as shootings) will get special attention, when that has in fact happened to particular offenders and groups, and what they can do to avoid enforcement action. One form of this communication is the "forum," "notification," or "call-in," in which offenders are invited or directed (usually because they are on probation or parole) to attend face-to-face meetings with law enforcement officials, service providers, and community figures.

We used this basic framework to assist in our determination whether particular programs engaged the focused deterrence approach. It is important to note here, however, that certain programs that were determined to be eligible for this review did not necessarily follow the very specific pulling levers steps identified by Kennedy (2006). Pulling levers focused deterrence 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. As such, we fully anticipated a variety of pulling levers focused deterrence strategies to be identified by our systematic review.

3.1.4 Types of Outcome Measures

Eligible studies must measure the effects of the focused deterrence intervention on officially recorded levels of crime at places or crime by individuals. Appropriate crime measures included crime incident reports, citizen emergency calls for service, and arrest data. Particular attention was paid to studies that measured crime displacement effects and diffusion of crime control benefit effects. For instance, Kennedy (2006) described a place-based application of pulling levers focused on a disorderly drug market operating in High Point, North Carolina. Crime prevention strategies focused on specific locations have been criticized as resulting in displacement (see Repetto, 1976). More recently, academics have observed that crime prevention programs may result in the complete opposite of displacement—that crime control benefits were greater than expected and "spill over" into places

beyond the target areas (Clarke and Weisburd, 1994). The quality of the methodologies used to measure displacement and diffusion effects, as well as the types of displacement (spatial, temporal, target, modus operandi) examined, was also assessed.

3.2   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 was performed on an array of online abstract databases (see lists of keywords and databases below). Second, we reviewed the bibliographies of past narrative and empirical reviews of literature that examined the effectiveness of pulling levers focused deterrence programs (Kennedy, 2006, 2008; Wellford et al., 2005; Skogan and Frydl, 2004). Third, we performed forward searches for works that have cited seminal pulling levers focused deterrence studies (Kennedy et al., 1996; Kennedy, 1997; Braga et al., 2001; McGarrell et al., 2006; Papachristos et al., 2007). Fourth, we searched bibliographies of narrative reviews of police crime prevention efforts (Braga, 2008a; Braga, Kennedy, and Tita, 2002; Sherman, 2002; Weisburd and Eck, 2004) and past completed Campbell systematic reviews of police crime prevention efforts (Braga, 2007; Mazerolle et al., 2007; Weisburd et al., 2008). Fifth, we performed hand searches of leading journals in the field.[1]

Our searches were all completed between May and September 2010. Thus, our review only covers studies published in 2010 and earlier. Sixth, after finishing the above searches and reviewing the studies as described later, we e-mailed the list of studies meeting our eligibility criteria in September 2010 to leading criminology and criminal justice scholars knowledgeable in the area of pulling levers focused deterrence strategies. These 90 scholars were defined as those who authored at least one study which appeared on our inclusion list, anyone involved with the National Academy of Sciences reviews of police research (Skogan and Frydl, 2004) and firearms research (Wellford et al., 2005), and other leading scholars identified by the authors (see Appendix A). This helped us identify studies the above searches left out as these experts were able to refer us to studies we missed, particularly unpublished studies. Finally, we consulted with an information specialist at the outset of our review and at points along the way in order to ensure that we used

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[1] 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 1979-2009.

appropriate search strategies to identify the studies meeting the criteria of this review.[2]

The following fifteen databases were searched:

Criminal Justice Periodical Index

1. Sociological Abstracts
2. Social Science Abstracts (SocialSciAbs)
3. Social Science Citation Index
4. Arts and Humanities Search (AHSearch)
5. Criminal Justice Abstracts
6. National Criminal Justice Reference Service (NCJRS) Abstracts
7. Educational Resources Information Clearinghouse (ERIC)
8. Legal Resource Index
9. Dissertation Abstracts
10. Government Publications Office, Monthly Catalog (GPO Monthly)
11. Google Scholar
12. Online Computer Library Center (OCLC) SearchFirst
13. CINCH data search
14. C2 SPECTR (The Campbell Collaboration Social, Psychological, Educational and
15. Criminological Trials Register)

The following terms were used to search the fifteen databases listed above:

1) Pulling levers AND police
2) Problem-oriented policing
3) Police AND repeat offenders
4) Police AND gangs
5) Police AND guns
6) Gang violence prevention
7) Focused deterrence
8) Deterring violent offenders
9) Strategic gang enforcement
10) Crackdowns AND gangs
11) Enforcement swamping
12) Drug market intervention

3.3   DETAILS OF STUDY CODING CATEGORIES

All eligible studies were coded (see coding protocol attached in Appendix B) on a variety of criteria including:

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[2] Ms. Phyllis Schultze of the Gottfredson Library at the Rutgers University School of Criminal Justice executed the initial abstract search and was consulted throughout on our search strategies.

1. Reference information (title, authors, publication etc.)
2. Nature of description of selection of site, problems etc.
3. Nature and description of selection of comparison group or period
4. The unit of analysis
5. The sample size
6. Methodological type (randomized experiment or quasi-experiment)
7. A description of the pulling levers intervention
8. Dosage intensity and type
9. Implementation difficulties
10. The statistical test(s) used
11. Reports of statistical significance (if any)
12. Effect size/power (if any)
13. The conclusions drawn by the authors

Two graduate research assistants from the Program in Criminal Justice Policy and Management at Harvard University independently coded each eligible study. Where there were discrepancies, Dr. Braga reviewed the study and determined the final coding decision.

3.4   STATISTICAL PROCEDURES AND CONVENTIONS

Analysis of outcome measures across studies were carried out in a uniform manner and, when appropriate and possible, involved quantitative analytical methods. We used meta-analyses of program effects to determine the size and direction of the effects and weighting effect sizes based on the variance of the effect size and the study sample size (Lipsey and Wilson, 2001). In this systematic review, we used the standardized mean difference effect size (also known as Cohen's d; see Rosenthal, 1994). We employed the Effect Size Calculator, developed by David B. Wilson and available on the Campbell Collaboration's web site, to calculate standardized mean difference effect sizes for reported outcomes in each study. [3] We then used Biostat's Comprehensive Meta Analysis Version 2.2 to conduct the meta-analysis of effect sizes. The specific approaches used to calculate effect sizes for each outcome in the eligible studies are described in the meta-analysis section.

3.4.1 Determination of Independent Findings

One problem in conducting meta-analyses in crime and justice is that investigators often did 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. For example, the Boston Operation Ceasefire evaluation presents an array of outcome measures including youth homicides, shots fired calls for service, gun assault incidents, and youth gun assault incidents in one high-risk district (Braga et al., 2001). However, the lack of prioritization of outcomes in a study raises the question of how to derive an overall effect of treatment. For

[3] http://www.campbellcollaboration.org/resources/effect_size_input.php

example, the reporting of one significant result may reflect a type of "creaming" in which the authors focus on one 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.

We analyze the studies using three approaches. The first is conservative in the sense that it combines all reported outcomes reported 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. This was true for the Boston Operation Ceasefire evaluation, which examined a wider range of serious gun violence outcome measures, but assumed that the largest program effects given the intervention would be found in the case of youth homicide incidents (Braga et al., 2001). Finally, we present the smallest effect size for each study. This approach is the most conservative and likely underestimates the effect of pulling levers focused deterrence on crime. We use it here primarily to provide a lower bound to our findings.

4 Findings

4.1 SELECTION OF STUDIES

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. The four search strategies produced 2,473 distinct abstracts. The contents of these abstracts were reviewed for any suggestion of an evaluation of pulling levers interventions. 93 distinct abstracts were selected for closer review and the full-text reports, journal articles, and books for these abstracts were acquired and carefully assessed to determine whether the interventions involved pulling levers focused deterrence strategies and whether the studies used randomized controlled trial designs or nonrandomized quasi-experimental designs. Ten eligible studies were identified and included in this review:

1. Operation Ceasefire in Boston, Massachusetts (Braga, Kennedy, Waring, and Piehl, 2001)
2. Indianapolis Violence Reduction Partnership in Indianapolis, Indiana (McGarrell, Chermak, Wilson, and Corsaro, 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, Baker, Skubak Tillyer, Dunham, Hall, Ozer, Henson, and Godsey, 2009)
6. Operation Ceasefire in Newark, New Jersey (Boyle, Lanterman, Pascarella, and Cheng, 2010)
7. Operation Ceasefire in Los Angeles, California (Tita, Riley, Ridgeway, Grammich, Abrahamse, and Greenwood, 2003)
8. Project Safe Neighborhoods in Chicago, Illinois (Papachristos, Meares, and Fagan, 2007)
9. Drug Market Intervention in Nashville, Tennessee (Corsaro and McGarrell, 2009)
10. Drug Market Intervention in Rockford, Illinois (Corsaro, Brunson, and McGarrell, 2010)

4.2   CHARACTERISTICS OF ELIGIBLE STUDIES

The ten selected studies examined pulling levers focused deterrence interventions that were implemented in small, medium, and large U.S. cities (Table 1).[4] Three of the eligible evaluations (Cincinnati, Nashville, and Newark) were not published at the time the review of abstracts was completed.[5] All ten included evaluations that were released after 2000 and seven were completed after 2007. Six studies evaluated the crime reduction effects of pulling levers strategies on serious violence generated by street gangs or criminally-active street groups (Boston, Cincinnati, Indianapolis, Los Angeles, Lowell, and Stockton). Two studies evaluated strategies focused on reducing crime driven by street-level drug markets (Nashville and Rockford) and are generally called "Drug Market Intervention" (DMI) pulling levers focused deterrence strategies. Two studies evaluated crime reduction strategies that were focused on individuals (Chicago and Newark).

The two crime reduction strategies focused on individuals represented deviations from the classic pulling levers focused deterrence approach developed in Boston and defined by Kennedy (2006). However, after a careful review of program elements, we determined that the necessary components of an eligible study were present. As an additional check on our selections, we contacted academics associated with each of the two programs and reviewed our decisions with them.[6] All concurred with our decision to include these programs in the review.

Boyle et al. (2010) described Newark's "Operation Ceasefire" strategy as focused on preventing gun violence by criminally-active individuals that was a hybrid of the Boston Ceasefire pulling levers model (Kennedy et al., 1996) and the Chicago CeaseFire public health approach that uses trained street outreach staff, public education campaigns, and community mobilization to prevent shootings (Skogan et al., 2008). The Chicago Project Safe Neighborhoods study evaluated the violence reduction effects of a strategy comprised of four key interventions: (1) increased federal prosecutions for convicted felons carrying or using guns, (2) lengthy sentences associated with federal prosecutions, (3) supply-side firearm policing

[4] During our search for eligible studies, several scholars suggested that the systematic review include the Hawaii Opportunity with Probation Enforcement randomized controlled trial (Hawken and Kleiman, 2009). While this program represented a departure from our selection criteria, we agree that the deterrence mechanisms in HOPE are very similar to the mechanisms engaged by the ten pulling levers focused deterrence evaluations included in this report. We included HOPE in an earlier version of this review (Braga and Weisburd, 2012) and discuss the program and its implications for this review in Appendix E.
[5] During the development of this report, the Newark study was accepted for publication at *Justice Research and Policy* and the Nashville study was accepted for publication at *Evaluation Review.*
[6] For Newark's Operation Ceasefire, we contacted Professor George Kelling of Rutgers University. Kelling led the research team that was primarily responsible for the development and implementation of the program evaluated by Boyle et al. (2010). For Chicago Project Safe Neighborhoods, we contacted Professor Andrew Papachristos of the University of Massachusetts, Professor Tracey Meares of Yale Law School, and Professor Jeffrey Fagan of Columbia University.

activities, and (4) social marketing of deterrence and social norms messages through offender notification meetings (Papachristos et al., 2007).

All ten eligible studies used quasi-experimental designs to analyze the impact of pulling levers focused deterrence strategies on crime. Seven evaluations used quasi-experimental designs with non-equivalent comparison groups (Boston, Cincinnati, Indianapolis, Lowell, Nashville, Rockford, and Stockton). Two evaluations used quasi-experimental designs with comparison groups created through matching techniques (Chicago and Newark). One evaluation used a quasi-experimental design that included both non-equivalent comparison groups and comparison groups created through matching techniques (Los Angeles). Table 2 provides a brief summary of the treatment, units of analysis, and research design used by the ten eligible studies.

Five studies evaluated the crime reduction effects of focused deterrence strategies by comparing trends in key outcome variables in a targeted geographic area (identified as a neighborhood, policing district, or well-defined zone) to trends in key outcome variables in comparison areas. The Chicago study used propensity score matching techniques to identify very similar comparison policing districts to compare against the targeted policing districts. The Los Angeles study used two non-equivalent comparisons (the target area relative to the remainder of the larger neighborhood; the targeted neighborhood relative to the surrounding larger geographic community area). The Los Angeles study also used propensity score matching techniques to identify similar Census block groups to compare against the Census block groups that comprised the targeted area. The Newark evaluation used crime mapping technology and simple matching techniques to identify a comparison gun hot spot area that was similar to the targeted Ceasefire zone in terms of gunshot wounding incidents, geographic size, and socio-demographic characteristics. The Nashville and Rockford studies compared crime trends in targeted neighborhoods relative to crime trends in the surrounding County and city areas, respectively.

Five studies evaluated the crime reduction effects of citywide pulling levers interventions. The Boston, Indianapolis, Lowell, and Stockton quasi-experimental designs compared citywide trends in key outcomes to citywide trends in key outcomes in sets of non-equivalent cities that did not experience a pulling levers intervention during the study time period. The Cincinnati evaluation compared citywide trends in homicides involving members of criminally-active groups targeted by the pulling levers intervention relative to trends in homicides that did not involve members of criminally-active groups.

Three studies examined possible immediate spatial crime displacement and diffusion of crime control benefits that may have been generated by the pulling levers interventions (Los Angeles, Nashville, and Newark). Only one study noted potential threats to the integrity of the treatment. Tita et al. (2003) reported that the Los Angeles pulling levers 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.

4.3 NARRATIVE REVIEW OF THE EFFECTS OF PULLING LEVERS FOCUSED DETERRENCE STRATEGIES ON CRIME

This section provides a brief narrative review of the effects of the eligible pulling levers focused deterrence interventions on crime. Table 3 summarizes the main effects of the intervention on crime outcomes and, when measured, crime displacement and diffusion of crime control benefits effects. A more detailed narrative review of the focused deterrence strategies contained in the eligible studies is provided in Appendix C. Nine of the ten evaluations of pulling levers focused deterrence strategies reported statistically-significant crime reduction effects associated with the approach. While the authors did report a noteworthy non-statistically significant reduction in gunshot wound incidents, only the evaluation of Newark's Operation Ceasefire did not report any statistically significant crime prevention benefits generated by the violence reduction strategy.

Evaluations of focused deterrence strategies targeting gangs and criminally-active groups reported large statistically-significant reductions in violent crime; these results included: a 63% reduction in youth homicides in Boston, 44% reduction in gun assault incidents in Lowell, 42% reduction in gun homicides in Stockton, 35% reduction in homicides of criminally-active group members in Cincinnati, 34% reduction in total homicides in Indianapolis, and significant short-term reductions in violent crime in Los Angeles. The two drug market intervention evaluations also reported statistically significant crime reductions. In Nashville, the drug market intervention generated a 55% reduction in illegal drug possession incidents. In Rockford, the drug market intervention generated a 22% reduction in non-violent offenses. While Newark's strategy did not generate statistically significant crime control gains when high-rate offenders were targeted, the other two individual-focused programs reported statistically significant crime reductions. In Chicago, the PSN intervention was associated with a 37% reduction in homicide.

Two of the three studies that measured possible crime displacement and diffusion effects reported noteworthy diffusion of crime control benefits associated with the focused deterrence intervention. Consistent with the absence of a treatment effect, the Newark evaluation did not report any statistically significant crime displacement or diffusion effects. The Nashville evaluation reported statistically significant reductions in drug offenses and total calls for service in the non-treated area immediately adjoining the targeted drug market area. The Los Angeles evaluation found statistically-significant reductions in violent crime in treatment census block groups surrounding the treatment census block groups as well as noteworthy

reductions in violent offending by non-treated gangs that were "socially-tied" to treatment gangs.

To test the statistical significance of the observed distribution of crime reduction effects reported by the ten eligible studies, we used an application of the binomial distribution known as the sign test (Blalock, 1979). This simple test examines the probabilities of getting an observed proportion of successes from a population of equal proportions of successes and failures. Nine of the ten studies (90.0%) reported noteworthy crime reductions associated with the pulling levers approach. According to the sign test, this result was statistically significant (exact binomial two tailed probability = .0215).

4.4 META-ANALYSIS OF THE EFFECTS OF PULLING LEVERS STRATEGIES ON CRIME

It is important to note here that, while the impacts of the pulling levers focused deterrence programs were measured at larger areal units (city, policing area, targeted zone, census unit), these interventions were highly focused on a small number of risky individuals and risky groups who tend to commit their crimes at a relatively small number of high crime places within those larger areas. Pulling levers focused deterrence strategies are highly targeted interventions that are not broadly diffused across large populations or large areas. Computation of effect sizes in the studies was not always direct. The goal was to convert all observed effects into a standardized mean difference effect size metric. None of the studies we examined calculated standardized effect sizes, and indeed, it was sometimes difficult to develop precise effect size metrics from published materials. This reflects a more general problem in crime and justice with "reporting validity" (Farrington, 2006; Lösel and Köferl, 1989), and has been documented in recent reviews of reporting validity in crime and justice studies (see Perry and Johnson, 2008; Perry et al., 2010).

As described earlier, David B. Wilson's Effect Size Calculator was used to calculate the standardized mean difference effect sizes for all outcomes in the eligible studies. For the Boston, Lowell, and Stockton studies, we calculated standardized mean effect sizes based on newly-estimated Ordinary Least Squares regression models that used the same covariates and modeling choices as the count regression models in the original evaluations.[7] For the Chicago, Cincinnati, Indianapolis, Nashville,

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[7] We could not find a satisfactory method of calculating effect sizes based on the reported treatment coefficients estimated by the Poisson and negative binomial regression models used in these evaluations. Braga was involved as the lead author of the Boston, Lowell, and Stockton studies and ran the new OLS regressions from the original study data. The treatment dummy variables in the new models produced an "adjusted" mean difference for the intervention effect for each of the outcome variables. The unstandardized regression coefficients, standard deviation of the dependent variables, and treatment and control Ns were entered into the Effect Size Calculator to calculate the standardized mean difference effect size. It is important to note that, despite the change from count regression

Newark, and Rockford, we calculated standardized mean effect sizes based on the t-test results reported for the intervention variables' effects on the outcome variables.[8] For the Los Angeles study, we acquired the original evaluation data, calculated the mean gain scores, pre-test and post-test standard deviations, and paired t-tests, and entered these data into the Effect Size Calculator to estimate the standardized mean difference effect sizes. In Appendix C, we provide effect sizes for each outcome for the ten eligible studies.

Using the mean effect criterion for all eligible studies, the forest plots in Figure 1 show the standardized difference in means between the treatment and control or comparison conditions (effect size) with a 95 percent confidence interval plotted around them for all eligible studies. Points plotted to the right of 0 indicate a treatment effect; in this case, the study showed a reduction in crime or disorder. Points to the left of 0 indicate a backfire effect where control conditions improved relative to treatment conditions. Since the Q statistic which was significant at the $p < .05$ level (Q = 41.752, df = 9), we used a random effects model to estimate the overall mean effect size based on a heterogeneous distribution of effect sizes. The meta-analysis of effect sizes suggests a strongly significant effect in favor of pulling levers focused deterrence strategies. The overall effect size for these studies is .604 (see Cohen, 1988). This is above Cohen's standard for a medium effect of .50 and below that of a large effect at .80 (Cohen, 1988). Nonetheless, the overall effect size is relatively large compared to assessments of interventions in crime and justice work more generally.

All studies reported effect sizes that favor treatment conditions over control conditions, with only the Cincinnati, Los Angeles, and Newark studies not reporting statistically significant effect sizes. The Lowell (1.186) and Indianapolis (1.039) studies reported the largest statistically significant effect sizes while the Chicago study (.181) reported the smallest statistically significant effect size. The forest plots in Figures 2 and 3 present the meta-analyses of the largest and smallest effect sizes for each study, respectively.[9] For the largest effect size meta-analysis, the overall standardize mean difference effect size was large (.806) and statistically significant at the $p < .05$ level. For the smallest effect size meta-analysis, the overall

standardize mean difference effect size was medium (.474) and statistically significant at the $p < .05$ level.

4.5   PROGRAM TYPE AND RESEARCH DESIGN AS EFFECT SIZE MODERATORS

Our narrative review documented that pulling levers focused deterrence strategies have been directed at reducing crime by street gangs and criminally-active groups, drug markets, and high-risk individuals in high-crime areas. These programs represent differing applications of pulling levers strategies to control distinct types of problems. Moderator variables help to explain and understand differences across studies in the outcomes observed. Program type could be an influential moderator of the observed effect sizes in our overall meta-analysis. Figure 4 presents a random effects model examining the three different program types identified in the narrative review. It is important to note that the Q-statistic associated with the between group variation was large and statistically significant (Q = 20.430, df = 2, $p < .05$), suggesting that program type is influential in determining effect sizes. The gang / group intervention programs were associated with the largest within-group effect size (.770, $p < .05$), followed by the drug market intervention (DMI) programs (.661, $p < .05$) and the high-risk individuals programs (.186, $p < .05$). When program type was included as a moderator, the meta-analysis estimated a more modest overall effect size (.306, $p < .05$).

Pulling levers focused deterrence strategies directed at high-risk individuals in high-crime areas generated a smaller within-group effect size when compared to the DMI and gang / group intervention strategies. These smaller effect sizes may, in part, stem from methodological decisions to analyze outcomes at the area level rather than the individual level. As described in our narrative review, the Chicago PSN quasi-experimental evaluation conducted additional analyses of the effects of the intervention on individual recidivism patterns. As an exploratory exercise, we calculated an effect size for the Chicago (.434, $SE = .050$, $p < .05$). This medium and statistically significant effect size suggests a program impact similar to area-level impacts generated by the DMI and gang / group interventions.

Given the important distinction in methodological quality between the non-equivalent quasi-experiments and the quasi-experiments that used matching techniques to identify comparison groups, we also examined research design as a moderator variable. Figure 5 presents a random effects model examining the two different classes of quasi-experimental designs included in this review.[10] It is important to note that the Q-statistic associated with the between group variation was large and statistically significant (Q = 31.039, df = 1, $p < .05$), suggesting that research design is influential in determining effect sizes. Consistent with prior research suggesting that weaker designs are more likely to report stronger effects in

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[10] For the overall model, Q = 41.725, df = 9, $p < 0.0001$.

crime and justice studies (Weisburd et al., 2001; Welsh et al., 2011), the non-equivalent quasi-experimental designs were associated with a much larger within-group effect size (.766, $p <.05$) relative to the matched quasi-experimental designs (.196, $p <.05$). When research design type was included as a moderator, the meta-analysis estimated a more modest overall effect size (.312, $p<.05$).

Lipsey (2003) cautions that analyses that include single moderator variables can be misleading as these types of analyses can be confounded by the absence of other relevant variables. In this analysis, "research design type" is highly correlated with "program type." Five of the six gang / group intervention studies used non-equivalent quasi-experimental designs to analyze program effects. With the noteworthy exception of the Los Angeles evaluation, the five gang / group intervention programs attempted to influence ongoing feuds among gangs and groups in conflict networks that spanned the urban landscape. For instance, the authors of the Boston Ceasefire evaluation reported that, given the nature of their intervention and the dynamics of the problem, it was not possible to set aside within-city control gangs or comparison areas (Braga et al., 2001).

4.6   PUBLICATION BIAS

Publication bias presents a strong challenge to any review of evaluation studies (Rothstein, 2008). Campbell reviews, such as ours, take a number of steps to reduce publication bias, as represented by the fact that four of the 10 eligible studies in our review came from unpublished sources (one government report and three unpublished reports). Wilson (2009) has argued moreover that there is often little difference in methodological quality between published and unpublished studies suggesting the importance of searching the "grey literature." As a first step in investigating potential publication bias, we compared mean effect sizes for unpublished vs. published studies. Using random effects models, the mean effect size for published studies is 0.713 ($p < .05$) and for unpublished studies, the average effect is 0.384 ($p < .05$). While the published studies report larger effect sizes, the similarity in the direction and statistical significance in the mean effect sizes between the published and unpublished literature suggests that publication bias may not have major impact on the outcomes of this review.

We then used the trim-and-fill procedure (Duval and Tweedie 2000) to estimate the effect of potential data censoring, such as publication bias, on the outcome of the meta-analyses. 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 re-computes the mean effect size. 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 the left of the mean. The trim-and-fill procedure determined that three 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 0.604 (95% *CI* = 0.349, 0.859) to 0.437 (95% *CI* = 0.200, 0.637). Indeed, the 95 percent confidence intervals overlap, suggesting that the mean effect sizes may actually be the same.

5   Conclusion

The available scientific evidence on the crime reduction value of focused deterrence strategies has been previously characterized as "promising" but "descriptive rather than evaluative" (Skogan and Frydl, 2004: 241) and as "limited" but "still evolving" (Wellford et al., 2005: 10) by the U.S. National Research Council's Committee to Review Research on Police Policy and Practices and Committee to Improve Research Information and Data on Firearms, respectively. Our systematic review identified ten evaluations of focused deterrence strategies; nine of these evaluations were completed after the National Research Council reports were published. A better-developed base of scientific evidence now exists to assess whether crime prevention impacts are associated with this approach.

The basic findings of our review are very positive. Nine out of ten eligible studies reported strong and statistically significant crime reductions associated with the approach. Nonetheless, we are concerned with the lack of rigorous randomized experimental evaluations of this promising approach. While the biases in quasi-experimental research are not clear (e.g. Campbell and Boruch, 1975; Wilkinson and Task Force on Statistical Inference, 1999), recent reviews in crime and justice suggest that weaker research designs often lead to more positive outcomes (e.g. see Weisburd, Lum, and Petrosino, 2001; Welsh et al., 2011). This does not mean that non-experimental studies cannot be of high quality, but only that there is evidence that non-experimental designs in crime and justice are likely to overstate outcomes as contrasted with randomized experiments. In his review of situational crime prevention evaluations, Guerette (2009) finds that the conclusions of randomized evaluations were generally consistent with the majority conclusion of the non-randomized evaluations. While our narrative review is consistent with Guerette's (2009) conclusion, our calculated effect sizes reveal that less rigorous focused deterrence evaluation designs were associated with stronger reported effects. As such, we think that caution should be used in drawing conclusions regarding population effect sizes for the pulling levers intervention.

At the same time, the effects observed in the studies reviewed were often very large, and such effect sizes are evidenced as well in those studies using strong comparison groups (e.g. Papachristos, Meares, and Fagan, 2007). Our review provides strong empirical evidence for the crime prevention effectiveness of focused deterrence strategies. Even if we assume that the effects observed contain some degree of upward bias, it appears that the overall impact of such programs is noteworthy.

These findings are certainly encouraging, and point to the promises of this approach.

We certainly believe that the positive outcomes of the present studies indicate that additional experimental evaluations, however difficult and costly, are warranted. The potential barriers are real, especially in regards to identifying valid treatment and comparison areas. But existing evidence is strong enough to warrant a large investment in multi-site experiments (Weisburd and Taxman, 2000). Such experiments could solve the problem of small numbers of places in single jurisdictions, and would also allow for examination of variation in effectiveness across contexts.

Despite our concerns over the lack of randomized experiments, we believe that the findings of eligible focused deterrence evaluations fit well within existing research suggesting that deterrence-based strategies, if applied correctly, can reduce crime (Apel and Nagin, 2011). The focused deterrence approach seems to have the desirable characteristic of altering offenders' perceptions of sanction risk. Our findings are also supported by the growing body of scientific evidence that suggests police departments, and their partners, can be effective in controlling specific crime problems when they engage a variety of partners, and tailor an array of tactics to address underlying criminogenic conditions and dynamics (Braga, 2008a; Weisburd and Eck, 2004). Indeed, our study suggests that Durlauf and Nagin (2011) are correct in their conclusion that imprisonment and crime can both be reduced through the noteworthy marginal deterrent effects generated by allocating police officers, and their criminal justice partners, in ways that heighten the perceived risk of apprehension.

While the results of this review are very supportive of deterrence principles, we believe that other complementary crime control mechanisms are at work in the focused deterrence strategies described here that need to be highlighted and better understood (see Weisburd, 2011). In Durlauf and Nagin's (2011) article, the focus is on the possibilities for increasing perceived risk and deterrence by increasing police presence. Although this conclusion is warranted by the data and represents an important component of the causal mechanisms that have increased the effectiveness of focused deterrence strategies, we believe it misses an important part of the story. In the focused deterrence approach, the emphasis is not only on increasing the risk of offending, it is also on decreasing opportunity structures for violence, deflecting offenders away from crime, increasing the collective efficacy of communities, and increasing the legitimacy of police actions. Indeed, we suspect that the large effects we observe come precisely from the multi-faceted ways in which this program influences criminals.

A number of scholars have focused on the mechanism of "discouragement" when discussing crime prevention benefits of interventions (see, e.g. Clarke and Weisburd, 1994). Discouragement emphasizes reducing the opportunities for crime and

increasing alternative opportunity structures for offenders.   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; Skubak Tillyer and Kennedy, 2008). For instance, the Cincinnati Initiative to Reduce Violence used civil forfeiture techniques to close down a highly problematic bar that generated recurring serious violence (Engel, Corsaro, and Skubak Tillyer, 2010). Extending guardianship, assisting natural surveillance, strengthening formal surveillance, reducing the anonymity of offenders, and utilizing 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 (see, e.g. Welsh and Farrington, 2009). The focused deterrence approach also seeks to redirect offenders away from violent crime through the provision of social services and opportunities.   In all the gang / group interventions reviewed here, gang members were offered job training, employment, substance abuse treatment, housing assistance, and a variety of other services and opportunities.

Aspects of "broken windows" theory may also be relevant for our understanding of how and why focused deterrence programs reduce crime (Wilson and Kelling, 1982). Broken windows theory argues that intensive efforts by police to reduce social and physical disorder can reverse the breakdown of community social controls that accompanies untended and unrestrained violations of social order. Thus, crime is reduced in part because of efforts by the police and in part because of increased vigilance by community members.   Kleiman and Smith (1990: 88) describe the potential benefits of an intensive police effort to reduce drug crime and disorder by noting "a dramatic police effort may call forth increased neighborhood efforts at self-protection against drug dealing activity; given police resources such self-defense may be essential to long-run control of drug dealing."

Sampson, Raudenbush, and Earls (1997) emphasize the capacity of a community to realize common values and regulate behavior within it through cohesive relationships and mutual trust among residents. They argue 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 by "high capacities for collective action for the public good" (St. Jean, 2007: 3). Focused deterrence enhances collective efficacy in communities by emphasizing the importance of engaging and enlisting community members in the strategies developed. The High Point DMI strategy, for example, drew upon collective efficacy principles by engaging family, friends, and other "influential" community members in addressing the criminal behaviors of local drug dealers (Kennedy, 2009).

Finally, 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 (Skogan and Frydl, 2004; Tyler, 1990, 2004). Legitimacy is the public belief that there is a responsibility and obligation to voluntarily accept and defer to the decisions made

by authorities (Tyler, 1990, 2004). Recent studies suggest that when procedural justice approaches are used by the police citizens will not only evaluate the legitimacy of the police more highly, they will also be more likely to obey the law in the future (see, e.g. Paternoster et al., 1997). Advocates of focused deterrence strategies argue that targeted offenders should be treated with respect and dignity (Kennedy, 2008, 2009), reflecting procedural justice principles. The Chicago PSN strategy, for instance, sought to increase the likelihood that the offenders would "buy in" and voluntarily comply with the pro-social, anti-violence norms being advocated by interacting with offenders in ways that enhance procedural justice in their communication sessions (Papachristos, Meares, and Fagan, 2007).

In closing, we think it is important to recognize that focused deterrence strategies are a very recent addition to the existing scholarly literature on crime control and prevention strategies. While the evaluation evidence needs to be strengthened and the theoretical underpinnings of the approach needs further refinement, we believe 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.

6   Plans for Updating the Review

The authors expect to update the review every five years.

7   Acknowledgements

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9 Tables

9.1 CHARACTERISTICS OF ELIGLBLE PULLING LEVERS FOCUSED DETERRENCE EVALUATIONS

N=10

City population	N	Percent
Small (< 200,000 residents)	2	20.0
Medium (200,000 – 500,000 residents)	3	30.0
Large (> 500,000 residents)	5	50.0

Study type	N	Percent
Quasi-experiment with matched comparison group	2	20.0
Quasi-experiment with non-equivalent comparison group	7	70.0
Quasi-experiment with both types of comparison groups	1	10.0

Intervention type	N	Percent
Gang / group violence	6	60.0
Individual crime	2	20.0
Drug market	2	20.0

Publication type	N	Percent
Peer-reviewed journal	6	60.0
Published report	1	10.0
Unpublished report	3	30.0

Completion year	N	Percent
2001 – 2002	1	10.0
2003 – 2004	1	10.0
2005 – 2006	1	10.0
2007 – 2008	3	30.0
2009 – 2010	4	40.0

9.2 ELIGIBLE PULLING LEVERS FOCUSED DETERRENCE EVALUATIONS

Study	Treatment	Units of Analysis	Research Design
Operation Ceasefire Boston, Massachusetts Braga, Kennedy, Waring, and Piehl (2001)	Pulling levers 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	Non-equivalent 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
Indianapolis Violence Reduction Partnership Indianapolis, Indiana McGarrell, Chermak, Wilson, and Corsaro (2006)	Pulling levers 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	Non-equivalent 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
Operation Peacekeeper Stockton, California Braga (2008)	Pulling levers strategy focused on reducing serious violence by street gangs 65 month post-intervention period (September 1997 – December 2002)	Citywide intervention Outcome measure was the monthly count of citywide gun homicides	Non-equivalent 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

Study	Treatment	Units of Analysis	Research Design
	No threats to integrity of treatment noted during program implementation		models controlling for trends and seasonal variations used to estimate impact of intervention on time series
Project Safe Neighborhoods Lowell, Massachusetts Braga, Pierce, McDevitt, Bond, and Cronin (2008)	Pulling levers 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 non-fatal gun assault incidents	Non-equivalent 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
Cincinnati Initiative to Reduce Violence Cincinnati, Ohio Engel, Corsaro, and Skubak Tillyer (2010)	Pulling levers 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 non-group member-involved homicides	Non-equivalent quasi-experiment comparing group-member-involved homicide trends relative to non-group-member-involved homicides Count-based regression models controlling for trends and seasonal variations used to estimate impact of intervention on time series

Study	Treatment	Units of Analysis	Research Design
Operation Ceasefire Newark, New Jersey Boyle, Lanterman, Pascarella, and Cheng (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
Operation Ceasefire Los Angeles, California Tita, Riley, Ridgeway, Grammich, Abrahamse, and Greenwood (2003)	Pulling levers 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 due to 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 non-equivalent 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

Study	Treatment	Units of Analysis	Research Design
Project Safe Neighborhoods Chicago, Illinois Papachristos, Meares, and Fagan (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 32 month post-intervention period (May 2002 – December 2004) No threats to integrity of treatment noted during program implementation	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
Drug Market Intervention Nashville, Tennessee Corsaro and McGarrell (2009)	Pulling levers 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	Non-equivalent 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

Study	Treatment	Units of Analysis	Research Design
Drug Market Intervention Rockford, Illinois Corsaro, Brunson, and McGarrell (2010)	Pulling levers 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 Delancey Heights neighborhood of Rockford Outcome measures were monthly count of violent crime incidents and non-violent crime incidents	Non-equivalent 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

9.3 RESULTS OF ELIGIBLE PULLING LEVERS FOCUSED DETERRENCE EVALUATIONS

Study	Crime Outcomes	Displacement / Diffusion
Operation Ceasefire Boston, Massachusetts Braga, Kennedy, Waring, and Piehl (2001)	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	Not measured
Indianapolis Violence Reduction Partnership Indianapolis, Indiana McGarrell, Chermak, Wilson, and Corsaro (2006)	Statistically significant 34% reduction in total homicide	Not measured
Operation Peacekeeper Stockton, California Braga (2008)	Statistically significant 42% reduction in gun homicide	Not measured
Project Safe Neighborhoods Lowell, Massachusetts Braga, Pierce, McDevitt, Bond, and Cronin (2008)	Statistically significant 44% reduction in gun assault incidents	Not measured

Study	Crime Outcomes	Displacement / Diffusion
Cincinnati Initiative to Reduce Violence Cincinnati, Ohio Engel, Skubak Tillyer, Dunham, Hall, Ozer, Henson, and Godsey (2009)	Statistically significant 35% reduction in group member-involved homicides	Not measured
Operation Ceasefire Newark, New Jersey Boyle, Lanterman, Pascarella, and Cheng (2010)	No statistically significant reduction in gunshot wound victims in target zone	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 The results of the analysis were inconclusive
Operation Ceasefire Los Angeles, California Tita, Riley, Ridgeway, Grammich, Abrahamse, and Greenwood (2003)	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.	Examined immediate spatial displacement and diffusion effects in 11 Census block groups surrounding targeted Census block groups and gang crime committed by non-targeted gangs that were "socially tied" to targeted gangs 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

Study	Crime Outcomes	Displacement / Diffusion
Project Safe Neighborhoods Chicago, Illinois	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	Not measured
Papachristos, Meares, and Fagan (2007)	No statistically significant reduction in gang homicides in targeted police districts	
Drug Market Intervention Nashville, Tennessee	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	Examined immediate spatial displacement and diffusion effects in areas contiguous to the targeted neighborhood
Corsaro and McGarrell (2010)	No significant decreases reported in violent crime incidents and total calls for service	Analyses suggested significant diffusion of crime control benefits into contiguous areas
Drug Market Intervention Rockford, Illinois Corsaro, Brunson, and McGarrell (2010)	Statistically significant 22% reduction in non-violent offenses No significant decreases reported in violent offenses	Not measured

10 Figures

10.1 MEAN EFFECT SIZES FOR AREA OUTCOMES

Study name	Outcome	Statistics for each study			Std diff in means and 95% CI
		Std diff in means	Standard error	p-Value	
Lowell, MA	Gun assaults	1.186	0.207	0.000	
Indianapolis, IN	Total homicides	1.039	0.283	0.000	
Nashville, TN	Combined	0.838	0.320	0.009	
Stockton, CA	Gun homicides	0.763	0.157	0.000	
Boston, MA	Combined	0.645	0.241	0.008	
Los Angeles, CA	Combined	0.565	0.351	0.108	
Rockford, IL	Combined	0.521	0.285	0.067	
Cincinnati, OH	GMI homicides	0.352	0.224	0.115	
Newark, NJ	Gun shot wounds	0.225	0.160	0.159	
Chicago, IL	Combined	0.181	0.061	0.003	
Random Effect		0.604	0.130	0.000	

-2.00   -1.00   0.00   1.00   2.00

Favors Control   Favors Treatment

Meta Analysis

10.2 LARGEST EFFECT SIZES FOR AREA OUTCOMES

Study name	Outcome	Std diff in means	Standard error	p-Value
Nashville, TN	Narcotics offenses	1.512	0.341	0.000
Los Angeles, CA	Violent crimes	1.255	0.399	0.002
Lowell, MA	Gun assaults	1.186	0.207	0.000
Boston, MA	Youth homicides	1.161	0.251	0.000
Indianapolis, IN	Total homicides	1.039	0.283	0.000
Rockford, IL	Non-violent offenses	0.882	0.291	0.002
Stockton, CA	Gun homicides	0.763	0.157	0.000
Cincinnati, OH	GMI homicides	0.352	0.224	0.115
Chicago, IL	Gun homicides	0.256	0.061	0.000
Newark, NJ	Gun shot wounds	0.225	0.160	0.159
Random Effect		0.806	0.153	0.000

Std diff in means and 95% CI

-2.00   -1.00   0.00   1.00   2.00

Favors Control   Favors Treatment

Meta Analysis

10.3 SMALLEST EFFECT SIZES FOR AREA OUTCOMES

Study name	Outcome	Std diff in means	Standard error	p-Value
Lowell, MA	Gun assaults	1.186	0.207	0.000
Indianapolis, IN	Total homicides	1.039	0.283	0.000
Stockton, CA	Gun homicides	0.763	0.157	0.000
Nashville, TN	Violent incidents	0.476	0.310	0.126
Boston, MA	Shots fired calls	0.365	0.237	0.124
Cincinnati, OH	GMI homicides	0.352	0.224	0.115
Newark, NJ	Gun shot wounds	0.225	0.160	0.159
Los Angeles, CA	Gang crimes	0.165	0.292	0.572
Rockford, IL	Violent offenses	0.160	0.278	0.566
Chicago, IL	Gang homicides	0.089	0.061	0.145
Random Effect		0.474	0.134	0.000

Std diff in means and 95% CI

-2.00   -1.00   0.00   1.00   2.00

Favors Control   Favors Treatment

Meta Analysis

10.4   PROGRAM TYPE AS MODERATOR FOR AREA OUTCOMES

Group by Program Type	Study name	Outcome	Std diff in means	Standard error	p-Value
DMI	Nashville, TN	Combined	0.838	0.320	0.009
DMI	Rockford, IL	Combined	0.521	0.285	0.067
DMI			0.661	0.213	0.002
Gang/Group	Lowell, MA	Gun assaults	1.186	0.207	0.000
Gang/Group	Indianapolis, IN	Total homicides	1.039	0.283	0.000
Gang/Group	Stockton, CA	Gun homicides	0.763	0.157	0.000
Gang/Group	Boston, MA	Combined	0.645	0.241	0.008
Gang/Group	Los Angeles, CA	Combined	0.565	0.351	0.108
Gang/Group	Cincinnati, OH	GMI homicides	0.352	0.224	0.115
Gang/Group			0.770	0.127	0.000
Individual	Newark, NJ	Gun shot wounds	0.225	0.160	0.159
Individual	Chicago, IL	Combined	0.181	0.061	0.003
Individual			0.186	0.057	0.001
Overall			0.306	0.051	0.000

Meta Analysis, Random Effects Model

10.5   RESEARCH DESIGN TYPE AS MODERATOR FOR AREA OUTCOMES

Group by Design	Study name	Outcome	Std diff in means	Standard error	p-Value
Matched Quasi	Los Angeles, CA	Combined	0.565	0.351	0.108
Matched Quasi	Newark, NJ	Gun shot wounds	0.225	0.160	0.159
Matched Quasi	Chicago, IL	Combined	0.181	0.061	0.003
Matched Quasi			0.196	0.057	0.001
Non-Eq Quasi	Lowell, MA	Gun assaults	1.186	0.207	0.000
Non-Eq Quasi	Indianapolis, IN	Total homicides	1.039	0.283	0.000
Non-Eq Quasi	Nashville, TN	Combined	0.838	0.320	0.009
Non-Eq Quasi	Stockton, CA	Gun homicides	0.763	0.157	0.000
Non-Eq Quasi	Boston, MA	Combined	0.645	0.241	0.008
Non-Eq Quasi	Rockford, IL	Combined	0.521	0.285	0.067
Non-Eq Quasi	Cincinnati, OH	GMI homicides	0.352	0.224	0.115
Non-Eq Quasi			0.766	0.112	0.000
Overall			0.312	0.050	0.000

Meta Analysis, Random Effects Model

10.6 FUNNEL PLOT FOR ELIGIBLE STUDIES WITH IMPUTED STUDIES FROM TRIM-AND-FILL ANALYSIS

Note: Empty circles are the original studies. Filled-in circles indicate imputed studies from the trim-and-fill analysis.
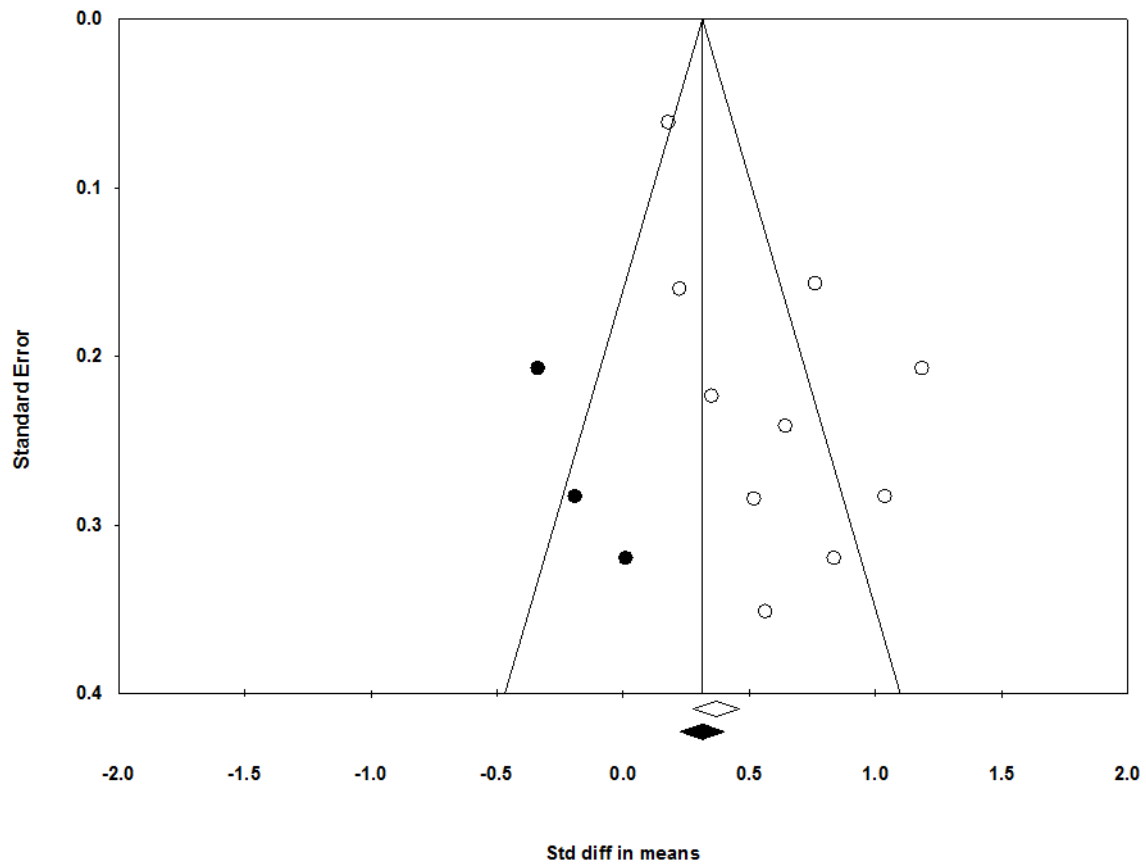
11 Appendix A: List of 90 Experts Contacted During Search Process

1. Allan Abrahamse, RAND Corporation
2. David Bayley, University at Albany, SUNY
3. Lawrence Bobo, Harvard University
4. Brenda Bond, Suffolk University
5. Robert Boruch, University of Pennsylvania
6. Douglas Boyle, University of Medicine and Dentistry of New Jersey
7. Alfred Blumstein, Carnegie Mellon University
8. Rod Brunson, Rutgers University
9. Chia-Cherng Cheng, University of Medicine and Dentistry of New Jersey
10. Steven Chermak, Michigan State University
11. Ronald V. Clarke, Rutgers University
12. Philip J. Cook, Duke University
13. Nicholas Corsaro, Southern Illinois University
14. Linda Cottler, Washington University of St. Louis
15. Shea Cronin, Boston University
16. Robert Crutchfield, University of Washington
17. Ruth Davis, The Pymatuning Group
18. Scott Decker, Arizona State University
19. Jessica Dunham, University of Cincinnati
20. John E. Eck, University of Cincinnati
21. Robin Engel, University of Cincinnati
22. Jeffrey Fagan, Columbia University
23. Graham Farrell, Loughborough University
24. Herman Goldstein, University of Wisconsin
25. Timothy Godsey, University of Cincinnati
26. Peter Grabosky, Australian National University
27. Clifford Grammich, RAND Corporation
28. Peter Greenwood, Greenwood and Associates
29. Davin Hall, University of Cincinnati
30. David Hemenway, Harvard University
31. William Henson, University of Cincinnati
32. Natalie K. Hipple, Michigan State University

33. Joel Horowitz, Northwestern University

34. Robert L. Johnson, University of Medicine and Dentistry of New Jersey

35. Shane Johnson, University College London

36. David M. Kennedy, John Jay College of Criminal Justice

37. Mark A.R. Kleiman, University of California, Los Angeles

38. David A. Klinger, University of Missouri, St. Louis

39. John Klofas, Rochester Institute of Technology

40. Johannes Knutsson, Norwegian Police University College

41. Jennifer Lanterman, University of Medicine and Dentistry of New Jersey

42. Janet Lauritsen, University of Missouri, St. Louis

43. Gloria Laycock, University College London

44. Steven Levitt, University of Chicago

45. Jens Ludwig, University of Chicago

46. Tracey Maclin, Boston University

47. Edward R. Maguire, American University

48. Stephen D. Mastrofski, George Mason University

49. Lorraine Mazerolle, University of Queensland

50. Jack McDevitt, Northeastern University

51. Edmund McGarrell, Michigan State University

52. Tracey Meares, Yale University

53. Terrie Moffitt, University of Wisconsin

54. Mark H. Moore, Harvard University

55. Susan Murphy, University of Michigan

56. Daniel Nagin, Carnegie Mellon University

57. Karen Norberg, Washington University of St. Louis

58. Murat Ozer, University of Cincinnati

59. Andrew Papachristos, University of Massachusetts, Amherst

60. Joseph Pascarella, Capella University

61. John V. Pepper, University of Virginia

62. Ruth Peterson, Ohio State University

63. Anne M. Piehl, Rutgers University

64. Glenn L. Pierce, Northeastern University

65. Alex Piquero, Florida State University

66. Peter Reuter, University of Maryland

67. Greg Ridgeway, RAND Corporation

68. K. Jack Riley, RAND Corporation

69. Dennis Rosenbaum, University of Illinois, Chicago

70. Richard Rosenfeld, University of Missouri, St. Louis

71. Elaine B. Sharp, University of Kansas

72. Lawrence Sherman, University of Cambridge

73. Wesley Skogan, Northwestern University

74. Nick Tilley, University College London

75. Marie S. Tillyer, University of Texas, San Antonio

76. George E. Tita, University of California, Irvine

77. Jeremy Travis, John Jay College of Criminal Justice
78. Tom Tyler, New York University
79. Stewart Wakeling, Public Health Institute
80. Joel Waldfogel, University of Minnesota
81. Samuel Walker, University of Nebraska, Omaha
82. Elin J. Waring, Lehman College, CUNY
83. Alexander Weiss, Northwestern University
84. Charles Wellford, University of Maryland
85. James Q. Wilson, Pepperdine University
86. Jeremy M. Wilson, Michigan State University
87. Christopher Winship, Harvard University
88. Garen Wintemute, University of California, Davis
89. Robert Worden, University at Albany, SUNY
90. Franklin Zimring, University of California, Berkeley

12 Appendix B: Coding Sheets

12.1 ELIGIBILITY CHECK SHEET

1. Document ID: __ __ __ __

2. Study Author Name(s) _____

3. Study Title:_____

4. Journal Name, Volume and Issue: _____

5. Document ID: __ __ __ __

6. Coder's Initials __ __ __

7. Date eligibility determined: _____

8. A study must meet the following criteria in order to be eligible. Answer each question with a "yes" or a "no."

    a. The study is an evaluation of a pulling levers focused deterrence intervention. _____

    b. The study includes a comparison group (or a pre-intervention comparison period in the case of pre-post studies), which did not receive the treatment condition (problem-oriented policing). Studies may be experimental or quasi-experimental. _____

    c. The study reports on at least one crime outcome. _____

    d. The study is written in English. _____

If the study does not meet the criteria above, answer the following question:

    a. The study is a review article that is relevant to this project (e.g. may have references to other studies that are useful, may have pertinent background information) _____

9. Eligibility status:

        _____ Eligible

        _____ Not eligible

        _____ Relevant review

Notes:

_____

_____

12.2 CODING PROTOCOL

Reference Information

1. Document ID: ___ ___ ___ ___

2. Study author(s): _____

3. Study title: _____

4. Publication type: _____
    1. Book
    2. Book chapter
    3. Journal article (peer reviewed)
    4. Thesis or doctoral dissertation
    5. Government report (state/local)
    6. Government report (federal)
    7. Police department report
    8. Technical report
    9. Conference paper
    10. Other (specify))_____

5. Publication date (year): _____

6a. Journal Name: _____
6b. Journal Volume: _____
6c. Journal Issue: _____

7. Date range of research (when research was conducted):
Start: _____
Finish: _____

8. Source of funding for study: _____

9. Country of publication: _____

10. Date coded: _____

11. Coder's Initials: ___ ___ ___

Describing the Pulling Levers Focused Deterrence Intervention

12a. Did the study formally identify the treatment as a pulling levers policing
   intervention?
   1. Yes
   2. No

12b. If No, what did the study call the intervention?

   _____

13. What crime problem was targeted for the intervention? (Select all that apply)
   1. Total homicide
   2. Youth homicide
   3. Gun violence
   4. Drug-related violence
   5. Street-level drug markets
   6. Other (specify) _____

14. Who were the primary targets of the intervention? (Select all that apply)
   1. Street gangs
   2. Semi-organized / organized crime
   3. Informal criminally-active groups
   4. Drug-selling crews
   5. High-risk individuals
   6. Other group (specify) _____

15. If the intervention was primarily targeted at "high-risk individuals," please
describe the individuals: (Select all that apply)
   1. Probationers
   2. Parolees
   3. Convicted felons
   4. Gang members
   5. Street-level drug dealers
   6. Other (specify) _____

16. Specifically, what event(s) makes up the problem?

_____

_____

17. Was the intervention developed based on an analysis of the targeted problem?
1. No, the intervention was implemented without any analysis of the targeted problem.
2. Yes, the intervention was implemented after a cursory / limited analysis of the targeted problem.
3. Yes, the intervention was implemented after a thorough analysis of the targeted problem.
4. Other (specify) _____

18. At what unit of analysis was the treatment delivered/intervention directed at? (Select all that apply)
1. Specific individuals
2. Groups of individuals
3. Micro places (crime hot spots, specific housing project, etc.)
4. Small police-defined units (such as beats)
5. Larger police-defined units (such as districts or sectors)
6. Neighborhood or community level
7. City or town level
8. State level
9. Other (specify) _____

19. What agency was primarily responsible for the implementation of the intervention? (Select the lead agency only)
1. Local police
2. State police
3. Federal law enforcement agencies (e.g. ATF, DEA, FBI)
4. Local / County / State prosecutor
5. Federal prosecutor
6. Probation
7. Parole
8. Correctional agency
9. Local / County / State governmental agency (e.g. Mayor's Criminal Justice Office)
10. Social service provider
11. Community-based agency
12. Other (specify) _____

20. What groups were involved in the implementation of the intervention? (Select all that apply)

1. Local police
2. State police
3. Federal law enforcement agencies (e.g. ATF, DEA, FBI)
4. Local / County / State prosecutor
5. Federal prosecutor
6. Probation
7. Parole
8. Correctional agency
9. Local / County / State governmental agency (e.g. Mayor's Criminal Justice Office)
10. Social service provider
11. Community-based agency
12. Other (specify) _____

21. What key elements of the focused deterrence strategy were identified in the program evaluation? (Select all that apply)

1. Clear "triggering" event that provoke the pulling levers response
2. Enforcement levers that could be customized to targeted groups / individuals
3. Social services / opportunities for targeted groups / individuals
4. Communications strategy
5. Other (specify) _____

22. If a communications strategy was present, please identify the key elements of the message(s) (Select all that apply)

1. Deterrence message
2. Social service / opportunity-based message
3. Changing norms / decision making message
4. Reintegration of offender(s) back into community message
5. Other (specify) _____
6. N/A

23. If a communications strategy was present, how were the message(s) delivered / marketed to the targeted audience? (Select all that apply)

1. Formal meetings (e.g. forums or "call-ins")
2. Home visits
3. On the street (i.e. "retail" delivery on corners, in parks, etc.)
4. Advertising (e.g. billboards, TV / radio spots, handouts, etc.)
5. School assemblies
6. Correctional setting (e.g. in-prison meeting, in-juvenile detention facility, etc.)
7. Other (specify) _____

8. N/A

24. What did the evaluation indicate about the implementation of the response? __
   1. The response was implemented as planned or nearly so
   2. The response was not implemented or implemented in a radically different way than originally planned
   3. Unclear/no process evaluation included

25. If the process evaluation indicated there were problems with implementation of the response, describe these problems:

_____

_____

_____

_____

26. If the process evaluation identified inadequate participation by involved agencies, indicate the agencies below that were responsible for weak participation (Select all that apply)
   1. Local police
   2. State police
   3. Federal law enforcement agencies (e.g. ATF, DEA, FBI)
   4. Local / County / State prosecutor
   5. Federal prosecutor
   6. Probation
   7. Parole
   8. Correctional agency
   9. Local / County / State governmental agency (e.g. Mayor's Criminal Justice Office)
   10. Social service provider
   11. Community-based agency
   12. Other (specify) _____

27. Country where study was conducted: _____

28. City (and state/province, if applicable) where study was conducted: _____

Methodology/Research design:
29. Type of study: _____
   1. Randomized experiment
   2. Nonequivalent control group (quasi-experimental)
   3. Multiple time series (quasi-experimental)
   4. Other (specify) _____

30. How were study units allocated to treatment or comparison conditions?
    1. Simple random allocation
    2. Random allocation in pairs, blocks, or some other sophisticated technique
    3. Simple descriptive matching
    4. Sophisticated statistical matching (e.g. propensity scores)
    5. Other (specify) _____

31. Explain how independent and extraneous variables were controlled so that it was possible to disentangle the impact of the intervention or how threats to internal validity were ruled out.

_____

_____

_____

The following questions refer to the units receiving treatment:

32. Units receiving treatment: _____
    1. Micro places (crime hot spots, specific housing project, etc.)
    2. Small police-defined units (such as beats)
    3. Larger police-defined units (such as districts or sectors)
    4. Neighborhood or community level
    5. City or town level
    6. State level
    7. Individuals
    8. Other (specify) _____

33. What is the exact unit receiving treatment?_____

The following question refers to the units not receiving treatment

34. Units NOT receiving treatment: _____
    1. Micro places (crime hot spots, specific housing project, etc.)
    2. Small police-defined units (such as beats)
    3. Larger police-defined units (such as districts or sectors)
    4. Neighborhood or community level
    5. City or town level
    6. State level
    7. Individual
    8. Other (specify) _____

35. What were the casual hypotheses tested in this study?_____

36. Please identify any theories from which the causal hypotheses were derived.

_____

_____

Outcomes reported (Note that for each outcome, a separate coding sheet is required)

37. How many crime / alternative outcomes are reported in the study? _____

38. What is the specific outcome recorded on this coding sheet?_____

39. Was it the primary outcome of the study? _____
    1. Yes
    2. No
    3. Can't tell/researcher did not prioritize outcomes

40. Was this initially intended as an outcome of the study? _____
    1. Yes
    2. No (explain)
    3. Can't tell

41. If no, explain why:

_____

_____

Unit of analysis

42. What was the unit of analysis for the research evaluation?
    1. Individuals
    2. Micro places (crime hot spots, specific housing project, etc.)
    3. Small police-defined units (such as beats)
    4. Larger police-defined units (such as districts or sectors)
    5. Neighborhood or community level
    6. City or town level
    7. State level
    8. Other (specify) _____

43. How many units of analysis are there for the intervention in the study? _____

44. Did the researchers collect nested data within the unit of analysis?
    1. Yes
    2. No

Dependent Variable

45. What type of data was used to measure the outcome covered on this coding sheet? _____
    1. Official data (from the police)
    2. Researcher observations
    3. Self-report surveys
    4. Other (specify) _____

46. If official data was used, what specific type(s) of data were used? (Select all that apply)
    1. Calls for service (911 calls)/crime reports
    2. Arrests
    3. Incident reports
    4. Level of citizen complaints
    5. Other (specify)
    6. N/A (official data not used)
    7. Other (specify) _____

47. If researcher observations were used, what types of observations were taken? (Select all that apply)
    1. Physical observations (e.g. observed urban blight, such as trash, graffiti)
    2. Social observations (e.g. observed disorder, such as loitering, public drinking)
    3. Other observations (specify)
    4. N/A (researcher observations not used)
    5. Other (specify) _____

48. If self-report surveys were used, who was surveyed? (Select all that apply)
    1. Residents/community members
    2. Business owners
    3. Elected officials
    4. Government/social service agencies
    5. Other (specify) _____
    6. N/A (self-report surveys not used)

49. Specifically identify the outcome covered on this coding sheet _____

50. For the units of analysis in this study, what time periods were examined for the outcome covered on this coding sheet?
    1. Yearly
    2. Monthly
    3. Weekly
    4. Other researcher defined time periods (specify) _____

51. What was the length in time of the follow-up period after the intervention?

_____

52. Did the researcher assess the quality of the data collected?
      1. Yes
      2. No

52a. Did the researcher(s) express any concerns over the quality of the data?
      1. Yes
      2. No
52b. If yes, explain_____

53a. Does the evaluation data correspond to the initially stated problem? (i.e. if the problem is gang violence, does the evaluation data specifically look at whether gang violence changed?)
      1. Yes
      2. No

53b. If no, explain the discrepancy:

_____

_____

Effect size/Reports of statistical significance

Dependent Measure Descriptors
54. Statistical analysis design: _____
      1. Pretest comparison
      2. Post-test comparison
      3. Follow-up comparison
      4. N/A

Sample Size

55. Based on the unit of analysis for this outcome, what is the total sample size in the analysis?_____

56. What is the total sample size of the treatment group (group that receives the response)?_____

57. What is the total sample size of the control group (if applicable)? _____

58a. Was attrition a problem in the analysis for this outcome?
      1. Yes

2. No

58b. If attrition was a problem, provide details (e. g. how many cases were lost and why were they lost).

_____

_____

_____

59. What do the sample sizes above refer to?
      1. Crimes
      2. People
      3. Geographic areas
      4. Places
      5. Other (specify) _____

Effect Size Data

60. Raw difference favors (i.e. shows more success for):
      1. Treatment group
      2. Control group
      3. Neither (exactly equal)
      9. Cannot tell (or statistically insignificant report only)

61. Did a test of statistical significance indicate statistically significant differences between either the control and treatment groups or the pre and post tested treatment group? _____
      1. Yes
      2. No
      3. Can't tell
      4. N/A (no testing completed)

62. Was a standardized effect size reported?
      1. Yes
      2. No

63. If yes, what was the effect size? _____

64. If yes, page number where effect size data is found _____

65. If no, is there data available to calculate an effect size?
      1. Yes
      2. No

66. Type of data effect size can be calculated from:

1. Means and standard deviations
2. *t*-value or *F*-value
3. Chi-square (df=1)
4. Frequencies or proportions (dichotomous)
5. Frequencies or proportions (polychotomous)
6. Other (specify) _____

Means and Standard Deviations

67a. Treatment group mean. _____
67b. Control group mean. _____

68a. Treatment group standard deviation. _____
68b. Control group standard deviation. _____

Proportions or frequencies

69a. *n* of treatment group with a successful outcome. _____
69b. *n* of control group with a successful outcome. _____

70a. Proportion of treatment group with a successful outcome. _____
70b. Proportion of treatment group with a successful outcome. _____

Significance Tests

71a. *t*-value _____
71b. *F*-value _____
71c. Chi-square value (*df*=1) _____

Calculated Effect Size

72a. Effect size _____
72b. Standard error of effect size _____

Conclusions made by the author(s)
Note that the following questions refer to conclusions about the effectiveness of the intervention
in regards to the current outcome being addressed on this coding sheet.

73. Conclusion about the impact of the intervention? _____
    1. The authors conclude problem declined
    2. The authors conclude the problem did not decline
    3. Unclear/no conclusion stated by authors

74. Did the assessment find evidence of a geographic displacement of crime? _____
    1. Yes
    2. No
    3. Not tested

75. Did the assessment find evidence of a temporal displacement of crime? _____
    1. Yes
    2. No
    3. Not tested

76. Did the author(s) conclude that the pulling levers intervention was beneficial? _
    1. Yes
    2. No
    3. Can't tell

77. Did the author(s) conclude there a relationship between the pulling levers intervention and a reduction in crime? _____
    1. Yes
    2. No
    3. Can't tell

78. Who funded the intervention?
_____
_____

79. Who funded the evaluation research?
_____
_____

80. Were the researchers independent evaluators?
    1. Yes
    2. No

80b. If no, explain the nature of the relationship:
_____
_____

81. Additional notes about conclusions:
_____

82. Additional notes about study:
_____

13 Appendix C: Detailed Narrative Review of the Effects of Eligible Focused Deterrence Evaluations on Crime

13.1 OPERATION CEASEFIRE IN BOSTON, MASSACHUSETTS

The Boston Gun Project was a problem-oriented policing enterprise expressly aimed at taking on a serious, large-scale crime problem — homicide victimization among young people in Boston in the 1990s. The trajectory of the Boston Gun Project, and the resulting Operation Ceasefire intervention, is by now well known and extensively documented (Braga et al., 2001; Kennedy, 1997, 2006; Kennedy et al., 1996). Briefly, a working group of law enforcement personnel, youth workers, and Harvard researchers diagnosed the youth violence problem in Boston as one of patterned, largely vendetta-like hostility amongst a small population of chronic offenders, and particularly among those involved in 61 loose, informal, mostly neighborhood-based "gangs." These 61 gangs consisted of some 1,300 members, representing less than 1 percent of the city's youth between the ages of 14 and 24. Although small in number, these gangs were responsible for more than 60 percent of youth homicide in Boston.

The Operation Ceasefire focused deterrence strategy was designed to prevent violence by reaching out directly to gangs, saying explicitly that violence would no longer be tolerated, and backing up that message by "pulling every lever" legally available when violence occurred (Kennedy, 1997). The chronic involvement of gang members in a wide variety of offenses made them, and the gangs they formed, vulnerable to a coordinated criminal justice response. The authorities could disrupt street drug activity, focus police attention on low-level street crimes such as trespassing and public drinking, serve outstanding warrants, cultivate confidential informants for medium- and long-term investigations of gang activities, deliver strict probation and parole enforcement, seize drug proceeds and other assets, ensure stiffer plea bargains and sterner prosecutorial attention, request stronger bail terms (and enforce them), and bring potentially severe federal investigative and prosecutorial attention to gang-related drug and gun activity. Simultaneously, youth workers, probation and parole officers, and later churches and other community groups offered gang members services and other kinds of help.

These partners also delivered an explicit message that violence was unacceptable to the community and that "street" justifications for violence were mistaken. The Ceasefire Working Group delivered this message in formal meetings with gang members (known as "forums" or "call-ins"), through individual police and probation contacts with gang members, through meetings with inmates at secure juvenile facilities in the city, and through gang outreach workers. The deterrence message was not a deal with gang members to stop violence. Rather, it was a promise to gang members that violent behavior would evoke an immediate and intense response. If gangs committed other crimes but refrained from violence, the normal workings of police, prosecutors, and the rest of the criminal justice system dealt with these matters. But if gang members hurt people, the Working Group concentrated its enforcement actions on their gangs.

The Ceasefire "crackdowns" were not designed to eliminate gangs or stop every aspect of gang activity, but to control and deter serious violence. To do this, the Working Group explained its actions against targeted gangs to other gangs, as in "this gang did violence, we responded with the following actions, and here is how to prevent anything similar from happening to you." The ongoing Working Group process regularly watched the city for outbreaks of gang violence and framed any necessary responses in accord with the Ceasefire strategy. As the strategy unfolded, the Working Group continued communication with gangs and gang members to convey its determination to stop violence, to explain its actions to the target population, and to maximize both voluntary compliance and the strategy's deterrent power.

The DOJ-sponsored evaluation of the impact of Operation Ceasefire used a nonrandomized quasi-experimental design to compare youth homicide trends in Boston to youth homicide trends in other major cities in the United States and large New England cities (Braga et al., 2001).[11] The key outcome variable was the monthly number of homicide victims ages 24 and under between January 1, 1991 and May 31, 1998. 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. Poisson and negative binomial 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 with June 1996 selected as the commencement of the post-implementation period.

The Ceasefire evaluation concluded that the program was associated with statistically significant reductions in youth homicide and the other indicators of

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[11] The Braga and Pierce (2005) evaluation of the effects of Operation Ceasefire on illegal gun market dynamics in Boston was not included in this review as it did not examine the effects of a pulling levers strategy on crime outcomes.

serious gun violence in Boston. Controlling for the other covariates, the evaluation reported that Ceasefire was associated with a 63 percent reduction in the monthly count of youth homicides, a 25 percent reduction in the monthly count of citywide gun assault incidents, a 32 percent reduction in the monthly count of citywide shots-fired calls for service, and a 44 percent reduction in the monthly count of youth gun assaults in selected high-risk district (Braga et al., 2001; see also Piehl et al., 2000). In a companion paper, Piehl et al. (2003) closely analyzed the monthly counts in the youth homicide time series to determine whether the timing of the implementation of Ceasefire coincided with the start of the significant decrease in Boston youth homicides. The authors developed an econometric model that evaluated all possible monthly break points in the time series to identify the maximal monthly break point associated with a significant structural change in the trajectory of the time series. Controlling for trends and seasonal variations, the timing of the "optimal break" in the time series was in the summer months after Ceasefire was implemented.

The DOJ-sponsored evaluation then conducted a comparative analysis of youth homicide trends in 39 of the most populous cities in the United States and 29 New England cities with populations of more than 60,000 residents (Braga et al., 2001). Using count regression models that controlled for trends, seasonal variations, and serial autocorrelation, the research found that only three cities (Dallas, TX; Jacksonville, FL; and Virginia Beach, VA) had significant reductions in the monthly count of youth homicides that coincided with the implementation of Ceasefire in Boston and an additional four cities (Los Angeles, CA; New York City, NY; Philadelphia, PA; and Tucson, AZ) had significant reductions in the monthly count of youth homicides at some point within the time series. Further examination of the youth homicide trends in these cities, however, revealed trajectories that looked distinct from the trajectory of Boston youth homicide over the same time period. As such, the researchers concluded that Boston's youth homicide reduction associated with Operation Ceasefire was distinct when compared to trends in most major U.S. cities.

The DOJ evaluation has been reviewed by a number of researchers and the relationship between the implementation of Ceasefire and the trajectory youth homicide in Boston during the 1990s has been closely scrutinized. Fagan (2002) suggested that some of the decrease in homicide may have occurred without the Ceasefire intervention in place as violence was decreasing in most major U.S. cities. In support of this perspective, Fagan's (2002) presented a simple time-series graph on youth gun homicide in Boston and in other Massachusetts cities that suggested a general downward trend in gun violence may have existed before Operation Ceasefire was implemented. Using growth-curve analysis to examine predicted homicide trend data for the 95 largest U.S. cities during the 1990s, Rosenfeld and his colleagues (2005) found some evidence of a sharper youth homicide drop in Boston than elsewhere but suggest that the small number of youth homicide incidents precludes strong conclusions about program effectiveness based on their statistical models. However, in his review of their analysis, Richard Berk (2005)

raised a number of statistical and methodological concerns with the analysis developed by Rosenfeld and his colleagues. Ludwig (2005) suggested that Ceasefire was associated with a large drop in youth homicide but, given the complexities of analyzing city-level homicide trend data, there remained some uncertainty about the extent of Ceasefire's effect on youth violence in Boston. Morgan and Winship's (2007) review of the DOJ evaluation concluded that the analysis was a "very high-quality example" of how to conduct an interrupted time series analysis of program impact and further noted "they offer four types of supplemental analysis... which can be used to strengthen the warrant for causal assertion" (252).

The National Academies' Panel on Improving Information and Data on Firearms (Wellford et al., 2005) concluded that the Ceasefire evaluation was compelling in associating the intervention with the subsequent decline in youth homicide. However, the Panel also suggested that many complex factors affect youth homicide trends and it was difficult to specify the exact relationship between the Ceasefire intervention and subsequent changes in youth offending behaviors. While the DOJ-sponsored evaluation controlled for existing violence trends and certain rival causal factors such as changes in the youth population, drug markets, and employment in Boston, 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. Therefore, the non-randomized control group research design cannot rule out these internal threats to the conclusion that Ceasefire was the key factor in the youth homicide decline.

13.2 INDIANAPOLIS VIOLENCE REDUCTION PARTNERSHIP IN INDIANAPOLIS, INDIANA

The Indianapolis Violence Reduction Partnership (IVRP) working group was comprised of Indiana University researchers and federal, state, and local law enforcement agencies (McGarrell and Chermak, 2003). During the problem analysis phase, the researchers examined 258 homicides from 1997 and the first 8 months of 1998 and found that a majority of homicide victims (63%) and offenders (75%) had criminal and/or juvenile records. Those with a prior record often had a substantial number of arrests. The working group members followed the structured qualitative data gathering exercises used in Boston to gain insight on the nature of homicide incidents. The qualitative exercise revealed that 59% of the incidents involved "groups of known chronic offenders" and 53% involved drug-related motives such as settling business and turf disputes (McGarrell and Chermak, 2003). It is worth noting that the terminology "groups of known chronic offenders" was initially used because, at that point in time, there was not a consensual definition of "gang" and the reality of much gang activity in Indianapolis was of a relatively loose structure (McGarrell and Chermak, 2003).

The working group developed two sets of overlapping strategies. First, the most violent chronic offenders in Indianapolis were identified and targeted for heightened arrest, prosecution, and incarceration (McGarrell and Chermak, 2003). Second, the working group engaged the pulling levers approach to reduce violent behavior by gangs and groups of known chronic offenders (McGarrell and Chermak, 2003). The IVRP strategy implemented by the Indianapolis working group closely resembled the Boston version of pulling levers. The communications strategy, however, differed in an important way. The deterrence and social services message was delivered in meetings with high-risk probationers and parolees organized by neighborhoods. Similarly, home visits by probation and parolees were generally organized by neighborhood. As the project progressed, when a homicide or series of homicides involved certain groups or gangs, the working group attempted to target meetings, enforcement activities, and home visits on the involved groups or gangs (McGarrell and Chermak, 2003).

The evaluation of the IVRP gang violence reduction strategy used a nonrandomized quasi-experimental design to compare homicide incident trends in Indianapolis to homicide incident trends in six Midwestern cities (McGarrell et al. 2006). The six comparison cities included Cincinnati (OH), Cleveland (OH), Columbus (OH), Kansas City (MO), Louisville (KY), and Pittsburgh (PA). For all seven cities, the key outcome variable was the monthly number of homicide incidents between January 1, 1997 and June 30, 2001. The evaluation used Auto Regressive Integrated Moving Average (ARIMA) models to analyze the city time series data.

The impact of the IVRP strategy was estimated using a dummy variable with April 1999 selected as the commencement of the post-intervention period (McGarrell et al., 2006). The ARIMA analyses of the Indianapolis homicide time series estimated that the IVRP intervention was associated with a statistically significant 34 percent reduction in monthly numbers of homicides. The ARIMA models analyzing the other cities' homicide time series did not report any statistically significant associations between the timing of IVRP and subsequent decreases in monthly homicide numbers. In a subsequent analysis of Indianapolis homicide time series data, Corsaro and McGarrell (2009) used ARIMA models to analyze the impact of IVRP on gang and non-gang homicides. The analyses found a statistically significant 38 percent reduction in gang homicides following the implementation of IVRP and did not find a statistically significant reduction in the non-gang homicides during the post-intervention time period. Since IVRP was explicitly designed to reduce gang violence, the authors concluded that these results support the position that the intervention was indeed having the desired effects on violent gang offending.

13.3 OPERATION PEACEKEEPER IN STOCKTON, CALIFORNIA

Beginning in mid-1997, criminal justice agencies in Stockton began experimenting with the pulling levers approach to address a sudden increase in youth homicide. The Stockton Police Department and other local, state, and federal law enforcement agencies believed that most of the youth violence problem was driven by gang conflicts and that the pulling levers approached used in Boston might be effective in reducing Stockton's gang violence problem. The strategy was implemented by the Stockton Police Department's Gang Street Enforcement Team and grew into what is now known as "Operation Peacekeeper" as more agencies joined the partnership (Wakeling, 2003).

The Peacekeeper intervention was managed by a working group of line-level criminal justice practitioners; social service providers also participated in the working group process as appropriate. When street gang violence erupted or when it came to the attention of a working group member that gang violence was imminent, the working group followed the Boston model by sending a direct message that gang violence would not be tolerated, pulling all available enforcement levers to prevent violence, continuing communications, and providing social services and opportunities to gang members who want them.

The Operation Peacekeeper evaluation used a nonrandomized quasi-experimental design to compare gun homicide trends in Stockton to gun homicide trends in eight other midsized California cities (Braga, 2008b). The eight comparison California cities included Anaheim, Bakersfield, Fresno, Long Beach, Oakland, Riverside, Sacramento, and Santa Ana. For each of the nine cities included in the evaluation, the key outcome variable was the monthly number of gun homicide victims between January 1, 1990 and December 31, 2005. The evaluation carefully analyzed the distributions of the dependent variables for each city's time series to determine the appropriate regression models for the impact assessment. Ordinary Least Squares (Santa Ana), maximum likelihood with an AR(1) autoregressive component (Long Beach, Oakland), negative binomial (Anaheim, Bakersfield, Fresno, Riverside, Stockton), and Poisson (Sacramento) regression models were used to analyze the city time series data.

Stockton's Operation Peacekeeper intervention was implemented in September 1997 and was operational until it was discontinued in December 2002 (Braga, 2008b). Multiple category dummy variables indicating the time periods when the Stockton Peacekeeper intervention was present or not were included in the regression models to estimate the trajectory of the monthly counts of gun homicide in each of the time series after Stockton implemented its gun violence reduction initiative. Controlling for existing linear and non-linear trends, seasonal variations, and violent crime trends, the negative binomial regression analyzing the Stockton gun homicide time series estimated that the intervention was associated with a statistically significant

42 percent reduction in the monthly count of gun homicides. None of the comparison cities experienced a statistically significant reduction in the monthly count of gun homicides that coincided with the implementation of the Peacekeeper intervention in Stockton.

13.4   PROJECT SAFE NEIGHBORHOODS IN LOWELL, MASSACHUSETTS

Supported by funds from the U.S. Department of Justice-sponsored Project Safe Neighborhoods initiative, an interagency task force implemented a pulling levers focused deterrence strategy to prevent gun violence among Hispanic and Asian gangs in Lowell, Massachusetts in 2002 (Braga, McDevitt, and Pierce, 2006). The Lowell authorities used a pulling levers focused deterrence strategy that replicated Boston's Operation Ceasefire to prevent violence among Hispanic gangs. However, from the outset, they felt much less confident about their ability to prevent Asian gang violence by applying the same set of criminal justice levers to Asian gang members. During the intervention time period, the Lowell Police Department (LPD) had little reliable intelligence about Asian gangs in the city (Braga et al., 2006). The LPD had attempted to develop informants in the past but most these efforts had been unsuccessful.

Through PSN, the LPD increased its efforts to develop intelligence about the structure of the city's Asian gangs and particularly the relationship between Asian gang violence and ongoing illegal gambling that was being run by local Asian businesses. In Lowell, Cambodian and Laotian gangs were comprised of youth whose street activities were influenced by "elders" of the gang (Braga et al., 2006). Elders were generally long-time gang members in their 30s and 40s that no longer engaged in illegal activities on the street or participated in street-level violence with rival youth. Rather, these older gang members were heavily involved in running illegal gambling dens and informal casinos that were operated out of cafes, video stores, and warehouses located in the poor Asian neighborhoods of Lowell. The elders used young street gang members to protect their business interests and to collect any unpaid gambling debts. Illegal gaming was a very lucrative business that was much more important to the elders than any ongoing beefs the youth in their gang had with other youth (Braga et al., 2006). In contrast to acquiring information on individuals responsible for gun crimes in Asian communities, it was much easier to detect the presence of gambling operations through surveillance or a simple visit to the suspected business establishment.

The importance of illegal gaming to influential members of Asian street gangs provided a potentially potent lever to law enforcement in preventing violence. The authorities in Lowell believed that they could systematically prevent street violence among gangs by targeting the gambling interests of older members. When a street gang was violent, the LPD targeted the gambling businesses run by the older members of the gang. The enforcement activities ranged from serving a search

warrant on the business that houses the illegal enterprise and making arrests to simply placing a patrol car in front of the suspected gambling location to deter gamblers from entering. The LPD coupled these tactics with the delivery of a clear message, "when the gang kids associated with you act violently, we will shut down your gambling business. When violence erupts, no one makes money" (Braga et al., 2006: 40). Between October 2002 and June 2003, the height of the focused attention on Asian gangs, the LPD conducted some 30 search warrants on illegal gambling dens that resulted in more than 100 gambling-related arrests (Braga et al., 2006).

The evaluation of the PSN gang violence reduction strategy used a nonrandomized quasi-experimental design to compare fatal and non-fatal gun assault incident trends in Lowell to fatal and non-fatal gun assault incident trends in seven other Massachusetts cities and the entire State of Massachusetts (Braga et al., 2008). The seven comparison Massachusetts cities included Boston, Brockton, Fall River, Lynn, New Bedford, Springfield, and Worcester. For the State of Massachusetts and the eight cities included in the evaluation, the key outcome variable was the monthly number of gun assault incidents between January 1, 1996 and December 31, 2005. The evaluation carefully analyzed the distributions of the dependent variables for each time series to determine the appropriate regression models for the impact assessment. Maximum likelihood with an AR(1) autoregressive component (Boston, Springfield, and State of Massachusetts), negative binomial (Brockton, Lynn, New Bedford, Worcester), and Poisson (Lowell, Fall River) regression models were used to analyze the city time series data.

The impact of Lowell's PSN strategy was estimated using a dummy variable with October 2002 selected as the commencement of the post-intervention period (Braga et al., 2008). Controlling for existing linear and non-linear trends, seasonal variations, population changes, and violent crime trends, the Poisson regression model reported that the Lowell PSN intervention was associated with a statistically significant 44 percent reduction in the monthly count of gun assault incidents. Neither the comparison cities nor the State of Massachusetts experienced a statistically significant reduction in the monthly count of gun homicides that coincided with the implementation of the PSN intervention in Lowell.

13.5   CINCINNATI INITIATIVE TO REDUCE VIOLENCE IN CINCINNATI, OHIO

In response to a disturbing increase in homicides between 2001 and 2006, Cincinnati's political leadership partnered with law enforcement officials, academics, medical professionals, street advocates, and community and business leaders, to form the Cincinnati Initiative to Reduce Violence (CIRV; Engel et al., 2010). Problem analyses suggested that violent street groups of active criminal offenders generated the bulk of homicides and shootings in Cincinnati. As described by Engel et al. (2010), Cincinnati implemented a group violence reduction strategy

that was modeled after the pulling levers focused deterrence strategy implemented in Boston and included law enforcement consequences for violence, along with social service opportunities and community engagement. In face-to-face offender notification meetings, police, community activists, political figures, civil rights activists, ex-offenders, parents of murdered children, social service providers, medical personnel, and business, civic and religious leaders told members of violent groups that the violence must stop, that there would be law enforcement consequences for the entire group if it did not, and that the community would support these consequences (Engel et al., 2010). The working group partners also told violent group members that there was social service help for all who wanted it.

In series of unpublished reports, researchers from the Policing Institute at the University of Cincinnati completed ongoing preliminary evaluations of the impact of the CIRV interventions (Engel et al. 2008; Engel et al., 2009; Engel et al., 2010). The CIRV intervention was implemented in July 2007; members of criminally-active street groups in Cincinnati were the target population for the pulling levers focused deterrence strategy. The research team used Poisson regression models controlling for trends and month seasonal variations to estimate the effects of the CIRV treatment on monthly counts of group-member-involved (GMI) homicides in Cincinnati. As a comparison, they also ran the same Poisson model on non-GMI homicides in Cincinnati. The authors also used a similar negative binomial regression model to examine the CIRV on total shootings in Cincinnati. The citywide shooting data were not further parsed to conduct a comparative analysis of GMI and non-GMI shootings over time. Using a dummy variable to indicate the presence or absence of the CIRV treatment, the pre-intervention period comprised January 2004 through June 2007 and the post-intervention period comprised July 2007 through July 2010.

These analyses revealed that the CIRV strategy was associated with a statistically significant 35 percent reduction in the monthly count of GMI homicides between the pre-intervention and post-intervention time periods ($p<.10$). Non-GMI homicides experienced a non-statistically significant 35 percent increase between the pre-intervention and post-intervention time periods. The analyses also revealed a statistically significant 21 percent reduction ($p<.05$) in the monthly count of citywide total shootings associated with the implementation of the CIRV strategy.

13.6 OPERATION CEASEFIRE IN NEWARK, NEW JERSEY

In an unpublished report, researchers from the Violence Institute of New Jersey at the University of Medicine and Dentistry of New Jersey (UMDNJ) evaluated the Operation Ceasefire gang violence strategy in Newark, New Jersey (Boyle et al., 2010). The Newark Ceasefire strategy focused on preventing gun violence by individual gang members in a targeted "Ceasefire Zone." According to Boyle et al. (2010), the Newark strategy blended the law enforcement actions developed by the Boston Ceasefire pulling levers strategy (Kennedy et al., 1996) with the public health

violence prevention activities developed by CeaseFire Chicago (Skogan et al., 2008). Shooting teams of detectives from the Newark, Irvington, and New Jersey State police departments aggressively investigated fatal and non-fatal shootings in the Ceasefire Zone. Parole officers also closely monitored high-risk individuals in the targeted area.

Drawing on the Chicago approach, Newark Ceasefire addressed risk and protective factors for individual gang members through five program components: public education, community mobilization, faith-based leader involvement, youth outreach, and criminal justice system engagement. Ceasefire youth outreach workers attempted to change the way gang members thought about and reacted to violence and to connect them with available services and opportunities. While there were not any formal offender-notification strategies in place, the participating law enforcement agencies, community groups, and outreach workers actively communicated with individual gang members to prevent retaliatory shootings and disrupt ongoing conflicts.

The Ceasefire Zone was a roughly two-square mile section of Newark that experienced elevated levels of gun homicides and shooting incidents. The Newark Ceasefire intervention was implemented on May 11, 2005. The evaluation team used ARIMA models to examine non-fatal gunshot wound trends in the Ceasefire Zone, a comparison zone, and the remainder of the City of Newark minus the Ceasefire Zone (Boyle et al., 2010). The comparison zone was identified through spatial analyses of non-fatal gunshot wounds to identify an area of similar size with similar levels of gun violence in Newark and also matched to the Ceasefire Zone based on 2000 Census data on the number of block groups in each area, population, resident race and ethnicity, median resident age and household income, concentrated poverty, and vacant housing units.

In the Newark Ceasefire evaluation, the key outcome variable was the weekly number of non-fatal gunshot wound victims treated at the Trauma Center at University Hospital in Newark in the Ceasefire Zone, comparison zone, and remainder of Newark between January 1, 2004 and December 31, 2006 (Boyle et al., 2010). These victimizations were geocoded by the location of gunshot wounding and then aggregated into weekly counts in the larger areas in which the events were contained. The ARIMA model estimated that the Newark Ceasefire intervention was associated with a non-statistically significant decrease in the weekly number of non-fatal gunshot wound victims in the Ceasefire Zone. The comparison zone also experienced a smaller, non-significant decrease in the weekly number of non-fatal gunshot victims and the remainder of Newark experienced a non-significant increase in the weekly number of non-fatal gunshot victims. As such, the evaluators concluded that Newark Ceasefire was not associated with any significant reductions in non-fatal gunshot wounds.

The researchers also used crime mapping software to examine potential crime displacement and diffusion effects in the areas immediately surrounding the Ceasefire Zone. While the researchers noted changes in the spatial distributions of non-fatal gunshot woundings in the areas surrounding the Ceasefire Zone, they concluded that their analyses could not link the development of new gun violence hot spots and "cold spots" to displacement and/or diffusion processes associated with the Ceasefire intervention.

13.7 OPERATION CEASEFIRE IN LOS ANGELES, CALIFORNIA

In March 1998, NIJ funded the RAND Corporation to develop and test strategies for reducing gun violence among youth in Los Angeles. In part, the goal was to determine which parts of the Boston Gun Project might be replicable in Los Angeles. In designing the replication, RAND drew a clear distinction between the process governing the design and implementation of the strategy (data-driven policy development; problem solving, working groups) and the elements and design (pulling levers, collective accountability, retailing the message) of the Boston model. Processes, in theory, can be sustained and adaptive, and as such can be utilized to address dynamic problems. By singling out process as an important component, the RAND team hoped to make clear that process can affect program effectiveness independently of the program elements or the merits of the actual design (see Tita, Riley, and Greenwood, 2003).

The Los Angeles replication was unique in several important ways. First, the implementation was not citywide, but only within a single neighborhood (Boyle Heights) within a single Los Angeles Police Department Division (Hollenbeck). The project site, Boyle Heights, had a population that was relatively homogenous. Well over 80% of the residents were Latinos of Mexican origin. The same was true for the gangs, many of which were formed prior to the Second World War. These gangs were clearly "traditional" gangs, with memberships exceeding a hundred members or more. The gangs were strongly territorial, contain age-graded sub-structures, and are inter-generational in nature (Tita, Riley, and Greenwood, 2003).

Unlike other cities where gang and group-involved violence was a rather recent phenomenon, Los Angeles represented an attempt to reduce gun violence in a "chronic gang city" with a long history of gang violence, and equally long history of gang reduction strategies. The research team had to first convince members of the local criminal justice and at large community that the approach we were espousing differed in important ways from these previous efforts to combat gangs. And in fact it does – the RAND project was not about "doing something about gangs," but rather "doing something about gun violence" in a community where gang members committed an overwhelming proportion of gun violence. The independent analysis of homicide files confirmed the perception held by police and community alike that gangs were highly over-represented in homicidal acts. From 1995-1998, 50% of all

homicides had a clear gang motivation. Another 25% of the homicides could be coded as "gang related" because they involved a gang member as a victim or offender, but were motivated for reasons other than gang rivalries.

Given the social organization of violence in Boyle Heights, the multi-disciplinary working group fully embraced the pulling levers focused deterrence strategy developed in Boston. A high-profile gang shooting that resulted in a double homicide in Boyle Heights triggered the implementation of the Operation Ceasefire intervention in October 2000. The processes of retailing the message were formally adopted, though it was mostly accomplished through personal contact rather than in a group setting. Police, probation, community advocates, street gang workers, a local hospital and local clergy were all passing along the message of collective accountability for gangs continuing to commit gang violence. Unfortunately, Tita, Riley, Ridgeway et al. (2003) reported that the Los Angeles pulling levers intervention was not fully implemented as planned. The implementation of the Ceasefire program in the Boyle Heights was negatively affected by the well-known Ramparts LAPD police corruption scandal and a lack of ownership of the intervention by the participating agencies.

Despite the implementation difficulties, the RAND Corporation evaluated the Operation Ceasefire pulling levers strategy to reduce gun violence among gangs in the Boyle Heights area of Los Angeles (Tita et al., 2003). In their evaluation, RAND researchers examined the effects of the pulling levers gang violence reduction strategy on violent crime (homicides, attempted homicides, robberies, assaults, and kidnappings), "gang crime" (violent crime and terrorist threats, firearm discharge, vandalism, and graffiti committed by gang members), and gun crime (any of the above crimes that involved use of a firearm).

The RAND evaluation analyzed changes in their key outcome variables for three time periods across three comparison areas (Tita et al., 2003). The three time periods were the six months prior to the triggering event—the *pre-intervention* period; the four months in which all parts of the intervention were applied—the *suppression* period; and the two months in which only selected parts of the intervention were applied, such as heightened patrol of public housing units in the area and greater enforcement of probation and parole regulations—the *deterrence* period. The three comparison areas were (1) Boyle Heights compared with the remainder of the Hollenbeck area, (2) the five police reporting districts where the intervention was targeted compared with the remainder of Boyle Heights, and (3) the Census block groups comprising the turf of the targeted gangs compared with a group of Census block groups scattered throughout Hollenbeck that most closely matched the characteristics of the targeted area based on a propensity score analysis. In explaining the rationale for their research design, the RAND researchers reported:

A reduction in crime in the treatment areas greater than those in the comparison areas during the suppression period would help show the effects of all measures combined, whereas continuing reductions in the deterrence period would suggest that the intervention may have had some long-term effects in changing behavior, or that short-term application of some resources can produce a long-term deterrence effect (although we recognize our measure of deterrence is confounded by the continuation of some suppression activities). (Tita et al., 2003: 24 – 25).

The evaluation used a variety of approaches to detect the effects of the Ceasefire intervention across the study time period (Tita et al., 2003). The RAND researchers used Bayesian analyses of the count-based distributions of the outcome variables across these time periods in Boyle Heights relative to the remainder of Hollenbeck comparison and in the five targeted reporting districts relative to the remainder of Boyle Heights comparison. In their analyses of targeted Census block groups relative to matched comparison Census block groups, the RAND researchers used Probit and step-wise linear regression models to define the matched comparison Census block groups. They then used a slightly more rigorous version of the "difference in differences" approach that assumed the level of crime followed a Poisson distribution and considered serial time trends to evaluate the effects of the Ceasefire intervention across these time periods (Tita et al., 2003).

Their statistical analyses revealed that gang crime in Boyle Heights 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. The analyses suggested that the significant reduction in gang crime may have begun in the suppression period. Violent crime, however, did not decrease significantly in the suppression 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. Neither gang crime in the deterrence period nor gun crime in the deterrence or suppression periods decreased significantly in comparison with the remainder of Boyle Heights. The RAND evaluation also reported that, in the Census block groups overlapping the targeted reporting districts, violent crime decreased significantly compared with the matched blocks (Tita et al., 2003). Their analyses also suggested that some of this significant reduction may have persisted into the deterrence period.

In addition to their analyses of the main effects of the intervention, RAND researchers examined the effects of the intervention on neighboring areas and gangs. Their analyses suggested a strong diffusion of violence prevention benefits emanating from the targeted areas and targeted gangs (Tita et al., 2003). In the six months after the intervention, the researchers reported in the 6 targeted Census block groups that violent crime had decreased by 34 percent, gang crime decreased by 28 percent, and gun crime decreased by 26 percent. In the 11 Census block

groups immediately surrounding the targeted block groups, violent crime had decreased by 33 percent, gang crime decreased by 44 percent, and gun crime decreased by 28 percent. The RAND research team also examined gang crime by gangs not targeted by the Ceasefire intervention that were "socially tied" through conflicts and alliances to the target gangs. After the Ceasefire intervention was implemented, gang crimes committed by the targeted gangs and the non-targeted, socially-tied gangs decreased by a matching 26 percent.

13.8   PROJECT SAFE NEIGHBORHOODS IN CHICAGO, ILLINOIS

The PSN was implemented in two adjacent police districts in Chicago's West Side where rates of murder and gun violence were more than four times higher than the city average in 2002. As described by Papachristos, Meares, and Fagan (2007), the PSN team reasoned that the best way to address Chicago's homicide and gun violence problem was to craft intervention strategies focused on the population with a very high risk of being a victim or offender of gun violence in the targeted neighborhoods. Two principles guided the design and development of PSN interventions: (1) that enforcement efforts be highly specified and targeted to those most at risk of being a victim and offender of gun violence, and (2) that serious efforts should be directed towards changing the normative side of gun violence, i.e., the reasons young men use guns and their attitudes towards the law and law enforcers. Following these principles the PSN team devised several law enforcement, community outreach, and offender notification forums and follow-up re-entry programs. The PSN interventions were implemented in May 2002.

A quasi-experimental design was used to evaluate the impact of the various PSN programs on neighborhood-level homicide rates in Chicago (Papachristos et al., 2007). As described, two adjacent police districts were nonrandomly selected from the city's 25 police districts as PSN treatment districts and, via propensity score matching procedures, two other police districts selected as near-equivalent controls. Monthly and quarterly counts of homicide incidents between January 1999 and December 2004 were identified as the key outcome variables (Papachristos et al., 2007; Meares et al., 2009); however, the evaluation also analyzed monthly and quarterly counts of gun homicide incidents, gang homicide incidents, and aggravated assault incidents in the treatment districts relative to the control districts.

The research team analyzed the overall effects of the PSN treatment as well as the four interventions that comprised the PSN treatment: (1) increased federal prosecutions for convicted felons carrying or using guns, (2) the length of sentences associated with federal prosecutions, (3) supply-side firearm policing activities (gun recoveries by ATF-CPD gun teams), and (4) social marketing of deterrence and social norms messages through justice-style offender notification meetings. In these offender notification meetings, randomly selected gun- and gang-involved recently-

released former prison inmates returning to the treatment districts were informed of their vulnerability as felons to federal firearms laws, with stiff mandatory minimum sentences; offered social services; and addressed by community members and ex-offenders. Using individual growth curve regression models, the research team found that the PSN treatment was associated with a statistically significant 37 percent reduction in the number of homicides in the treatment district relative to the control districts.   The overall PSN treatment was also associated with statistically significant decreases in gun homicide incidents and aggravated assault incidents, and a non-statistically significant decrease in gang homicide incidents.

The PSN intervention that generated the largest, statistically significant effect on decreased homicide in the treatment districts relative to control districts was the offender notification forums. In short, the greater the proportion of offenders who attended the forums, the greater the decline in treatment district levels of homicide. Increased federal prosecutions and the number of guns recovered by the gun teams were associated with modest but statistically significant declines in homicides in the treatment districts relative to the control districts. Getting more guns off the street and prosecuting more offenders federally for gun crimes were associated with small but meaningful homicide decreases. The length of sentences associated with federal prosecutions was not associated with the observed homicide decreases.

In a supplemental unpublished analysis, Fagan et al. (2008) analyzed recidivism rates of individuals who participated in the PSN notification forums. Using survival analyses, the authors found that those who attended a PSN forum were 30 percent less likely to be rearrested relative to a comparison group of similar recently-released individuals from the same neighborhood. The program diminished recidivism levels for both gang and non-gang members and seemed to be particularly effective for individuals who had only one prior felony conviction.

13.9   DRUG MARKET INTERVENTION IN NASHVILLE, TENNESSEE

In an unpublished report, Corsaro and McGarrell (2009) evaluated the impact of a pulling levers focused deterrence strategy to reduce crime and disorder problems associated with an illegal drug market operating in the McFerrin Park neighborhood of Nashville, Tennessee. Drawing on similar intervention conducted in High Point, North Carolina (Kennedy, 2009), the project employed a joint police-community partnership to identify individual offenders, notify them of the consequences of continued dealing, provide supportive services through a community-based resource coordinator, and convey an uncompromising community norm against drug dealing. This application of focused deterrence is generally referred to as the "Drug Market Intervention" (DMI) strategy.

The DMI seeks to shut down overt drug markets entirely (Kennedy, 2009). Enforcement powers are used strategically and sparingly, employing arrest and

prosecution only against violent offenders and when nonviolent offenders have resisted all efforts to get them to desist and to provide them with help. Through the use of "banked" cases,[12] the strategy makes the promise of law enforcement sanctions against dealers extremely direct and credible, so that dealers are in no doubt concerning the consequences of offending and have good reason to change their behavior. The strategy also brings powerful informal social control to bear on dealers from immediate family and community figures. The strategy organizes and focuses services, help, and support on dealers so that those who are willing have what they need to change their lives. Each operation also includes a maintenance strategy.

The strategy was implemented in March 2008 and the evaluation examined outcome data for the March 2005 through April 2009 time period. The evaluation measured the effects of the DMI intervention on five outcome variables: violent crime (the aggregated number of homicide, forcible rape, robbery, and aggravated assault incidents), property crime (the sum of burglaries, larcenies, and motor vehicle thefts), illegal drug possession offenses, drug equipment offenses (the monthly number of charges for drug-paraphernalia and related crimes), and total calls for service. The researchers analyzed the aggregated monthly number of these outcome variables for the following Nashville areas: 1) the McFerrin Park target neighborhood to assess the local effect; 2) adjoining, contagious areas to the McFerrin Park neighborhood to assess whether a local displacement or a diffusion of benefits occurred; and 3) the remainder of Davidson County, once the target and adjoining areas were subtracted from the county totals for general trend comparison purposes.

ARIMA models with a dummy variable to represent the implementation of the DMI strategy were used to analyze trends in the treatment, adjoining, and comparison areas. The analyses reported that the DMI intervention was associated with a statistically significant 28.4 percent reduction in property offenses, a statistically significant 55.5 percent reduction in illegal drug possession offenses, and a statistically significant 36.8 percent reduction in drug equipment offenses. The analyses also found that the DMI intervention was associated with non-statistically significant reductions in violent crime incidents and total calls for service. The evaluation did not find any significant reductions in any of the key outcome variables in the remainder of Davidson County. The researchers did find, however, statistically significant reductions in illegal drug possession offenses, drug equipment offenses, and total calls for service in the adjoining area. This suggested that the DMI intervention was associated with a noteworthy diffusion of crime control benefits beyond the McFerrin Park target neighborhood.

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[12] A "banked" case refers to a potential prosecution for narcotics sales, supported by audio and video evidence usually obtained through a controlled buy that is held in inactive status unless the subject of the prosecution continues dealing, at which point an arrest warrant is issued and prosecution proceeds (Kennedy, 2009).

13.10 DRUG MARKET INTERVENTION IN ROCKFORD, ILLINOIS

Corsaro et al. (2010) evaluated the impact of a pulling levers focused deterrence strategy to reduce crime and disorder problems associated with an illegal drug market operating in the Delancey Heights neighborhood of Rockford, Illinois. Like the Nashville strategy described above, this research and development study was a replication of the High Point, North Carolina DMI (Kennedy, 2009). The strategy was implemented in May 2007 and the evaluation examined outcome data for the June 2006 through June 2008 time period. The evaluation measured the effects of the DMI intervention on two outcome variables: violent crime (the aggregated number of homicide, rape, kidnapping, robbery, and aggravated assault incidents) and nonviolent crime (the aggregate number of property, drug, and nuisance crime incidents). The researchers analyzed the aggregated monthly number of these outcome variables for the Delancey Heights neighborhood and for the remainder of Rockford without Delancey Heights.

Hierarchical generalized linear growth curve regression models with a dummy variable to represent the implementation of the DMI strategy were used to analyze trends in the treatment and comparison areas. The analyses reported that the DMI intervention was associated with a statistically significant 22 percent reduction in nonviolent offenses and a non-statistically significant reduction in violent offenses in the Delancey Heights target neighborhood. The evaluation did not find any significant reductions in either violent offenses or nonviolent offenses in the remainder of Rockford. Corsaro et al. (2010) also presented qualitative data from interviews with 34 adult residents from the Delancey Heights neighborhood. The authors reported that the majority of the residents interviewed noted considerable crime and disorder improvements in their neighborhood after the DMI was implemented.

14 Appendix D: List of Effect Sizes for All Outcomes for Eligible Studies

Study	Outcome	Effect Size (std. diff. in means)	Std. Error
Boston, MA	Gun assaults	0.439	0.238
Boston, MA	Shots fired calls	0.365	0.237
Boston, MA	Youth gun assaults	0.617	0.240
Boston, MA	Youth homicides	1.161	0.251
Chicago, IL	Agg. Batteries	0.140	0.061
Chicago, IL	Gang homicides	0.089	0.061
Chicago, IL	Gun homicides	0.256	0.061
Chicago, IL	Homicides	0.237	0.061
Chicago, IL	New arrest	0.434	0.050
Cincinnati, OH	GMI homicides	0.352	0.224
Cincinnati, OH	Total shootings	0.468	0.225
Honolulu, HI	New arrest	0.666	0.115
Indianapolis, IN	Total homicides	1.039	0.283
Los Angeles, CA	Gang crimes	0.165	0.292
Los Angeles, CA	Gun crimes	0.275	0.355
Los Angeles, CA	Violent crimes	1.255	0.399
Lowell, MA	Gun assaults	1.186	0.207
Nashville, TN	Calls for service	0.520	0.311
Nashville, TN	Drug equip. offenses	0.832	0.318
Nashville, TN	Narcotics offenses	1.512	0.341
Nashville, TN	Property incidents	0.850	0.318

Study	Outcome	Effect Size (std. diff. in means)	Std. Error
Nashville, TN	Violent incidents	0.476	0.310
Newark, NJ	Gunshot wounds	0.225	0.160
Rockford, IL	Non-violent offenses	0.882	0.291
Rockford, IL	Violent offenses	0.160	0.278
Stockton, CA	Gun homicides	0.763	0.157

15 Appendix E: Discussion of Hawaii Opportunity with Probation Enforcement

Hawaii Opportunity with Probation Enforcement (HOPE) intervention was a community supervision program aimed at substance-abusing probationers (Hawken and Kleiman, 2009). The program relied on a mandate to abstain from illicit drugs, backed by swift and certain sanctions for drug test failures, and preceded by a clear and direct warning. Probationers were sentenced to drug treatment only if they continued to test positive for drug use, or if they requested a treatment referral. The deterrence-based HOPE intervention differs significantly from typical drug court operations as it economizes on treatment and court resources. As Hawken and Kleiman (2009) suggest, HOPE does not mandate formal treatment for every probationer, and does not require regularly scheduled meetings with a judge; probationers appear before a judge only when they have violated a rule. HOPE is often linked to the DMI approaches as a related application of focused deterrence (see, e.g., Boyum, Caulkins, and Kleiman, 2011) as well as gang and group-based pulling levers focused deterrence based on the common strategy of certain punishment for offenders (Durlauf and Nagin, 2011).

The HOPE evaluation used a randomized controlled trial among general-population substance-abusing probationers where probationers assigned to treatment conditions were compared to probationers assigned to probation-as-usual control conditions (Hawken and Kleiman, 2009). In their unpublished report to the U.S. National Institute of Justice, Hawken and Kleiman (2009) state that that HOPE relies on a mandate to abstain from illicit drugs, backed by swift and certain sanctions and preceded by a clear and direct warning. Unlike most diversion programs and drug courts, it does not attempt to impose drug treatment on every participant. Under HOPE, probationers are sentenced to drug treatment only if they continue to test positive for drug use, or if they request a treatment referral. According to Hawken and Kleiman (2009), HOPE should be considered to be distinct from drug courts in economizing on treatment and court resources (probationers appear before a judge only when a violation is detected). HOPE's stated goals are reductions in drug use, new crimes, and incarceration.

The randomized controlled trial used an intent-to-treat design where all offenders randomly allocated to the treatment condition were included in the HOPE group

whether they formally entered the program or not. Of the eligible probationers, two thirds were assigned to the HOPE treatment (n = 330) and one third were assigned to the control group (n = 163). Ninety-three percent of the probationers assigned for treatment appeared for their initial HOPE warning hearing and participated in the intervention. The experiment commenced in October 2007 and the intervention period lasted for one year.

Based on their analyses of the experimental data, Hawken and Kleiman (2009) concluded that HOPE was very effective in changing the behaviors of substance-abusing probationers. Only 21 percent of HOPE probationers experienced new arrests as compared to 47 percent of control probationers ($p<01$). HOPE probationers outperformed control probationers on a number of other performance measures such as missed probation appointments (treatment = 9%, control = 23%), positive urine drug test results (treatment = 13%, control = 46%), revocation rates (treatment = 7%, control = 15%), and the number of days sentenced to incarceration (treatment = 138 days, control = 267 days).

Based on our selection criteria, HOPE was not included in our final review. However, as stated earlier, several scholars believed that HOPE does fit within the general framework of pulling levers focused deterrence strategies. We agree that it is broadly similar to another evaluation included in our systematic review that is focused on a corrections population - Chicago's Project Safe Neighborhoods intervention (Papachristos et al., 2007). The key elements of Chicago PSN strategy are administered by the Illinois Department of Correction and the U.S. Attorney's Office (the call-in session is given to returning parolees to selected neighborhoods). The contribution of the Chicago Police Department is limited to increasing their gun policing efforts in the selected neighborhoods. The CPD does not select the returning parolees for the intervention nor do they run the communications strategy. Their only role is to increase gun recoveries and arrest those who commit violent crimes in these neighborhoods.

Moreover, probation has a central role in all of the gang / group-based focused deterrence interventions included in our review. Monitoring offenders in the community to ensure they are abiding by probation conditions, changing conditions, and revoking probation are key levers that are pulled in the application of focused deterrence strategies to gangs and criminally-active groups. In interagency working group settings, all involved agencies govern the shape and content of the pulling levers interventions. While the police convene the working group meeting, they share governance with the other criminal justice agencies, social service providers, and community members in the group. Probation is involved as a key decision maker in the process.

Most applications of pulling levers focused deterrence strategies have therapeutic elements (e.g. Braga et al., 2001; Papachristos et al., 2007). Indeed, the working group has social service providers, street outreach workers, and community

members as core members. A vital part of the communications strategy to pair threats of sanctions with offers of help (job training / placement, education, substance abuse counseling, etc.). All targeted gangs and groups are offered services throughout the entire process. Under HOPE, probationers are sentenced to drug treatment only if they continue to test positive for drug use, or if they request a treatment referral. HOPE is distinct from drug courts in economizing on treatment and court resources (probationers appear before a judge only when a violation is detected).

If HOPE is included in our review, the substantive findings do not change (see Braga and Weisburd, 2012). Our narrative review of program effects would change to report that there were 10 out of 11 eligible evaluations with positive findings (two-tailed $p = 0.0117$). Our main meta-analysis of the effects of pulling levers focused deterrence strategies would still be limited to the ten studies that evaluated the impact of these interventions on crime outcomes at the area-level. As described by Lipsey and Wilson (2001), it is problematic to combine effect sizes from studies with very different units of analysis (such as combining studies focused on people with studies focused on places). We can, however, separately calculate and examine effect sizes for the HOPE intervention's impact on recidivism by individual probationers and the Chicago PSN intervention's impact on recidivism by individual recently-released prison inmates.

As already described in our systematic review, the Chicago PSN quasi-experimental evaluation conducted additional analyses of the effects of the intervention on individual recidivism patterns. The HOPE randomized controlled experiment also examined the effects of the treatment on individual recidivism patterns in Honolulu. Both studies measured recidivism as a new arrest by treatment and control individuals. The HOPE effect size was calculated by using the raw data from Hawken and Kleiman (2009: 63-64) to construct a two by two table and entering the cell counts in the Effect Size Calculator. The more conservative Logit method was used to calculate the effect size. We calculated effect sizes for the Chicago (.434, $SE = .050$, $p < .05$) and Honolulu (.666, $SE = .115$, $p < .05$) studies. When the effect sizes for these studies were combined via a fixed effects meta-analysis model (Q = 3.391, df = 1, $p = 0.066$), the overall effect size was medium and statistically significant (.471, $SE = .046$, $p < .05$), suggesting a program impact similar to area-level impacts generated by the DMI and gang / group interventions.