SYSTEMATIC REVIEW PROTOCOL
COVER SHEET

SPATIAL DISPLACEMENT AND DIFFUSION OF BENEFITS AMONG GEOGRAPHICALLY FOCUSED POLICING INITIATIVES

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1. Background[3]

One of the most common criticisms of focused policing efforts is that crime will simply relocate to other times and places since the "root causes" of crime were not addressed or because offenders may remain on the streets after certain crime opportunities are reduced. This phenomenon—called crime displacement—has important implications for many policing projects. By far spatial displacement is the form most commonly recognized (Eck, 1993), though the other five are also frequently acknowledged by those studying crime prevention. Formally, the six possible forms of displacement include: temporal (offenders change the time at which they commit crime), spatial (offenders switch from targets in one location to targets in another location), target (offenders change from one type of target to another target type), tactical (offenders alter the methods used to carry out crime), offense (offenders switch from one form of crime to another), and offender[4] (new offenders replace old offenders who have been removed or who have desisted from crime).[5] At the extreme, widespread displacement stands to undermine the effects of geographically focused policing actions. More often, however, emerging research suggests that crime displacement is rarely total. On the other end of the displacement continuum is the phenomenon of diffusion of crime control benefits (Clarke and Weisburd, 1994; see Figure 1). Crime diffusion is the reverse of displacement and its occurrence has been documented in several crime prevention evaluations (Bowers and Johnson, 2003; Chaiken, Lawless, and Stevenson, 1974; Green, 1995; Miethe, 1991; Weisburd et al., 2006; Weisburd and Green, 1995). Diffusion occurs when reductions of crime (or other improvements) are achieved in areas that are close to crime prevention interventions, even though those areas were not actually targeted by the intervention itself (Clarke and Weisburd, 1994). This feature of crime prevention activity has been referred to in a variety of ways including the "bonus effect," the "halo effect," the "free-rider effect," and the "multiplier effect." In cases where any degree of diffusion is observed the benefit of any treatment effects experienced in the targeted area are amplified since improvements were gained without expending resources in those areas. While there have been some noted experiments on the extent of displacement and diffusion following focused policing efforts which suggests this is the case, a systematic appraisal of all the available evidence this topic remains missing.

Overall, displacement is viewed as a negative consequence of crime prevention efforts, but even when displacement does occur it can still provide some benefit. For example, the *volume* of crime shifted could be less. A treatment area may experience a reduction of 100 crimes post intervention, whereas the displacement of crime may only result in an increase in the adjacent area of (say) 50 crimes post intervention. Thus, a net reduction of 50 crimes would still be achieved. Further, Barr and Pease (1990) contend that crime dislocation from more serious to less serious types of crime (such as the shift from robbery to petty thefts) is in effect "benign" since it produces less harm.[6]

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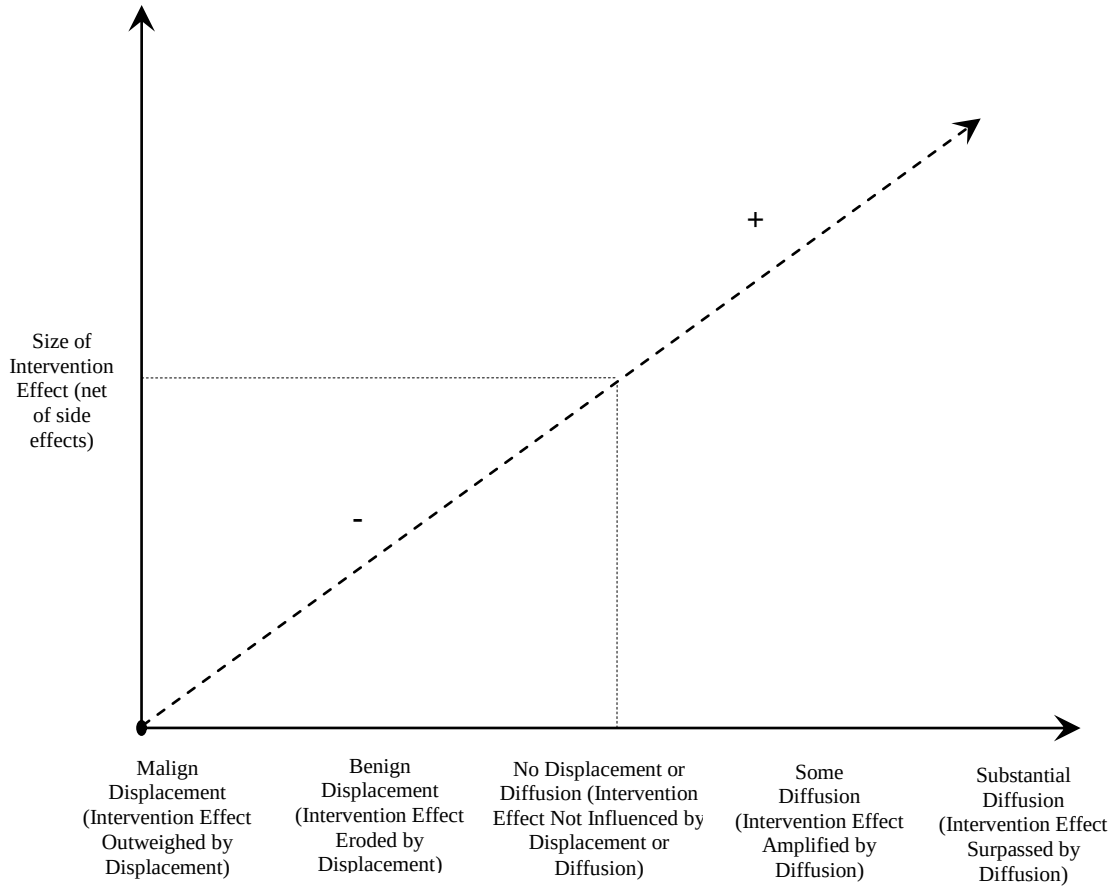
[3] This section was adapted from Guerette and Bowers (2009).

[4] Though offender displacement is often mentioned as a sixth type it is more accurate to describe this as offender *replacement* since it entails new offenders taking the place of other offenders who have been arrested or who have desisted from crime. Thus, it is not a form of displacement, which is a term reserved for changes that original offenders make so they can continue to offend when faced with reduced opportunities.

[5] For more on this see Reppetto (1976).

[6] The first to note this was Brantingham (1986) where she writes, "Displacement is always a possibility, and while the displacement of crime through a planning intervention has target-specific value, it has no overall value unless it takes the form of displacement from more serious forms of criminal behavior to a less serious form."

Figure 1. Relationship of Displacement and Diffusion to an Observed Intervention Effect



Benign displacement could occur in several ways: i) The redistribution of concentrated crime across a bigger pool of *victims* (i.e. relocating victimization from a small group of repeat victims to a larger pool of victims, as noted by Barr and Pease, 1990); ii) The transference of crime away from more *vulnerable groups* of the population (e.g. children and the elderly); iii) The relocation of crime to *places* where the community impact is less harmful. This could take two forms: a) the *relocation* of a street drug or prostitution market from a residential area to a remote area would produce less community harm, such as fear of crime or less residential and business decay; and b) the *dispersion* of the same volume of crime to a larger area where the harm is less concentrated. In short, 'benign' displacement could occur when the displacement is of lower volume, results in less harm, or is less severe.

Not all displacement is benign and at times it can lead to more harmful consequences. This occurs when there is a shift to more serious offenses or to offenses which have more serious consequences (Barr and Pease, 1990). Referred to as "malign" displacement, it would conceivably involve any situation where the relocation of crime made matters worse. This could be an increase in the volume of crime at the relocated area, the concentration of crime to a smaller group of victims, the relocation of crime to places where it has greater impact on the

community, or the relocation of crime to more vulnerable groups of the population. Only when the benefits of any crime prevention initiative achieved are outweighed by the harm and/or volume of displaced crime can the prevention effort be found ineffective (see Figure 1).

Much of the discourse surrounding whether displacement will or will not occur stems from divergent theoretical views of criminality but exactly how these theories apply to displacement is open to some interpretation. A common reading is that deterministic theories which view crime behavior as a result of influences such as unemployment, sub-cultural values, strained economic opportunities, etc., predict that blocking crime opportunities through situational alterations will inevitably lead offenders to seek out other crime opportunities (Clarke and Eck, 2005; Eck, 1993; Weisburd, et al. 2006). This is because criminal propensities are viewed as ongoing and undetermined by situational characteristics. Thus, in part, displacement would have to occur if deterministic theories of crime are correct.[7]

Rational choice theory, in contrast, views criminal behavior as a product of choices and decisions made by the offender (Cornish and Clarke, 1986) which are largely influenced by existing opportunities for crime. This view does not see offenders as driven to commit crime, but rather as deciding to carry out crime as a way of satisfying some need or want. In this, a calculation of the expected effort, risk, and rewards involved in conducting crime is performed. Because these choices are derived from offender perceptions of the situational landscape, crime prevention efforts to block opportunities are expected to deter crime. From this perspective, displacement is less likely to occur in so far as the relative rewards are offset by the effort and/or risk involved for other crime places, times, targets, offenses, or tactics. Offender perceptions as to whether to displace their crime behavior will be shaped by the variety of "choice structuring properties" across crime type, time and place (Cornish and Clarke, 1986).

The rational choice perspective, then, provides an explanation for both the presence and absence of displacement. Offenders will only displace their crime behavior when the risks and effort of committing new crimes are worth the reward (Cornish and Clarke, 1986). Another aspect to consider under the rational choice perspective is that when crime opportunities are closed down other crime is not the only choice available for offenders to meet their needs. Blocking of crime opportunities makes satisfying individual needs through legitimate activities more appealing. For instance, a qualitative study of street prostitutes in Jersey City, New Jersey revealed that following a focused police crackdown on a prostitution market some prostitutes gave up the trade altogether (see Brisgone, 2004). Similarly, Mathews (1990) found that many prostitutes engaged in the trade since it was an easy way to make money but gave up prostituting following street closures and a policing crackdown in Finsbury Park, London which appeared to offset the ratio between the effort, risk and reward of engaging in sex acts in exchange for money.

Routine activity theory (Cohen and Felson, 1979) gives more insight into the nature of crime opportunity and also helps to understand whether displacement will occur. This theory holds that crime occurs when a suitable target, a motivated offender converge in space and time in the absence of a capable guardian. And, hence, that displacement may occur in the aftermath

[7] Another interpretation of deterministic theories, however, might contend that because offenders maintain some deep seated compulsion to commit crime they would be insensitive to the implementation of crime prevention schemes and would continue to offend in those areas targeted until incapacitated. Thus, displacement would not be predicted under this interpretation since offenders would be viewed as not possessing the capacity to make reasoned decisions as to when and where to offend in order to escape detection. However, this understanding may be overly reductionist since even committed dispositional theorists would recognize that situations play some part in crime, even if minor (for instance, see Sutherland, 1947, as noted by Weisburd et al., 2006:552).

of a situational intervention where there are other convergences of these three elements (i.e. where other suitable/substitutive crime opportunities are plentiful) but will not occur where one or more of these elements are missing.

The extent to which crime opportunity is constant has implications not only for understanding displacement but also for thinking about crime and its prevention more generally. Early criminological thinking, for instance, viewed opportunities for crime as infinitely numerous which meant that the idea of crime prevention through opportunity reduction was impractical (see Clarke and Felson, 1993; Weisburd et al. 2006:552). Instead, altering criminal dispositions was viewed to be a more promising approach to preventing crime. Later research which focused on understanding crime as opposed to criminality was at least partly energized by the notorious Martinson (1974) report, which harnessed the fields thinking about crime reduction through rehabilitation. Recent studies suggest that crime opportunity is not constant but rather has been shown to cluster in time and place (Brantingham and Brantingham, 1981; Sherman, Gartin, and Buerger, 1989), among victims (Pease, 1998) and among facilities (Eck, Clarke, and Guerette, 2007). If crime opportunity is infinitely continuous as originally thought, then displacement should occur at very high levels following situational alterations at existing crime places. If, however, there is discontinuity of crime opportunity then displacement should be constrained.

The rational choice perspective also explains the occurrence of diffusion of benefits. Two processes have been identified related to diffusion: deterrence and discouragement (Clarke and Weisburd, 1994). As a prevention program in one area becomes known, offenders' uncertainty about the extent of the increased risk (deterrence) is coupled with the exaggerated perception that the rewards of particular crimes are no longer proportionate with the associated effort (discouragement). Using these derivatives of the rational choice perspective gives explanation as to why diffusion has been observed in places near treatment areas.

It is important to note that firstly it is entirely possible that displacement and diffusion of benefit may co-exist such that the problem worsens in some places and improves in others. Secondly, it is possible (and probably likely) that diffusion and displacement are directional in nature (for example, there may be a drift in crime in one direction but not others). Unfortunately, the consideration of such patterns is rarely addressed in the research literature. Instead general overall changes in non-directional displacement catchment areas surrounding an action[8] area are most commonly reported.

Prior reviews assessing displacement and diffusion

The most encompassing type of displacement research are literature reviews of empirical studies reporting on displacement, yet until recently there had only been three (Barr and Pease, 1990; Eck, 1993; and Hesseling, 1994) and there had not been any published systematic reviews of diffusion of benefits (Weisburd et al., 2006).[9] Results from each of the early displacement reviews were largely consistent in finding that displacement was often not observed and in cases where it was, it tended to be less than the gains achieved by the intervention. Of the 33 studies reviewed by Eck (1993), 91 percent found no or little displacement (e.g. displacement less than the treatment gain) and only three (9%) reported a substantial amount. Similarly, Hesseling

[8] Please note that 'action' area and 'treatment' area are used interchangeably here

[9] One caveat is that Hesseling (1994) did report on observed diffusion effects in 6 of the 55 studies reviewed as reported herein.

(1994) found that 40 percent of the 55 studies reviewed reported no displacement at all, and of these 6 reported diffusion of benefits. Finally, Barr and Pease (1990) took a different approach using a selective review of various crime topics and noted that in some cases, even in the minority event of total displacement, a redistribution of crime still achieved a desirable social gain.

Despite these mostly consistent findings, these early reviews of displacement research were limited in several ways. First, they were based on a small number of studies available for review at the respective time. In the fourteen years since the last review many more studies have been produced, notably as a byproduct of the increasing popularity that geographically focused prevention efforts have garnered. Second, all of the reviews were descriptive in their method. This was mostly due to the lack of data provided by individual study authors which allowed for more definitive determinations of displacement levels. In many cases, the reviewer was limited by the authors' reporting of whether displacement was or was not observed prima fascia. Third, even when sufficient data was reported available statistical methods allowing for more reliable empirical determinations of the extent of displacement (e.g. determinations of overall treatment effects while taking into account displacement and diffusion effects) have only recently been developed (Bowers and Johnson, 2003; Clarke and Eck, 2005).

Recently, a review of displacement and diffusion effects among situational crime prevention (SCP) initiatives sought to overcome these limitations (Guerette and Bowers, 2009). That review examined 102 evaluations of situational focused crime prevention projects in an effort to determine the extent to which crime displacement was observed. It was found that of the 102 studies which examined (or allowed for examination of) displacement and diffusion effects there were 574 observations; that is, some studies reported results for more than one treatment and displacement catchment area and/or more than one crime type. Of those observations, displacement was observed in 26 percent. The opposite of displacement, diffusion of benefit, was observed in 27 percent of the observations. Moreover, analysis of 13 studies which allowed for assessment of overall outcomes of the prevention project while taking into account spatial displacement and diffusion effects revealed that when spatial displacement did occur it tended to be less than the treatment effect, suggesting that the intervention was still beneficial. *That study, however, focused exclusively on situational crime prevention initiatives and did not assess the extent of displacement and diffusion among focused policing interventions.*

2. Objectives

The purpose of this systematic review is to determine the empirical extent of geographical displacement and diffusion of benefits among focused policing interventions. It will assess the magnitude of any displacement or diffusion observed in relation to any crime reduction successes achieved by the intervention. In doing so, it seeks to compliment the review on displacement and diffusion effects among situational interventions (Guerette and Bowers, 2009) to more completely understand the prevalence and nature of geographical displacement and diffusion. Thus, it will ask: To what extent does displacement and diffusion occur in the aftermath of focused policing efforts? Does it vary by the type of focused policing effort employed? Does it vary by the scale of the treatment? Does it vary across different types of location? How localized are the displacement and/or diffusion effects? How does this compare to what we know about the nature and extent of displacement and diffusion among situational crime prevention initiatives?

3. Methods

This review will be guided by procedures used in two other previous Campbell reviews conducted on the effectiveness of problem-oriented policing (Weisburd, Telep, Hinkle and Eck 2008) and the effects of hot-spots policing on crime (Braga, 2007) as well as those used in a recent review of displacement and diffusion effects among situational crime prevention evaluations (Guerette and Bowers, 2009).

3.1 Criteria for inclusion and exclusion of studies in review

To be included in the review the following conditions will need to be satisfied:

1. The study must evaluate a focused policing intervention which entails one of the following:
    1. Hotspot policing/ directed patrol
    2. Police crackdown
    3. Problem-oriented/ Intelligence-led policing project
    4. Community policing intervention
    5. Broken windows/ Compstat approaches
    6. Civil injunctions/ civil remedy
    7. Police-led environmental improvement

    To establish a problem-oriented policing project we use the operational definition used by Weisburd et al. (2008) which is an intervention that adheres to the SARA process and "involve[s] the identification of a problem believed to be related to crime and/or disorder outcomes, the development and administration of a response specifically tailored to this problem and an assessment of the effects of the response on a crime or disorder outcome."
2. The evaluation used some quantitative measure of crime and/or disorder;
3. The article reported original research findings. Systematic reviews or other meta-analyses of prevention projects themselves will not be included, though articles which report on several case studies will be included. In cases where the same project is reported in two different publications (e.g. in a government report and in a journal article), only the manuscript with the most detailed information will be included;
4. The intervention was geographically focused to a local area. Here 'local' means a specifically defined area that is smaller than a city or a region. Examples include census blocks, police areas (e.g. zones, beats, divisions or precincts), housing estates, districts, suburbs, block areas, series of roads, neighbourhoods or hotspots. Hence, policing interventions that were implemented on a large scale or jurisdiction wide will not be included;
5. The study could have been conducted at any point in time (i.e. there is no time frame for inclusion);
6. The study could have been conducted in any location (i.e. there will be no geographic limitations for inclusion);
7. The study is written in English; and
8. The study has either been published or unpublished. Both will be included for review.

We believe that it is important to limit this study to local area interventions as at larger scales it is unlikely that research (other than randomized control trial designs) could justifiably attribute increases or decreases in crime elsewhere to the activity of a scheme.

3.2 Search strategy for identification of relevant studies

The retrieval of relevant studies will include various search strategies, as follows:

1. A keyword search of electronic abstract databases (see lists of keywords and databases below).
2. A review of bibliographies of existing displacement reviews (i.e. Barr and Pease, 1990; Eck, 1993; Hesseling, 1994; Guerette and Bowers, 2009) and reviews of the effectiveness of focused policing initiatives (e.g. Braga, 2007; Mazerolle et al., 2007 Weisburd et al., 2008).
3. Forward searches for works that have cited key displacement publications, to include the displacement reviews listed above as well as Bowers and Johnson (2003), Clarke (1994), Clarke and Weisburd (1994) and Weisburd et al. (2006).
4. A review of research reports of professional research and policing organizations (see list below).
5. A hand search of pertinent journals and publications. These were The Security Journal; Crime Prevention and Community Safety: An International Journal; Crime Prevention Studies; Crime-prevention reports from the Home Office and the Australian Institute of Criminology and Police Quarterly

All the works identified through these means will be reviewed in relation to the inclusion criteria described earlier. Once a shortlist of works meeting these criteria has been populated, this will be sent to leading policing scholars and those who have published or are otherwise knowledgeable in displacement. These scholars contacted will be those authoring any of the works in the shortlist. It is hoped that they will be able to highlight any works that may have been missed. The full text of the works shortlisted will be obtained from either (in order of preference):

1. Electronic copies at Florida International University (FIU) and University College London (UCL; as well as other electronic works accessible through other universities as part of a consortium, e.g. University of London Senate House Library).
2. Paper copies at Florida International University and University College London (as well as other electronic works accessible through other universities as part of a consortium, e.g. M25 consortium).
3. Electronic/paper copies requested through UCL's Inter Library Loan (ILL) system, which sources most materials from the British Library.
4. Electronic/paper copies requested from the authors themselves.

Should any of the full text versions of the works collated not contain all the information required in the coding form (described in a later section), authors will be contacted directly.

The search will be conducted at an international level and cover all years for which the resources are available. The following databases (and any more deemed to be appropriate at the time of research) will be searched for relevant studies:

1. Criminal Justice Periodicals
2. Criminal Justice Abstracts
3. Criminology: A SAGE Full Text Collection
4. National Criminal Justice Reference Services (NCJRS) Abstracts
5. HeinOnline
6. JSTOR
7. Sociological Abstracts
8. Social Sciences Full Text
9. Social Science Citation Index
10. PsycINFO
11. Dissertations and Theses
12. Electronic Theses Online Service (ETHOS)
13. Index to Theses
14. Australian Digital Theses Program
15. Government Publications Office, Monthly Catalog (GPO Monthly)
16. Australian Institute of Criminology – CINCH Database
17. National Improvement Policing Agency (NPIA)
18. National Police Library (UK based)
19. SCOPUS
20. IBSS (International Bibliography of Social Sciences)

We will also search the publications of the following groups:

1. Center for Problem-Oriented Policing (Tilley Award and Goldstein Award winners)
2. Institute for Law and Justice
4. Vera Institute for Justice (policing publications)
5. Rand Corporation (public safety publications)
6. Police Foundation
7. Police Executive Research Forum (PERF)
8. The Campbell Collaboration reviews and protocols (C2)

Publications from national policing agencies' will also be searched and contacted if needed. These include but are not limited to:

1. Home Office (United Kingdom)
2. Australian Institute of Criminology
3. Swedish Police Service
4. Norwegian Ministry of Justice and the Police

5. Royal Canadian Mounted Police
6. Finnish Police (Polsi)
7. Danish National Police (Politi)
8. The Netherlands Police (Politie)
9. New Zealand Police

Searches of electronic databases will use the following Boolean search terms:

(displac* OR "diffusion of benefit" OR "diffusion of benefits" OR "multiplier effect" OR "free side benefit" OR " halo effect" OR "spill over*" OR "free rider effect" OR "bonus effect" OR "spill-over")

AND

(police OR policing OR law enforcement)

AND

("hot spot policing" OR 'hot spots policing" OR crackdown* OR "problem oriented policing" OR "problem solving" OR "focused policing" OR "targeted policing" OR "directed patrol" OR "enforcement swamping" OR "intelligence led policing" OR "broken windows" OR "compstat" OR "community policing")

AND

(evaluat* OR impact OR assessment OR test)

3.3 Description of methods used in the component studies

It is expected that studies collected will differ in their methodological approach. Some will have simple post assessments, some pre and post assessments, and some pre and post with at least one comparison[10] area. Fewer will have established some equivalency between comparison and treatment areas or will have used random assignment to minimize bias. Also, expected to be in the minority will be studies which use a buffer[11] area nearby or around the intervention zone to measure displacement or diffusion, or include a buffer for the control area. Because of this, the type of synthesis possible will vary according to research design. While those with designs which limit confident determinations of displacement or diffusion effects will be examined prima fascia for their presence, they will not be the focus of this review.

To account for the varying levels of methodological rigor, studies will be grouped according to a hierarchy of evidence – which reflects the extent to which causal inferences will be sensible - and analyzed separately (see Table 1). Estimates of effect size (ES) will be computed within groups (e.g. RCT versus non-RCT) and comparisons made between them. The statistical element of the quantitative review will focus on studies which at least meet the

[10] Comparison area and control area are used interchangeably here
[11] Buffer area and catchment area are used interchangeably here

following conditions: i) they presented raw crime counts or rates of crime, or reported a standard measure of effect size and sampling variance that is suitable for inclusion in meta analysis; and ii) the research design used in the evaluation comprised at least three areas: a treatment, catchment (or buffer), and a control. These may use random or non-random assignment to conditions (treatment and control), though it is anticipated that very few will have used randomization in the design. A possible framework for categorizing studies according to their methodological adequacy is as follows:

Table 1: Matrix of Hierarchy of Evidence (RCT – randomized control trials)

Design	Action area	Control area	Action buffer	Control buffer	Data and effect size considerations	Standard
RCT	X	X	X	X	More likely to have multiple action area and use regression/ time series designs	Highest
RCT	X	X	X			
Quasi-Experimental	X	X	X	X	More likely to use group difference calculations (using changes in average counts per week or month within each of the areas). Most basic designs likely to report descriptive statistics only.	
Quasi-Experimental	X	X	X			
Quasi-Experimental	X		X			Lowest

This classification assumes that there is both pre and post data for all areas but it could be extended if many studies do not include both. Note that data for pre-post studies without a control group but with quantitative information on action and buffer areas will be collected as part of this review. The findings from these will be summarized as supporting evidence to – but not included in - the main meta-analysis.

It is expected that a range of different data and methods will be used across the different primary studies. Some of the issues it is anticipated that the review will need to deal with will be the use of multiple action, control and or catchment areas within one primary study; the availability of time series data; the use of crime counts and/or crime incident rates; the use of different methods for calculating effect sizes; the computation of effect sizes for different crime

types and differences in the handling of intervention effect sizes and catchment area effect sizes within studies. These are characterized extensively in the coding section below.

One point of clarification that should be made at this point is that the 'unit of analysis' is likely to vary across the various studies. Hence, where the authors report an effect size, this might be based on a repeated measures analysis of many different treatment/control/catchment areas. Alternatively, it might be based on the mean change in levels of crime for a single treatment/control/catchment per unit time. It is envisaged that the most common form of reporting across studies will be simple counts for a single action area, buffer and possibly control area pre- and post- intervention. Here, effect sizes are rarely reported by the study authors and instead descriptive statistics are commonly relied upon to examine observed changes.

Hence, from the authors' knowledge of the studies it is anticipated that a common approach to the reporting of findings will be the presentation of descriptive counts of crime pre- and post- test for (at least) one action area, one control area and one catchment area. Previous meta-analyses that have summarized data of this type have used the odds-ratio approach to estimating mean effect sizes (e.g. Farrington and Welsh 2002). In the context of the current study, odds-ratios would be calculated for both the treatment area and the buffer area leading to estimates of observed changes in both areas; that is, scheme success and potential spin-offs that may have been caused by intervention. This approach to analysis essentially treats the crime (rather than the area) as the unit of analysis. Whilst acknowledging the limitations of this approach it likely to be the most logical way of consistently summarizing the data (see section 3.6 for more detail on this).

3.4 Criteria for determination of independent findings

It is anticipated that in some cases it will be possible to produce more than one effect size (odds ratio) for a single study. This may be because the study had more than one action area for which data is provided, or calculations will have been made for different crime types in the same area(s). It is also possible that some studies will have used more than one buffer or comparison area. Where there is dependency of this kind a problem arises when one wishes to estimate a mean effect size. The reason for this is that if all estimates of effect size are included, then those studies for which more than one effect size can be computed will contribute more than one estimate, meaning that inferences are likely to be biased. An alternative approach would be to include only one effect size for each study, but the obvious problem with this is that it would be hard to derive a universal rule that justified the selection of one effect size over all others. A still further approach – and the one adopted here – is to compute all possible effect sizes for each study, and to compute a mean effect size across studies by selecting one of these. This process will be repeated so that every effect size is used in the computation of the mean effect size and, hence all possible permutations will be calculated. In so doing, a distribution for the mean effect size will be generated enabling the mean "mean effect" to be calculated along with the upper and lower confidence intervals for the distribution. To provide a concrete example, let's say that we produce 10 different effect size observations (odds ratios) across a series of studies. Let's also say that five of the observations were generated using variants of the same data (e.g. the same target area was used but different control areas were examined). In this case, combining all the ten effect sizes into an overall mean would be inappropriate for the reasons discussed. The

approach suggested is that we would compute a mean effect size using the data for the five observations for which there was no dependency in the data and plus *one* observation from the five others where dependency was an issue. In this simple example, by computing the mean effect five times, using a different observation for the study with multiple measures each time, we would produce five estimates of the mean effect size. For each estimate of the mean effect size, there would be no dependency in the data. This way, all of the data are used and a distribution of mean effect sizes is generated. This mean effect size distribution can then be inspected to draw overall conclusions. Of course, this example is a simple one but the approach could be applied – and would be most valuable – if there are many studies for which there is dependency in the data.

Due to the very nature of the phenomena of interest there will be a different type of dependency caused by the physical propinquity of the areas involved in the analysis. Indeed, this is one of the underpinning elements of displacement theory- changes that occur in one area (such as crime control effort) will affect levels of crime in those nearby. This has led most researchers to carefully consider the potential issue of "contamination" (e.g. Weisburd and Green 1995) when selecting areas to be used in the evaluation of the impact of those changes. Hence, it is crucial to pick (a) control area(s) that is likely to be free of contamination from diffusion or displacement and likewise to pick a sensible area for where contamination might occur. Where possible (and if necessary), designs that fail to do this will be noted and excluded from the analysis.

3.5 Details of study coding categories

Each of the retrieved studies will be inspected independently by two reviewers to determine whether i) spatial displacement and diffusion were analyzed (as opposed to temporal, target, tactical, offense, or perpetrator) and ii) whether any displacement or diffusion was observable or reported by the author(s). In some instances there may be empirical evidence consistent with displacement and diffusion effects, yet it may not be noted by the study author. The findings from each reviewer will be compared and any differences in the coding will be discussed by the two reviewers until a consensus is reached. In the event agreement cannot be made a third reviewer will review the study and make a final decision.

The eligible studies will be coded on the following criteria:

- Study identifiers (title, author, year, publication type)
- Location of intervention (Country, Region, State, City)
- Size of intervention, control and buffer areas (e.g. km^2, number of residents, number of households)
- Research design (randomized experiment, pre-post w/buffer and comparison, etc.)
- Nature (type) of focused policing intervention. This will be divided into the categories mentioned in the criteria section above and useful distinctions will be made where appropriate. For example, one variable might be used to indicate whether the intervention involved law enforcement or not.
- Crime type targeted
- Length of pre-assessment, intervention and post assessment (i.e. follow up period)
- Unit of analysis/ sample size. This will depend on the study design (see below)

- Pre and post outcome measure statistics
    - In intervention area(s)
    - In buffer area(s)
    - In comparison area(s)
- Statistical test(s) employed. This will depend on the study design (see below)
- Effect size (where applicable; see below)
- Reported intervention, displacement and diffusion effects

As highlighted above and in earlier sections, the data gathering process for this systematic review is likely to be complex. We propose to collect the information in the greatest level of detail possible at the coding stage and subsequently make decisions about how to aggregate or summarize the data, as necessary. Some detailed procedures used in the coding process will include:

1. Recording multiple observations for each study.
   Some studies will involve a series of action, control and buffer areas (e.g. Braga and Bond 2008 have figures for 17 individual hotspots). Here we will collect information for individual combinations of these where possible. Statistics reported in such studies are likely to report effect sizes using regression or correlation coefficients.

   Other studies might report findings for a series of different types of crime and/or different types of data (e.g. Calls For Service data or Recorded Crime Data; see Madensen&Morgan2005 as an example). Here information will be captured on each of the different types with individual effect sizes collected as appropriate. A further type of study design with multiple observations will involve data on multiple time points. Here monthly count data might have been reported for the areas, for example. This opens up the possibility for time series analysis (e.g. Roman et al 2005).

2. Recording different types of effect size
   A multitude of different options for calculating effect sizes exist. These include simple T-tests, F-tests, differences-in-difference calculations and odds ratios. Such group difference calculations are usually only reported where there is data on the mean and standard deviation of the count of crime per unit of time (week or month for example). Such calculations can be conducted for different combinations of areas. For example, one t-test might compare the mean monthly count before and after intervention for the action area; a further can do the same for the control area and a further again for the catchment area. Where ESs have been specified these will all be recorded, along with relevant sample sizes.

   Caution will need to be exercised in using ESs reported by authors. Since there is no consensus of opinion on their construction, decisions on which statistic to use are likely to rely heavily on the views of the author and inappropriate techniques could easily be used. Furthermore, in some cases authors might have reported statistics for the action area but just undertaken a descriptive analysis of the

displacement or diffusion effect. It is anticipated that in many cases purely descriptive statistics and percentage changes will be reported.

3. Differences in dependent variable constructs
   It is likely that in some cases counts will be reported and in other cases it will be rates. We envisage that the former approach will be more common than that latter. Hence whilst we will record all data in the coding process we will additionally aim to convert rates to counts where possible. It will be important to ensure that all counts are constructed for comparable time periods across the action, control and catchment areas.

In addition, a coding system which reflects the hierarchy of evidence will be used. This will rate the studies in terms of the standard of their research design (see section 3.3, Table 1).

3.6 Statistical procedures and conventions

The quantitative part of the review will contain four elements. The first will be a summary of the effect sizes (ESs) as reported by the individual study authors. Including these will ensure that ESs are reported at the highest possible level of resolution (there might be aggregation of data in the second or further elements of the review).

A second element of the quantitative review will use traditional meta-analytic techniques used in other systematic reviews. Here, effect sizes will most likely be computed using odds ratio calculations. There will be some challenges in applying these measures for this particular review. One of these is a consequence of the need to summarize changes in up to four independent areas types (see Table 1). For example, in one study we might find a positive effect of the intervention, but a displacement effect, whereas another study might show a significant intervention effect but a positive diffusion effect. The complexities of dealing with such combinations will be dealt with as appropriate. For example, one possibility is to compute ESs for the action and buffer areas separately and to then produce a scatter or forest plot to investigate the relationship between the two. A further possibility is to produce an odds ratio which takes into account displacement and/or diffusion as well as the intervention effect. This would require thought as to how to combine the effects- for example, is it reasonable to use an additive model- or is some scaling required?

Further challenges associated with the use of an odds-ratio meta-analytic approach where the unit of analysis is an area rather than a person include;

- The level of heterogeneity of the sample (both in terms of outcome measures used and the contexts of the schemes) may make an average measure of effect inappropriate. It is likely that some studies will use crime rates before and after action and others will use total numbers of crime. This means that the quantitative scales of the analysis in the different studies would not be directly comparable. Pooling them would therefore not be meaningful. To address this we propose using counts throughout and whether possible,

for studies that report rates, to convert the figures back to counts. Furthermore, the types of crime outcomes used will likely vary as will the types of focused policing intervention employed and the spatial and temporal contexts in which they are carried out. A random effects model for computing the estimates of mean effects should assist in combating some of these issues. We will also produce separate tables to summarize effect sizes for meaningful subsets of the data.

- Assessing the significance of odds ratios requires that standard errors are known for individual schemes but because the denominator (the ultimate sizes of the action and control groups) is often unknown these may be problematic to compute. Unlike an offender rehabilitation scheme for example, where an action and control group of known sizes are monitored to see how many in each group re-offend, with geographically focused policing interventions outcome analysis involves consideration of the number of crimes in an area as a whole- not to a series of individuals. Farrington and Welsh propose a solution to this problem by using the number of crimes as the unit of analysis and thus to produce an estimate of the standard error for each scheme. Due to the parametric assumptions associated with this approach – that crime counts can be described by a Poisson process - this method has subsequently been criticized as under-estimating over-dispersion in the data (and confusing the unit of analysis) and consequently the standard error of the odds ratio. Consequently, Farrington and Welsh propose the use of an over-dispersion factor to control for this but the discussion continues and no universally accepted solution has been found to date. We will therefore use the guidance in e.g. Farrington and Welsh (2004) to use an over-dispersion factor of 2.

An alternative approach that may be used to estimate the confidence intervals of the odds ratio is the *bootstrap* (e.g. Moore &McCabe, 2006). This is a non-parametric approach which is commonly used in statistics where the sampling distribution of interest is unknown or where the assumptions necessary for parametric inference are likely to be violated (see, Mooney and Duval, 1993). The calculation of bootstrap confidence intervals requires intense computation, but with increases in computing power, confidence intervals can be computed in this way even on a standard laptop. There are a number of advantages to using this method, not least because compared to other approaches fewer assumptions are required to be met for it to be appropriate. This method will be used as well as that discussed above.

In order to conduct this odds-ratio based meta-analytic element of the review decisions will need to be made concerning schemes that provide detailed data for multiple areas, crime types and time points. Where deemed appropriate we will use the procedure detailed in section 3.4 above to cope with dependency and include sub-divisions of the data for a single primary study. In other cases, where comparability across studies will be enhanced by aggregating the data this will be done. An obvious drawback with this approach is the loss of some of the variation in the data. This is why the review will triangulate by reporting the author's effect sizes as well.

To assist with validation and triangulation, the third element of the quantitative review will include the computation of the gross effect (GE), net effect (NE), the total net effect (TNE) and the weighted displacement quotient (WDQ), and its constituent parts which were developed by

Bowers and Johnson (2003) and extended by Eck and Johnson (see Clarke and Eck 2005). The WDQ is a relative ratio and not subject to some of the limitations mentioned above. The gross effect (GE) and the net effect (NE) are defined as

$$GE = R_b - R_a \quad (1)$$

where R_a is the crime count in the treatment area post intervention, and R_b is the crime count in the treatment area before the intervention.

$$NE = (R_b/C_b) - (R_a/C_a) \quad (2)$$

where C_a is the crime count in the comparison area post intervention, C_b is the crime count in the comparison area before the intervention. The weighted distribution quotient or WDQ, was used to determine displacement or diffusion effects and is designated as

$$WDQ = \frac{D_a/C_a - D_b/C_b}{R_a/C_a - R_b/C_b} \quad (3)$$

where D_a is the crime count in the buffer area post intervention, D_b is the crime count in the buffer area before the intervention. The WDQ can also be broken down into separate measures of scheme success and scheme displacement/ diffusion, like so:

Success Measure (WDQ denominator) = $R_a/C_a - R_b/C_b$

Buffer Displacement Measure (WDQ numerator) = $D_a/C_a - D_b/C_b$

Additionally, the overall impact of the project was determined using the TNE or "total net effects" model which is defined by the relationship

$$TNE = [R_b(C_a/C_b) - R_a] + [D_b(C_a/C_b) - D_a] \quad (4)$$

The advantages of using the WDQ and the TNE are three fold. First, the WDQ allows for measurement of the relative size of displacement or diffusion of benefits. Second, the TNE allows for overall determinations of treatment effects while accounting for the size of any displacement or diffusion effects. Third, the WDQ and the TNE can be used where there are short post evaluation periods which are most common in SCP evaluations (Clarke, 2005; Guerette, 2009). This is also likely to hold true of the interventions examined here. Details on the use and interpretation of the GE, NE, WDQ, TNE and Success and Buffer Displacement measures are provided in Appendix A.

One current limitation of the WDQ and TNE, however, is that they are descriptive statistics. Thus, in their current form it is not possible to determine the likelihood that any observed changes are in fact real or are more likely due to chance (e.g. whether the change is statistically significant). Thus, a further methodological advance to be explored in this review

would be the use of a bootstrap method to estimate the statistical significance of the WDQ. To do this, for each test, a bootstrap would be used to compute confidence intervals for the WDQ, allowing statistical inferences to be made.

To supplement the Odds Ratio and WDQ elements of the review, a further analysis will simply compare treatment and control catchment areas. This is only possible when there is a randomized experiment and measurements from catchment areas around both the treatment and control sites and hence the analysis will be limited to this subset of studies. This analysis will mirror that done by Weisburd and Green, 1995b; Braga et al., 1999 and Braga and Bond, 2008.

For completeness, a final element of the quantitative review will involve a proportional change analysis. This will be undertaken for all of those studies for which there is count data for the action area and the catchment area pre- and post- implementation (this includes all study designs shown in Table 1). This will simply summarize percentage changes in the counts for the action and buffer zones- controlling for any differences in length of the time period covered before and after implementation. This will enable the most simple designs to be incorporated into one aspect of the quantitative analysis. This approach is similar to that used in Weisburd et al (2008).

3.7 Treatment of qualitative research

Qualitative studies will not be included in this review.

4. Timeframe

The review process will adhere to the following benchmarks and anticipated dates:

Search for published and unpublished studies	October - December 2009
Relevance assessments	November - December 2009
Coding of eligible studies	December - January 2010
Statistical analysis	January - February 2010
Presentation of statistical results	March 22-23, 2010
Preparation of written report	March - April 2010
Submission of completed report	April 30, 2010

5. Plans for Updating the Review

Once completed, the authors expect to update the review every five (5) years.

6. Statement Concerning Conflicts of Interest

Kate Bowers and Rob Guerette are the study authors of the most recent systematic review of crime displacement and diffusion of benefits among situational crime prevention interventions. Kate Bowers and Shane Johnson developed (with later elaboration by Eck and Johnson)some of the statistical procedures proposed to be used in this review (see section 3.6). Bowers, Johnson, and Guerette have all published in the areas of problem-oriented policing, situational crime prevention, and environmental criminology. Guerette is affiliated with the Center for Problem-Oriented Policing and all (Bowers, Johnson, and Guerette) have written series guides commissioned by that organization. Guerette also serves as the advisor and coordinator for the annual Herman Goldstein Awards for Excellence in Problem-Oriented Policing.

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8. Appendix A. Use and Interpretation of Coefficients to be used in Analytical Findings

Coefficient	Use	Interpretation
Gross Effect (GE)	Determines increase or decrease in treatment area.	Positive number > 0) indicates decrease in crime; Negative number < 0) indicates increase in crime. Zero = 0) means there was no change.
Net Effect (NE)	Determines increase or decrease in treatment area in relation to changes in control area.	Positive number > 0) indicates decrease in crime; Negative number < 0) indicates increase in crime. Zero = 0) means there was no change.
Weighted Displacement Quotient (WDQ)	Determines the extent of displacement or diffusion in buffer areas in relation to changes in treatment and control area.	Positive number > 0) indicates there was a diffusion effect and any treatment effects were amplified; If number is greater than positive one > + 1.00) then the diffusion effect was greater than the treatment effect. Negative number < 0) indicates there was displacement. A negative number between zero and negative one < 0 > -1.00) means that the displacement was not greater than the treatment effects and the intervention still achieved some benefit. A negative number beyond negative one < -1.00) means the treatment effect was eclipsed or erased by displacement. Zero = 0) means there was no effect.
Success Measure	Determines the degree to which the decrease in the action area outweighs that in the control area (i.e. the degree to which the scheme was successful).	Negative number < 0) indicates successful schemes where the decrease in the action area outweighed that in the control area. Positive number > 0) indicates schemes where the treatment was not effective.
Buffer Displacement Measure	Determines whether the interventions show possible evidence of displacement or diffusion.	Positive number > 0) indicates a possible displacement effect. Negative number <0) indicates a possible diffusion of benefit.
Total Net Effect (TNE)	Determines the overall effect of the intervention in relation to changes in the control area while adjusting for displacement and/or diffusion effects.	Positive number > 0) indicates intervention was effective overall; Negative number < 0) indicates that it was not. Zero = 0) means there was no change. The greater the number, either positive or negative, the more or less effective the intervention, respectively.