Spatial displacement and diffusion of benefits among geographically focused policing initiatives: a meta-analytical review

**Kate J. Bowers · Shane D. Johnson ·
Rob T. Guerette · Lucia Summers ·
Suzanne Poynton**

Abstract

Objectives To undertake a systematic review of the extent to which geographically focused policing initiatives appear to displace crime (simply relocate it to other places) or diffuse benefits (lead to reductions elsewhere).

Methods Evaluation research which assessed whether such schemes may have led to displacement or diffusion of benefit to nearby areas was identified, obtained and coded. Research reports were assessed in terms of their methodological rigor and the quantitative estimates of outcomes collected. A hierarchy for estimates of displacement was established. For 16 studies, meta-analysis was used to produce a collection of results that had two sets of outcome information in terms of effect sizes (the success of the intervention and the extent to which it may have caused displacement or diffusion).

Results The main findings of the meta-analysis suggested that on average geographically focused policing initiatives for which data were available were (1) associated with significant reductions in crime and disorder, and (2) that, overall, changes in catchment areas were non-significant but there was a trend in favor of a diffusion of benefit.

Conclusion The results demonstrate that in the case of geographically focused police efforts, displacement is far from inevitable, and in fact, on balance, that the opposite, a diffusion of crime control benefits, appears to be the more likely outcome. This in turn demonstrates that such endeavors on average have an overall reductive impact on crime.

Keywords Diffusion of benefits · Displacement · Evaluation · Focused policing initiatives · Meta-analysis

K. J. Bowers (✉) · S. D. Johnson · L. Summers · S. Poynton
Department of Security and Crime Science, University College London, London, UK
e-mail: k.bowers@ucl.ac.uk

R. T. Guerette
School of International and Public Affairs, Florida International University, Miami, FL, USA

Introduction

One of the most common criticisms of focused policing efforts is that crimes prevented will simply be relocated to other places since the "root causes" of crime were not addressed and 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. Our purpose in this article is not to review the theoretical literature on this topic but to synthesize empirical evidence on its extent. Hence, the explanation of the concept which follows is abridged and relies heavily on an earlier paper by two of the co-authors (Guerette and Bowers 2009). Interested readers are referred to this paper which was used directly in setting up the rationale for the current study.

Spatial displacement (where offenders switch from targets in one location to those in another)—as opposed to temporal, target tactical, offense or perpetrator—is by far the form most commonly recognized (Eck 1993). 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. At the other end of the displacement continuum is the phenomenon of diffusion of crime control benefits (Clarke and Weisburd 1994). Crime diffusion is the reverse of displacement, and its occurrence has been documented in several crime prevention evaluations (Bowers and Johnson 2003; Chaiken et al. 1974; Green 1995; Miethe 1991; Weisburd et al. 2006; Weisburd and Green 1995b). Diffusion of benefit occurs when the positive effects of an intervention are believed to extend to nearby areas that are not in receipt of any form of action (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 some degree of diffusion is observed, the treatment effect is amplified since improvements are gained without expending resources in the areas to which benefits were diffused. While there have been some noted experiments on the extent of displacement and diffusion, a systematic appraisal of all the available evidence on this topic is currently unavailable.

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 as a consequence of focused police activity if deterministic theories of crime are correct.

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.

The rational choice perspective also provides an explanation for 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 provides an explanation as to why diffusion may have been observed in places near treatment areas in some studies.

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 a treatment 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; Hesseling 1994), and there had not been any published systematic reviews of diffusion of benefits (Weisburd et al. 2006). 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% found no or little displacement (e.g., displacement less than the treatment gain) and only three (9%) reported a substantial amount. Similarly, Hesseling (1994) found that 40% 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, sometimes, 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 16 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 the reviews were descriptive in their method. This was mostly due to the

lack of data provided by individual study authors. 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 were reported, available statistical methods allowing for reliable empirical estimates of the extent of displacement (e.g., estimates 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 reported in 26%. The opposite of displacement, diffusion of benefit, was reported in 27% of the observations. However, for the majority of these studies, insufficient data were available to estimate the extent of displacement or diffusion of benefit using a quantitative methodology. For the 13 studies which allowed such an assessment, analyses suggested that when spatial displacement did occur it tended to be less than the treatment effect, suggesting that the intervention was still beneficial. Moreover, a diffusion of benefit appeared to be the more likely outcome. That study, however, focused exclusively on situational crime prevention initiatives and did not assess the extent of displacement and diffusion among focused policing interventions.

For clarity, the distinction between these two types of crime control strategies is as follows. Situational crime prevention (SCP) measures are those that focus on reducing opportunities for crime through alteration of the host environment. This broadly entails any technique that serves to increase the risk and/or effort associated with committing a given crime as well as ways of reducing the rewards, excuses, and provocations for offending. Some examples include target hardening (e.g.. improving locks or installing a burglar alarm), improving surveillance (e,g.. CCTV or Neighborhood Watch schemes), increasing awareness (e.g., publicity or mass media campaigns), and controlling access (e.g., street closures, identity permitted access, barricades).

In turn, geographically focused policing interventions, which we examine here, center on the strategic use of police officers at known crime locations, usually resulting in highly visible and highly active officers targeting specific crimes and/or offenders. The implementation of focused policing initiatives can be guided by a variety of policing models, such as problem-oriented policing, broken windows, intelligence led, hot-spots policing, or a more traditional police crackdown. This review uses the following definitions to categorize different types of focused policing interventions:

- Hotspot policing/ directed patrol: where certain places are intensively policed with, for example, high visibility foot patrols
- Police crackdown: where the police crackdown on a particular crime problem (e.g., through drug enforcement) or on a particular set of offenders (e.g., a certain group of juvenile offenders)

- Community policing intervention: these approaches attempt to ensure that the same officer is assigned to a specific geographical area on a permanent basis so that they can become familiar with the local issues
- Broken windows/Compstat: these include assertive enforcement of minor offenses and order maintenance approaches
- Civil injunctions/civil remedy: these approaches use civil law to disrupt or disperse certain groups or behaviors
- Police-led environmental improvement: where the police are the driving force in encouraging changes to the environment to discourage crime (e.g., physical redesign of areas or security surveys resulting in recommendations)
- Problem-oriented/Intelligence-led policing project: 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.

The ultimate purpose of the systematic review reported here was to determine the empirical extent of *geographical* displacement and diffusion of benefits among such focused policing interventions. In doing so, it seeks to more completely understand the prevalence and nature of geographical displacement and diffusion. Thus, we ask: To what extent does displacement and diffusion occur in the aftermath of focused policing efforts? Does it vary by the scale of the treatment? Does it vary by the type of focused policing effort employed?

Methods

This review was guided by procedures used in previous Campbell reviews of the effectiveness of problem-oriented policing (Weisburd et al. 2008, 2010) 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).

Criteria for inclusion and exclusion of studies in review

To be included in the review the following conditions had to be satisfied:

1. The study must have evaluated a focused policing intervention included in the categories described above
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 were not included, though articles which reported on several case studies were included. In cases where the same project was reported in two different publications (e.g., in a government

report and in a journal article), only the manuscript with the most detailed information was included

4. The intervention was geographically focused to a *local* area; specifically, an area that is smaller than a city or a region. Examples included census blocks, police areas (e.g., zones, beats, divisions or precincts), housing estates, districts, suburbs, block areas, a series of roads, neighborhoods or hotspots. Hence, policing interventions that were implemented on a large-scale or jurisdiction-wide were not included

5. The study could have been conducted at any point in time (i.e., there was no time frame for inclusion)

6. The study could have been conducted in any location (i.e., there were no geographic limitations for inclusion)

7. The study was either published or unpublished; both were included for review

Search strategy for identification of relevant studies

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

1. A keyword search of electronic abstract databases

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 and Weisburd (1994) and Weisburd et al. (2006)

4. A review of research reports of professional research and policing organizations

5. A hand search of pertinent journals and publications

The databases searched and keywords used can be found in Appendices 1 and 2, respectively. These searches were carried out between December 2009 and January 2010. Therefore, this review only covers manuscripts that were published (or made available) up until this date. Each manuscript was checked in relation to the inclusion/exclusion criteria. A list of those manuscripts meeting the criteria was compiled and sent to leading policing scholars in the field for comment, as the sixth and final stage of the literature search. These scholars were defined as those particularly knowledgeable in displacement and diffusion studies and/or POP and hot-spots policing.

Details of study coding categories

Each of the retrieved studies were inspected independently by two reviewers to determine whether (1) spatial displacement and diffusion were analyzed (as opposed to temporal, target, tactical, offense, or perpetrator) and (2) whether any displacement or diffusion was observable or reported by the author(s). In some instances, there may have been empirical evidence consistent with displacement and diffusion effects, yet it may not have been noted by the study author.

The eligible studies were coded on the following criteria:

- Study identifiers (title, author, year, publication type)
- Location of intervention (Country, Region, State, City)
- Size of intervention, control and catchment areas (e.g., km^2, number of residents, number of households)
- Research design (randomized control trial, pre-post w/catchment and control, etc.)
- Nature (type) of focused policing intervention. This was divided into the categories mentioned in the criteria section above.
- Crime type targeted
- Length of pre-assessment, intervention and follow up period
- Unit of analysis/ sample size. This depended on the study design. For example, some evaluations considered changes in only one treatment, catchment (for a definition, see below) and control area whereas others examined changes in many.
- Pre and post outcome measure statistics

- In intervention area(s)
- In catchment area(s)
- In control area(s)

- Measures of effect size and inferential statistical tests employed. The types of test used varied according to the study design employed (see above). For example, some studies employed time-series analyses, others used difference in difference statistics, others reported F tests, while others reported descriptive statistics alone.
- Effect sizes for the treatment area and the catchment area(s)

Since the coding exercise involved collecting information for possible displacement and diffusion as well as for intervention effectiveness, the data gathering process for this systematic review was quite complicated. We therefore collected the information in the greatest level of detail possible at the coding stage and subsequently made decisions about how to aggregate or summarize the data, as necessary.

Summary of the statistical approach taken

Previous meta-analyses of place-based interventions (e.g., Farrington and Welsh 2002; Weisburd et al. 2008) that have analyzed data of a similar type have estimated mean effect sizes and associated confidence intervals for each study using odds ratios calculations (see Lipsey and Wilson 2001: pp. 52–54)

In the current study, we were primarily interested in changes observed in *catchment areas*. These are the areas which are identified as those to which crime potential displaces or crime control benefit diffuses (e.g., Weisburd and Green 1995a). Catchment areas are very frequently, but not always, a 'ring' or donut-shaped region which directly surrounds the area of intervention. Here, we are interested in whether crime generally increased or decreased in these areas following intervention more than would be expected given the changes observed in the control areas. Of course, to put these changes into context, it is also necessary to examine the changes observed in the treatment areas themselves. Consequently, for each study odds ratios were calculated for both the treatment *and* catchment areas. In addition to computing individual

estimates of effect size for each study for each type of area (treatment and catchment), mean effect sizes were also calculated across studies so that general inferences could be made. All analyses and graphs were generated in the R statistical programming language using scripts developed by the authors.

The approach taken to estimate the confidence intervals for the odds ratios is the same as that adopted in previous meta-analyses of place-based interventions, but it is important to note that debate exists as to the adequacy of this method (Marchant 2004, 2005). One concern is the extent to which the parametric assumptions on which the approach is based are reasonable (see Farrington et al. 2007). For instance, one assumption is that the data generating process is a Poisson one. This may be a reasonable assumption for studies for which the unit of analysis is a person, but is probably unreasonable for those in which the unit of analysis is a place (Marchant 2005; Farrington et al. 2007; Johnson 2009). The consequence of this is that the standard error derived using the standard equation is likely to underestimate the actual variance, meaning that the estimated confidence intervals will be too small. For this reason, we adopted the approach used elsewhere (Farrington et al. 2007; Weisburd et al. 2008) of multiplying the standard error by an inflation factor (in this case two, see Farrington et al. 2007) when calculating confidence intervals. Doing so leads to larger confidence intervals and a more conservative test.

Also, it is worth noting that the measure of effect size is not an odds ratio in the traditional sense. The equations for the odds ratio effect size and the associated standard error are:

$$ ES_{OR} = \frac{ad}{bc} \qquad SE_{LOR} = \sqrt{\frac{1}{a} + \frac{1}{b} + \frac{1}{c} + \frac{1}{d}} $$

Where a, b, c and d are crime counts as follows:

	Pre-intervention	Post-intervention
Action Area	a	b
Control Area	c	d

Here, the units of analysis are crimes not people. Given this departure from the traditional definition and approach, Farrington et al. (2007) have recently referred to the test statistic as a measure of relative effect size when evaluating place-based interventions rather than an odds ratio. As the distinction may be seen as largely semantic, we use the term odds ratio here but acknowledge the issue. Moreover, whilst acknowledging the limitations of this approach, we adopt it here as it was the most logical way of consistently summarizing the available data.

Dealing with dependency in the data

For a number of studies there were multiple observations for the same treatment area(s). For example, in some cases, data were available for the periods pre- and post-intervention for the treatment, catchment and control areas for more than one type of crime. In other cases, data were available for more than one catchment area. While this is unproblematic where effect sizes for each observation are considered

independently, it is a problem where mean effect sizes are calculated by combining estimates. That is, where there *are* multiple observations, if all the data were included in the calculation of a mean effect size this would lead to dependency in the data and violate an assumption of the approach. On the other hand, to exclude observations would be to lose useful data and would require an unbiased approach to observation selection. One approach that represents a compromise is to calculate estimates of mean effect size using those observations that reflect the best- and worst-case scenarios (see Weisburd et al. 2008).

However, as this approach uses only two possible permutations of the data, conclusions based on such analysis may be over-sensitive to outliers. An alternative, and one that is adopted here, is to compute the mean effect size for every possible permutation of the available data. This provides the opportunity to examine the distribution of the mean effect size across permutations.

Results

Selection of eligible studies

Figure 1 summarizes the results of the systematic search. The electronic database search identified over 2,500 studies. The titles and abstracts of these studies were then reviewed and any studies that were obviously not evaluations of focused policing interventions, were not written in English, obviously lacked a quantitative measure of crime and disorder or were book reviews were then removed. Articles reporting on systematic reviews or meta-analyses were also excluded from the short list at this point. This left a total of 103 studies. The full text of these 103 studies was then reviewed to determine whether they met all the relevant selection criteria. Ultimately, 38 met the criteria, and the remaining 65 did not. As Figure 1 demonstrates, a further 41 studies meeting the criteria were identified from the other elements of the systematic search.

Of the 79 studies that met the criteria, in 27 cases the findings reported duplicated those reported elsewhere, and hence these studies were removed. In a further 8 cases, the study met the criteria but the raw figures could not be sourced (e.g., figures were no longer available or the author untraceable). The final 44 studies, that are used and described in what follows, are listed in full in the eligible study reference list at the end of the paper (see References of Eligible Studies).

Characteristics of eligible studies

The 44 coded studies differed in methodological approach. To account for the varying levels of methodological rigor, studies were grouped according to a hierarchy of evidence (see Table 1). The majority (57%) had simple pre and post assessments, and the remainder pre and post- with at least one control area. A minority (9%) used random assignment to minimize allocation bias. Also in the minority were studies which used a separate catchment for the control area (14% of studies). Note that all the studies included in this review had separate pre and post counts for at least one displacement/diffusion catchment area.
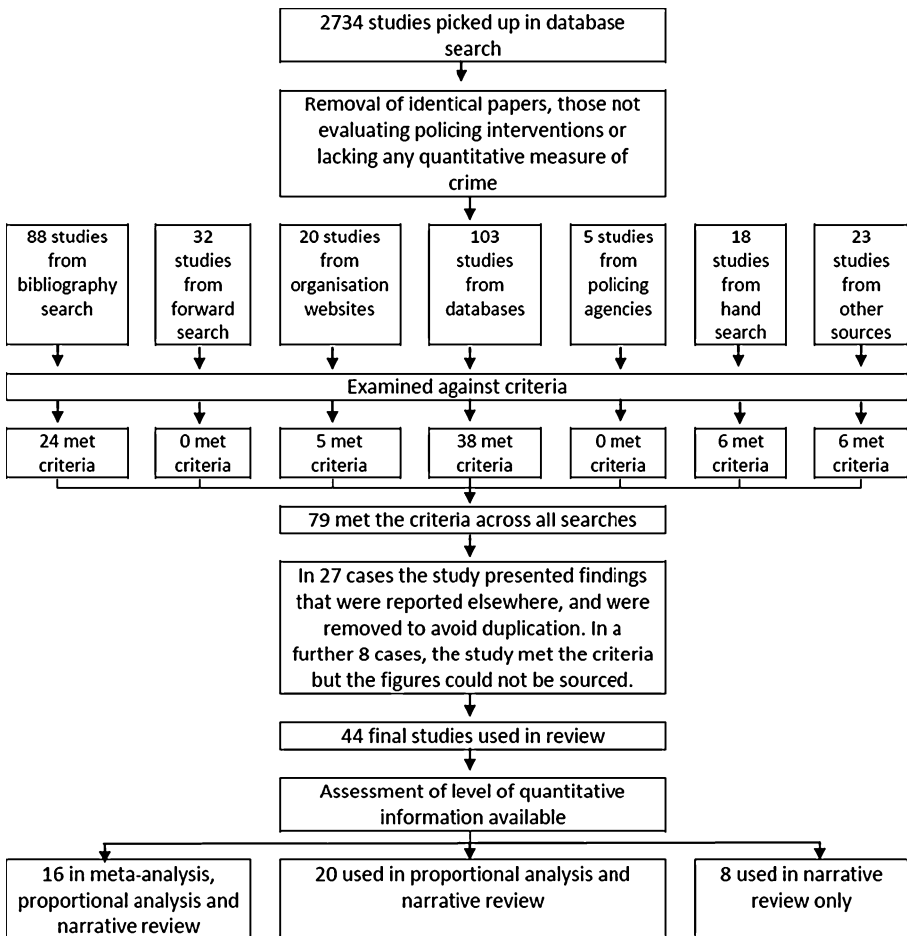
```
                    ┌─────────────────────────────────┐
                    │  2734 studies picked up in database │
                    │              search                 │
                    └─────────────────────────────────┘
                                    │
                                    ▼
                    ┌─────────────────────────────────┐
                    │ Removal of identical papers, those not │
                    │ evaluating policing interventions or │
                    │ lacking any quantitative measure of │
                    │              crime                  │
                    └─────────────────────────────────┘
```

88 studies from bibliography search	32 studies from forward search	20 studies from organisation websites	103 studies from databases	5 studies from policing agencies	18 studies from hand search	23 studies from other sources

Examined against criteria

24 met criteria	0 met criteria	5 met criteria	38 met criteria	0 met criteria	6 met criteria	6 met criteria

79 met the criteria across all searches

In 27 cases the study presented findings that were reported elsewhere, and were removed to avoid duplication. In a further 8 cases, the study met the criteria but the figures could not be sourced.

44 final studies used in review

Assessment of level of quantitative information available

16 in meta-analysis, proportional analysis and narrative review	20 used in proportional analysis and narrative review	8 used in narrative review only

Fig. 1 Systematic search results

As expected, a range of different data and methods were used across the different primary studies. In some cases, multiple treatment, control and or/catchment areas were used within one primary study and effect sizes calculated for a number of different combinations of these. Furthermore, some studies looked at effects for a number of different types of event data [e.g., calls for service (CFS), arrests and

Table 1 Hierarchy of evidence

Design	Treatment area	Control area	Treatment catchment	Control catchment	n (%)
RCT	X	X	X	X	4 (9%)
RCT	X	X	X		1 (2%)
Quasi-experimental	X	X	X	X	2 (5%)
Quasi-experimental	X	X	X		12 (27%)
Quasi-experimental	X		X		25 (57%)

recorded crime] and crime types. Finally, the methods used varied across studies: some used time series data, while others used crime counts and/or crime incident rates for aggregated periods of time pre- and post-intervention. A number of different methods were also used for calculating effect sizes and statistical significance.

Table 2 provides a summary of the basic aspects of the studies. Many of the studies reported interventions that had taken place in the US (68%) or UK (23%). A range of different environments were covered by the interventions. Many were purely residential environments (52%) or multiple types of environment (20%). Four studies were undertaken in mixed areas containing both residential and retail; two exclusively in retail areas. The most common type of data used was Recorded Crime (35 studies: 80%); CFS data were used in 7 studies (16%)[1]; arrest data was used in 4 cases; and in one study, data were generated using observation methods. The studies also varied in terms of the extent of physical area that was covered by the initiative; 24 studies (55%) covered 'large' areas, 9 (20%) medium, and 11 (25%) small.[2] Finally, the studies covered a range of different intervention types; most common were problem-orientated policing (27%), police crackdowns (23%), police patrols (16%), community-orientated policing (11%) and hotspots policing (9%).

Narrative review of displacement and diffusion from geographically focused policing initiatives

This section gives a brief overview of the findings reported by the original study authors. This is an unusual addition to a systematic review where the focus is traditionally on the findings of the meta-analysis. We include this summary as a consequence of the unusual nature of this particular review. In essence, we are attempting to summarize two effect sizes—that of the treatment itself and that of any consequent diffusion or displacement. Methods used to investigate these two effects vary substantially across studies, hence we feel it would be incomplete to proceed without passing comment on these variations.

In summarizing the findings, it is important to provide some sense of the extent to which authors reported spatial diffusion and/or displacement as a result of intervention. As has been demonstrated elsewhere (e.g., Petticrew and Roberts 2006), as a method of synthesizing evidence, simple vote counting has many dangers. However, with this warning in mind, it is valuable to provide an overview of the findings of all 44 studies, as only a subset of those (n=16), for which adequate data were available, are used in the main meta-analysis reported below. Hence, for descriptive purposes only, we include such a narrative summary here. This reveals

---

[1] Note that these figures relate to the total number of studies using this data source; some studies used more than one source of data.

[2] These sizes were inductively determined by noting the geographical scope of the intervention. Across the studies reviewed, size of intervention area information was conveyed in a number of different ways, such as population, physical extent, or administrative boundary. Once collected, relative categories of small, medium, and large geographical areas were partitioned. Small intervention areas ranged from one household to 5 blocks in size; Medium intervention areas ranged from the area of a single housing estate up to comparable areas equivalent to about three square miles; Large intervention areas were those that involved a scheme that covered any geographical area larger than three square miles. Note that because studies reported intervention area size in different ways, some subjective determinations were made by the authors.

Table 2 Descriptions of the eligible studies

Characteristic	Category	*n*
Size	Large	24
	Medium	9
	Small	11
Country	US	30
	UK	10
	Sweden	1
	Australia	3
Environment	Multiple	9
	Various	4
	Retail	2
	Educational	1
	Recreational	1
	Residential	23
	Residential and retail	4
Intervention type	POP	12
	Patrols	7
	Hotspots policing	4
	Police crackdown	10
	Intelligence-led policing	2
	Community-orientated policing	5
	Broken windows	2
	Civil abatement	1
	Police-led environmental	1
Data type	Recorded crime (RC)	33
	Calls for service (CFS)	5
	RC and CFS	1
	CFS/arrests	1
	RC/arrests	1
	Arrests	2
	Primary observations	1

differences between study authors' findings with respect to the likelihood of them reporting that they observed displacement or a diffusion of benefit. We find that 55% of the studies reported finding no spatial displacement, compared to 39% that did. For diffusion of benefit, this was reported in 43% of studies and not in 5%. For the remaining 7% (displacement) and 52% (diffusion) of studies, respectively, outcomes are unknown mainly due to the fact that they were not explicitly examined by the study authors. This indicates that there remains a bias in the literature whereby evaluators appear more likely to look for displacement rather than diffusion of benefit; something that would be overlooked in the absence of a narrative review of this kind.

In terms of method, it is apparent that displacement and diffusion are explored using many different designs and statistical methods. There is no standard (or even particularly frequently used) method of hypothesis testing. The most common type of design used involved the comparison of group differences using Analysis of Variance or *t*-tests. The authors who have used *t*-tests (Smith 2001; Weisburd et al. 2006; Wagers 2007; Segrave and Collins 2005; Sherman and Rogan 1995) tend to calculate statistics for treatment, control and catchment areas separately, comparing monthly or weekly counts before or after the intervention. The same is true of those using ANOVAs or reporting *F* statistics (Priest and Brown Carter 2002; Ratcliffe and Makkai 2004); Weisburd and Green 1995a, b); Farrell et al. 1998). Very few modeled interaction effects or considered the treatment and control changes together. Interrupted time series designs were used in 4 of the studies (Katz et al. 2001; Lawton et al. 2005; Novak et al. 1999; Roman et al. 2005). Katz et al. modeled the treatment and catchment area effects separately; Lawton et al. and Roman et al. modeled the treatment, catchment and control areas separately; and Novak et al. added a fourth model for a catchment control area. The time series modeling was done with varying degrees of sophistication regarding spatial lags and autocorrelation to account for temporal and spatial dependency in the data. Regression analysis was used in three studies (Braga and Bond 2008; Braga et al. 1999; Eck and Spelman (1987). The latter used monthly time point data in their analysis, whilst the others used pre- and post-data for multiple treatment, control and catchment areas. One study used correlational analyses (Chenery et al. 1997), and in this case the authors correlated rates in the treatment area with those in the adjacent divisions before and during the intervention. Finally, one study (McCabe 2009) used hierarchical linear modeling to enable simultaneous modeling of effects in distinct treatment areas. The point of central importance is that this variation in the methods adopted meant that the data were not available in a standard format across all studies.

Meta-analysis

Summary of the data used

Across the 44 studies, the most consistently reported findings were descriptive statistics that contrasted the counts of crime pre- and post- intervention for (at least) one treatment area, one control area, and one catchment area that surrounded the treatment area. Such data were available for 16 studies. However, as can be seen in Table 3, the specific evaluation design employed varied across studies. For most (*n*=11), only one control area was used for the treatment and catchment areas. However, for a small number of studies (*n*=5), *independent* control areas were identified for both the treatment and catchment areas. In some studies, data were collected for more than one catchment area for each treatment area (*n*=4). For others, data were collected for the periods before, during and after intervention. For some studies (*n*=5), areas were allocated to treatment and control conditions using a random allocation strategy, but for the majority they were not (*n*=11). Finally, some studies gave counts for multiple crime types, while others only examined a single category of offense (*n*=12). One

Table 3 Summary of the evaluation designs of the studies in the meta-analysis

	RCT	Periods	Control areas	Catchment controls?	Catchment area(s)	Crime type (and Source)
Allat (1984)	No	Before, during and after	One area selected	No	Two adjacent estates selected. One of private, one of public housing.	Burglary (RC)
Braga and Bond 2008	Yes	Before, after	17 control areas (collapsed data available for analysis)	Yes	17 catchment areas (collapsed data available for analysis)	All crimes (CFS)
Braga et al. (1999)	Yes	Before, after	12 control areas	Yes	12 catchment areas	All crimes (RC/CFS)
Cummings (2006)	No	Before, during	One area selected	No	Two catchment areas	Burglary (RC)
Esbensen (1987)	No	Before, during	One area selected as control; one area as catchment control	Yes	One catchment area	All crime/index crimes/public fisorder (RC)
Farrell et al. (1998)	No	Before, during and after	One area selected	No	One catchment area	Burglary (recorded crime)
Grogger (2002)	No	Before, after	One area selected	No	One catchment area	Gang violence (RC)
Higgins and Coldren (2000)	No	Before, after	One area selected	No	One catchment area	Narcotics (RC)
Mazerolle et al. (2000)	Yes	Before, after	50 control addresses; 43 residential, 7 commercial (collapsed data available for analysis)	Yes	50 catchment addresses; 43 residential, 7 commercial (collapsed data available for analysis)	Drug crime (CFS). Separated by commercial/residential addresses
McGarrell et al. (2001)	No	Before, after	One area selected	No	One catchment area	Firearm related crime (RC)
Press (1971)	No	Before, after	One area selected	No	One catchment area	Multiple categories- 10 'inside' crime types and 10 'outside' crime types (RC)
Ratcliffe et al. (2010)	Yes	Before and during	60 control areas (collapsed data available for analysis)	No	55 catchment areas (collapsed data available for analysis)	Violent crime
Segrave and Collins (2005)	No	Before and after	One area selected	No	Three catchment areas; different adjacent areas	Violent crime, property crime, disorder
Sherman and Rogan (1995)	No	Before and during	One area selected	No	Two catchment areas; contiguous police beats and the remainder of the police force area	Gun crime
Wagers (2007)	No	Before and after	One area selected	No	One catchment area	Part 1 crime (recorded crime)
Weisburd and Green (1995b)	Yes	Before and after	28 control areas (collapsed data on disorder-public morals available for analysis)	Yes	28 catchment areas (collapsed data on disorder-public morals available for analysis)	Violence/property/disorder and drug crime (CFS)

consequence of these methodological differences is that it was possible to calculate more than one odds ratio for some of the studies.

For many studies, there was only one treatment, catchment and control area. In others (e.g., Braga and Bond 2008), there were multiple treatment, catchment and control areas. In the case of the latter, data were nearly always unavailable for each individual area and so analyses were conducted in the aggregate.

Best– and worst-case scenarios

Figure 2 shows the individual effect sizes and confidence intervals for the best case-scenario for both the treatment and catchment areas for all 16 studies. Where there was only one observation for a particular study, this was used in both the best- and worst-case scenarios. Where there were multiple observations, for the best-case scenario, the observations used were those for which the treatment effect was most positive (i.e., in favor of a treatment effect). If there were multiple observations for which there was an equal treatment effect, the observation for which the most positive effect was observed for the catchment area was selected.

For some studies, data were available for catchment control areas as well as treatment control areas. When calculating the odds ratios for the catchment areas, we used the best available control area; that is, where there is a separate catchment control area we used that. Where there is not, we used the control area identified for the treatment area.

The Forest plot (left panel) of Fig. 2 shows the point estimates for the odds ratio and associated confidence intervals. Where the former is greater than one this indicates that the outcome favors treatment. Where it is less than one this indicates that crime increased in the treatment area at a rate that exceeded that observed in the control area (in relative terms). The left panel also shows the same calculation for the catchment area. Here, where the point estimate is greater than one, this indicates a
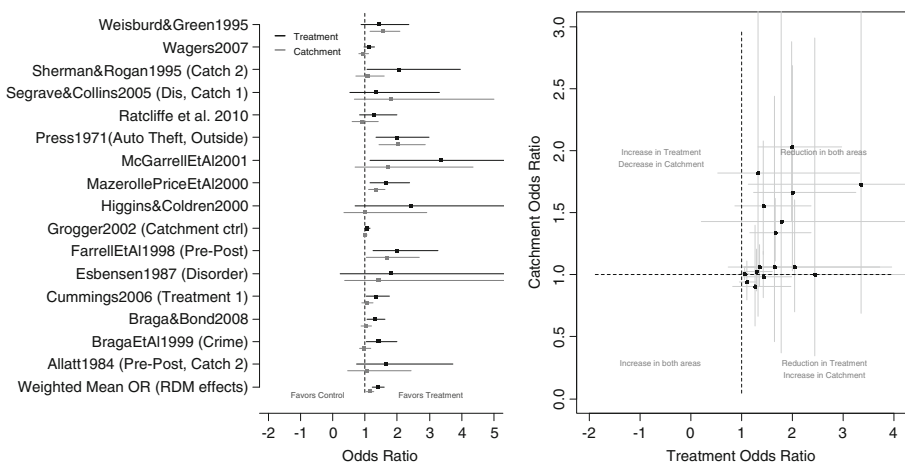

Fig. 2 Individual effect size estimates and confidence intervals for the best case scenario across the 16 studies (*left panel*), and a scatter plot showing the effect size estimates and confidence intervals for the treatment areas plotted against those for the catchment areas (*right panel*). Note: Weighted mean odds ratio (OR) was calculated using a random effects model

reduction in the catchment area relative to the control and hence a possible diffusion of crime-control benefits. Where the point estimate is less than one, this indicates that crime increased in the catchment area relative to the control and hence suggests a possible displacement of crime. The right panel shows a scatter plot of the effect size estimates and confidence intervals for the treatment areas plotted against those for the catchment areas. Where the evidence generally points to a positive overall effect (crime reduction in the treatment area and diffusion to the catchment area), we would expect a greater sample of points in the top right quadrant of the scatter plot.

Considering the general trend, we computed the Q statistic (see Lipsey and Wilson 2001) to determine if the variation in effect sizes indicated variation above and beyond that which would be expected for sampling error alone. For the treatment and catchments areas, the respective values of 34.8 ($df=15$) and 37.9 ($df=15$) were statistically significant at the $p<0.05$ level. Consequently, to calculate the weighted mean effect, we used a random effects model.

Overall, the weighted mean OR of 1.39 (CI: 1.22-1.59) suggests a positive significant effect at the treatment sites ($p<0.05$). In interpreting this finding the reader should recall that we included in this review only those studies for which data were available for treatment, catchment and control areas. Consequently, other studies that examined changes in treatment and control areas alone are excluded from the analysis. Thus, the reader should not interpret the effect size estimate for the treatment areas as representing the treatment effect for geographically focused police interventions in general, but just that for the subset of studies for which changes in catchment areas as well as the treatment and control areas were considered. For the catchment areas, the results are also positive and rather than suggesting that crime increased in the catchment areas, for the best case scenario the mean OR of 1.14 (CI: 1.03-1.14) indicates that it decreased overall ($p<0.05$). On average in this case, therefore, treatment areas enjoyed a reduction in crime and catchment areas a potential diffusion of benefits.

Figure 3 shows the same results for the worst case scenario. In this case, the Q statistic was not statistically significant for the treatment (Q=20.5, $df=14$, $p>0.05$) but was for the catchment (Q=26.9, $df=14$, $p<0.05$) areas and so both random effects and fixed effects model were conducted for the former[3] and only a random effects model for the latter. The weighted mean (random effects) OR for the treatment areas of 1.15 (CI: 1.05-1.27) suggests a positive impact of intervention ($p<0.05$). For the catchment areas, the weighted mean OR of 1.04 (CI: 0.95–1.13) is in the same direction as that for the best-scenario but is non-significant. In this case, then, there was evidence of a treatment effect but not a diffusion of benefit. It is interesting to note that the difference between the weighted mean effect sizes for the best and worse-case scenarios for the catchment areas is not large (ORs of 1.14 and 1.04 respectively).

Permutation tests

As discussed above, one potential issue with presenting data for the best- and worst-case scenarios alone is that they may be overly sensitive to the presence of outliers. An alternative is to not only compute the best- and worst-case scenarios but all of

---

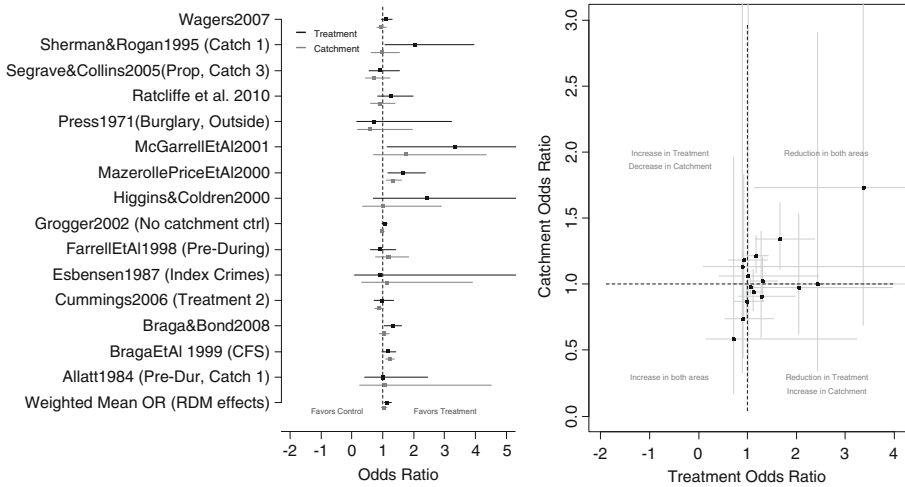[3] For a fixed effect model, the weighted mean OR was 1.11 (CI: 1.05-1.18).

Fig. 3 Individual effect size estimates and confidence intervals for the worst case scenario across the 15 studies (*left panel*), and a scatter plot showing the effect size estimates and confidence intervals for the treatment areas plotted against those for the catchment areas (*right panel*). In the case of Weisburd and Green (1995a, b), count data were only available for the treatment and catchment areas for public morals disorder for which reductions were observed in both the treatment and catchment areas. Data for the other types of offenses, for which reductions were not observed in both areas, were unavailable. Hence, the data for this study are included in the best-case, but not the worst-case, scenario. For the treatment OR, a fixed effects model was used

those in between. Considering all studies, there are 69,120 possible permutations. Instead of computing every mean effect size, we use a Monte Carlo simulation to sample 1,000 unique mean effect size estimates from all possible permutations. For each (re)sample, we use a random effects model to compute the weighted mean effect sizes and confidence intervals as these provide a more conservative test of the hypotheses under investigation. Figure 4 shows the results of this analysis. The point estimates and confidence intervals at the top of the two Forest plots are a sample of the 1,000 permutations, which are included for the purposes of illustration. The point estimates and confidence intervals at the bottom of the plot are the mean values computed across all permutations. Those shown in black represent the mean of the mean effect sizes and the mean of the upper and lower confidence intervals. Those shown in gray are the upper and lower 95% confidence intervals for the mean of the upper and lower confidence intervals. Consistent with the results discussed above, the analysis suggests that relative to the control areas, there was a reduction in crime in both the treatment areas and catchment areas, although this was only significant for the treatment areas.

Examining variation

In this section, analyses are presented for studies that may be meaningfully grouped together. Table 4 shows a summary of these. In each case, permutation tests were undertaken, and we report the average of the mean effect size across the permutations and associated average upper and lower confidence intervals. Where the number of possible permutations is greater than 1,000, we randomly select 1,000.
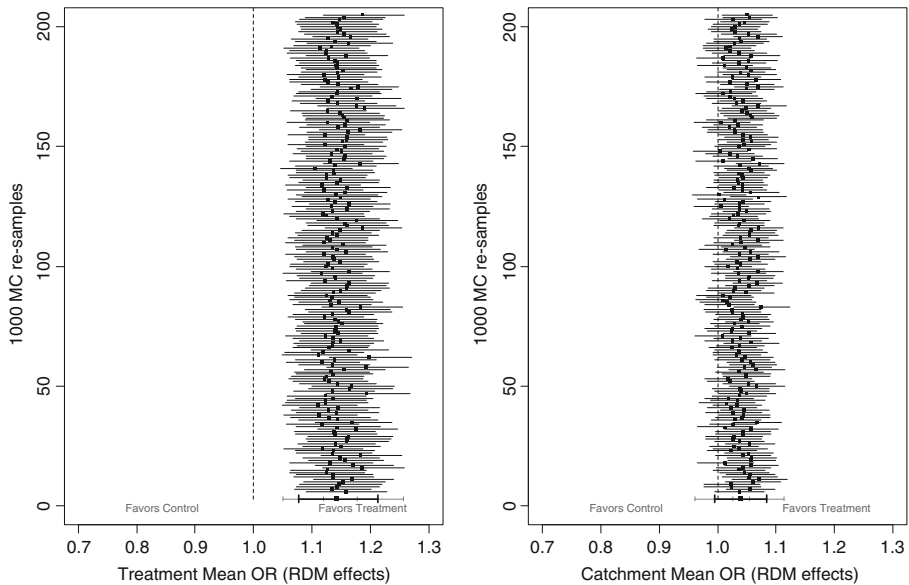
Fig. 4 Weighted mean effect size estimates and confidence intervals for all studies for 200 of the 1,000 Monte Carlo re-samples, and a summary of the distribution for the 1,000 samples (*left panel* treatment areas; *right panel* catchment areas)

Where it is lower, we compute the effect sizes for all possible permutations. Firstly, we examined results for studies which employed a random allocation strategy to assign areas to treatment and control groups. Such studies minimize threats to internal validity and hence any changes observed in the treatment and catchment areas can be attributed to the effects of intervention with confidence (Campbell and Stanley 1963). As the average of the effect sizes is substantially over 1 and the average lower confidence interval is also over 1, we conclude that overall crime appears to have reduced significantly in the treatment areas. In the catchment areas, the conclusion is slightly less clear but appears to indicate that a diffusion of crime control benefit is still the more likely outcome.

As discussed, for the quasi-experimental studies, threats to internal validity weaken causal inferences. For this reason, it seems reasonable to apply stricter criteria when estimating the extent to which crime might have been displaced or benefits diffused when examining those studies which used quasi-experimental designs. One such criterion is that the search for displacement should be conditional on the demonstration of an intervention effect in the treatment area(s) (see Weisburd and Green 1995a; Bowers and Johnson 2003). The results for this subset suggest that crime reduced in both the treatment and catchment areas; implying a positive impact of intervention and a potential diffusion of benefit.

For completeness, we also present results for all studies, irrespective of whether changes were observed in the treatment areas. In this case, there appears to be a positive effect of treatment, although the weighted mean effect is obviously lower than for the subset of schemes considered above. In the case of the catchment areas, while the mean weighted OR is positive, the confidence intervals suggest that the observed trend was unreliable.

Table 4 A summary of the average mean effect sizes and confidence intervals for meaningful subsets of studies

	Mean of mean OR	Mean of upper CI	Mean of lower CI	Number of studies	Possible permutations
RCTs:					
Treatment	1.34	1.46	1.13	5	2
Catchment	1.15	1.33	1.01		
Quasi-experimental designs:					
All					
Treatment	1.08	1.16	1.02	11	25,920
Catchment	1.01	1.05	0.97		
Successful only					
Treatment	1.61	1.86	1.39	5	4
Catchment	1.20	1.37	1.06		
Large treatment areas:					
Treatment	1.20	1.35	1.08	7	720
Catchment	1.05	1.15	0.96		
Medium treatment areas:					
Treatment	1.10	1.18	1.01	6	16
Catchment	1.08	1.18	0.98		
Small treatment areas:					
Treatment	1.30	1.57	1.08	3	6
Catchment	1.08	1.23	0.91		
POP/Community policing schemes:					
Treatment	1.20	1.34	1.08	6	18
Catchment	1.07	1.17	0.99		
Increased manpower schemes:					
Treatment	1.19	1.33	1.06	8	480
Catchment	1.00	1.10	0.96		
Other schemes:					
Treatment	1.08	1.18	1.01	4	8
Catchment	1.05	1.12	0.97		

A further way of meaningfully categorizing the studies is in terms of the geographical area they cover. For example, it is possible that interventions that have the largest geographical coverage are least likely to displace crime. That is, in such areas, if offenders were deterred by the intervention, they might have to travel considerable distances to seek out new opportunities; something that ethnographic research suggests is the exception to the rule (e.g., Rengert and Wasilchick 1995). Accordingly, we performed separate analyses for those studies that could be categorized as having a large geographical coverage and those that could be categorized as having a small or medium-sized geographic range.

Seven of the studies were categorized as large. Again, the results suggest a positive effect of treatment, and a trend in favor of a diffusion of crime control benefits albeit

non-significant. The areas described in six of the studies were classified as medium. The results in Table 4 show that the pattern here is also in line with a reliable treatment effect accompanied by a non-significant trend in favor of a diffusion of benefit. Only three of the studies were classified as evaluating interventions that took place over a small geographic area. Table 4, which shows the results for all six possible permutations of the data, supports the conclusions already discussed for the other categories. Thus, similar trends appear to emerge, irrespective of the size of the geographic area covered (at least for the classification criteria used here), indicating that size might not be a key factor in determining levels of displacement or diffusion.

The interventions employed across the 16 studies were categorized into three broad types: (1) POP and community policing; (2) increased manpower (hotspots policing, crackdown, foot patrols); and (3) other types of focused police-led intervention. Table 4 shows the results for those evaluations that were categorized as examining predominantly POP and community policing strategies (n=6).[4] The results show that reductions were consistently observed in both the treatment and catchment areas, although the effects observed in the catchment areas may be due to sampling error rather than a reliable effect of intervention.

Table 4 also shows the same analyses for those studies in which the predominant strategy was to increase manpower in a geographically focused area (n=8). Again, there appears to be a positive effect in the treatment areas, but overall the results suggest *no change* in the catchment areas. For other types of police-led intervention (n=4), the results were similar but with a non-significant trend which suggested that crime reduced in the catchments as well as treatment areas.

Finally, for four of the five RCT studies and one of the quasi-experimental studies, data were collected for both treatment and control catchment areas. The control catchment area may represent a more reliable counterfactual for the treatment catchment area than the control area used for the treatment area. A natural question then is whether the use of the different control areas in the estimates of the effect sizes leads to different conclusions. Overall, using either the specifically constructed catchment control areas—or the more generic treatment control area generates the same pattern of results—a positive and significant treatment and catchment effect. However, the average mean effect size is smaller for the catchment area when catchment control areas are used; the average absolute difference in the unweighted measures of effect size being an absolute difference of 0.16.

Publication bias

All but two of the studies examined were published evaluations and hence the findings reported may be biased. Fortunately, procedures are available to examine the possible effects of publication bias. Here, we use the trim and fill algorithm proposed by Duval and Tweedie (2000) to examine publication bias for both the best and the worst case scenarios presented in Figs. 2 and 3. For the best-case scenario

---

[4] It could be reasonably argued that POP and community policing should be analysed separately. There was only 1 study of the latter type, however, so this precluded statistical analysis of these as separate subcategories. Note although each studies had a main intervention classification, in some cases elements fell into another category. This is why the number of cases across the three intervention type categories in table 4 add up to more than 16.

for treatment areas, 8 studies are suggested as missing. The original point estimate in log units was 0.337, which corresponds to a risk ratio of 1.40. The adjusted point estimate is 0.193, which is an odds ratio of 1.21. For the worst case scenario for treatment areas, one study is suggested as missing. The original point estimate in log units was 0.156, which corresponds to a risk ratio of 1.17. The adjusted point estimate is 0.151, which is an odds ratio of 1.16. For the best-case scenario for catchment areas, 3 studies are suggested as missing. The original point estimate in log units was 0.136, which corresponds to a risk ratio of 1.15. The adjusted point estimate is 0.093, which is an odds ratio of 1.10. Finally, for the worst-case scenario for catchment areas, no studies are suggested as missing. The original point estimate in log units was 0.068, which corresponds to a risk ratio of 1.07. The adjusted point estimate remains the same in this case.

In short, the results suggest that accounting for publication bias does not affect the initial conclusions of the meta-analysis; the adjusted effect size estimates are fairly similar to the original ones. The slight exception to this is for the best case scenario for the treatment areas where there is a drop in the odds ratio. Here, even the adjusted effect size continues to suggest a significant effect of treatment. It appears, therefore, that publication bias is not a major concern for this analysis.

Discussions and conclusions

The aim of this research was to assess the degree to which geographically focused policing initiatives displace crime or diffuse crime control benefits to nearby areas. Hence, it should be stressed that the results are for a particular subset of crime prevention initiatives; those for which local level displacement and diffusion effects were examined. In choosing a fairly broad definition of 'focused policing initiatives' (including, for example, hotspots policing, POP, police patrols and crackdowns, broken windows policing and civil injunctions), our intention was to be able to make statements about focused policing efforts more generally. The number of studies that met our criteria was respectable at 44. Furthermore, there was a reasonable subset ($n=16$) that we were able to include in our formal meta-analysis.

The main findings of the meta-analysis suggested that on average geographically focused policing initiatives for which data were available:

(1). were associated with significant reductions in crime and/or disorder;
(2). overall, showed changes in catchment areas that were non-significant but with a trend in favor of a diffusion of benefit. This suggests that displacement is rarer than is often assumed and that diffusion is a common phenomenon; and
(3). showed variation in outcomes depending on their type. POP-based schemes appear to be more associated with a diffusion of benefit than do schemes that rely on intensive policing alone.

To assess the effects of study design, intervention size and intervention type, subsets of the data were considered in separate analyses. For those studies with the highest level of methodological adequacy (RCTs), there is evidence of a treatment effect and significant diffusion of benefit. For the quasi-experimental designs, when the analysis focused on that subset of observations for which a treatment effect was

observed, an apparently reliable diffusion of benefit was also noted. For studies where data were collected for a catchment control area as well as a treatment control area, it was evident that using a different control area can ultimately influence the results and therefore it is advisable to collect data for control catchment areas where possible. The results suggest little difference in the conclusions for those interventions which vary in the size of the area over which they are implemented. Possible publication bias was addressed using the trim and fill procedure, and the conclusion was that publication bias is unlikely to affect the conclusions drawn.

These findings, taken together with results from other reviews of displacement and diffusion, offer two primary implications for criminological theory. First, because diffusion of benefits was observed somewhat more readily than was displacement, it suggests that offenders actively engage in situational reasoning and rationality, which is a primary assertion of the rational choice perspective. Were offenders compelled in their disposition to offend, less diffusion and more displacement would have been expected or at least reductions in the targeted areas would not have been found. This implies that criminal behavior may be more "normal," in the sense that it is driven by satisfaction of fundamental needs and wants which are guided by cognitive reasoning, rather than by sociological or psychologically entrenched deviant "propensities" (i.e. determinism).

It is possible that since the evaluations reviewed here entailed focused police action that some incapacitation effect may be responsible for both the observed reductions in the targeted areas as well as the tendency toward diffusion (i.e. since fewer offenders might have been on the street). If so, this would discount the evidence in support of reasoning among offenders. Though the analyses performed in this review cannot rule out this incapacitation hypothesis, given the corroborating finding among evaluations of situational interventions (Guerette and Bowers 2009) which largely did not rely on incapacitation, this scenario seems less likely. It is also worth noting that it is unlikely that the focused policing operations resulted in the capture of all offenders during the course of the intervention meaning that some would have remained at liberty to continue offending. Thus, if the remainder were compelled by disposition to offend, the patterns of crime reduction in the intervention areas or at least the potential diffusion effects in the catchment areas would have been less pronounced.

Second, the findings also provide continued support for the notion that crime opportunity is discontinuous, rather than uniform. This supports the routine activity perspective and is in line with research that has examined the heterogeneity of crime risk (Brantingham and Brantingham 1981; Eck et al. 2007; Pease 1998; Sherman et al. 1989). If crime opportunity were constant then much more displacement would be expected since offenders could easily go elsewhere to reap the rewards of crime. Instead, the evidence suggests that crime opportunity concentrates to such a degree that effort to prevent crime by the elimination of offending opportunities, whether that be through focused policing or situational alterations, stands as a formidable means of crime reduction.

As with any research, there are a number of caveats associated with the review and outstanding questions that could be usefully addressed. In the authors' view three areas of enquiry would be particularly useful. First is the methodological issue of how to analyze displacement at the individual initiative level. In the process of

this review we found many different strategies for doing this; all with their own merits and drawbacks. One particular issue was that many authors consider changes in treatment, catchment and control areas independently, not relating changes in one type of area to those in another. This was evident for some studies that compared changes pre- and post-intervention and for some that used formal time-series models. In the future, it would be helpful if study authors also reported interaction terms to indicate if the changes observed in catchment areas exceed those observed in suitable control areas, above and beyond what might be expected as a result of sampling error.

Second is a consideration of the physical areas selected to assess displacement and diffusion of benefit. There was considerable variation in the selection of the catchment areas used across studies; and almost none considered the issue of directional displacement, the default being to consider changes in a concentric 'ring' around a treatment area.

Third is further investigation into the role of context and mechanism with regard to displacement and diffusion of crime control benefit. It is likely that the degree to which displacement and/or diffusion occurs will depend on a broad range of different factors including:

- The effectiveness of the policing activity in reducing crime
- The type of policing intervention
- The type of crime that is the focus of intervention
- The intensity and effectiveness of implementation
- The physical and social context of the scheme
- The perceptions and motivations of offenders in the area
- The context of the catchment area

Further investigation of these mediating factors would be useful in alerting practitioners to the conditions in which displacement is more or less likely.

In summary the message from this review is a positive one to those involved in the sort of operational policing initiatives considered here, the main point being that displacement is far from inevitable as a result of such endeavor, and, in fact that the opposite, a diffusion of crime control benefits appears to be a frequent outcome.

Appendix 1 Electronic databases searched

The following databases were 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 also searched the publications of the following groups:

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

Publications from national policing agencies were also searched. These included:

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 Natoinal Police (Politi)
8. The Netherlands Police (Politie)
9. New Zealand Police

Appendix 2 Keywords used to search

Searches of electronic databases used 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)

References

Allat, P. (1984). Residential security: containment and displacement of burglary. *Howard Journal of Criminal Justice 23*, 99–116.

Barr, R., & Pease, K. (1990). Crime placement, displacement and deflection. In Michael Tonry & N. Morris (Eds.), *Crime and justice: A review of research, vol. 12*. Chicago: University of Chicago Press.

Bowers, K., & Johnson, S. (2003). Measuring the geographical displacement and diffusion of benefit effects of crime prevention activity. *Journal of Quantitative Criminology, 193*, 275–301.

Braga, A. A. (2007). Effects of hot spots policing on crime. A Campbell Collaboration systematic review, available at: http://www.aic.gov.au/campbellcj/reviews/titles.html.

Braga, A. A., & Bond, B. J. (2008). Policing crime and disorder hot spots: A randomized controlled trial. *Criminology, 46*(3), 577–607.

Braga, A. A., Weisburd, D. L., Waring, E. J., Mazerolle, L. G., Spelman, W., & Gajewski, F. (1999). Problem-oriented policing in violent crime places: A randomized controlled experiment. *Criminology, 37*(3), 541–580.

Brantingham, P. L., & Brantingham, P. J. (1981). Notes on the geometry of crime. In P. Brantingham & P. Brantingham (Eds.), *Environmental criminology*. Beverly Hills: Sage Publications.

Campbell, D. T. & Stanley, J. C. (1963). *Experimental and quasi-experimental designsfor research*. Dallas: Houghton Mifflin.

Chaiken, J. M., Lawless, M., & Stevenson, K. A. (1974). The impact of police activity on crime. *Urban Analysis, 3*, 173–205.

Clarke, R. V., & Eck, J. (2005). *Crime analysis for problem solvers in 60 small steps*. Washington: Office of Community Oriented Policing Services, United States Department of Justice.

Clarke, R. V., & Weisburd, D. (1994). Diffusion of crime control benefits: Observations on the reverse of displacement. In R. V. Clarke (Ed.), *Crime prevention studies, vol. 2*. Monsey: Criminal Justice Press.

Cornish, D. B., & Clarke, R. V. (Eds.). (1986). *The reasoning criminal*. New York: Springer-Verlag.

Duval, S. J., & Tweedie, R. L. (2000). A non-parametric "trim and fill" method of accounting for publication bias in meta-analysis. *Journal of the American Statistical Association, 95*, 89–98.

Eck, J. E. (1993). The threat of crime displacement. *Criminal Justice Abstracts, 253*, 527–546.

Eck, J., Clarke, R. V., & Guerette, R. T. (2007). Risky facilities: Crime concentration in homogeneous sets of establishments and facilities. *Crime Prevention Studies, 21*, 225–264.

Farrington, D., & Welsh, B. (2002). Effects of improved street lighting on crime: A systematic review. Home Office Research, Development and Statistics Directorate, August, Research Study 251.

Farrington, D. P., Gill, M., Waples, S. J., & Argomaniz, J. (2007). The effects of closed-circuit television on crime: Meta-analysis of an English national quasiexperimental multi-site evaluation. *Journal of Experimental Criminology, 3*(1), 21–38.

Green, L. (1995). Cleaning up drug hot spots in Oakland, California: The displacement and diffusion effects. *Justice Quarterly, 124*, 737–754.

Guerette, R. T., & Bowers, K. (2009). Assessing the Extent of Crime Displacement and Diffusion of Benefits: A Review of Situational Crime Prevention Evaluations. *Criminology, 47*(4).

Hesseling, Rene. (1994). Displacement: A review of the empirical literature. In R. V. Clarke (Ed.), *Crime prevention studies, vol. 3*. Monsey: Criminal Justice Press.

Johnson, S. D. (2009). Potential uses of computational methods in the evaluation of crime reduction activity. In J. Knuttson & N. Tilley (Eds.), *Evaluating crime prevention*. New York: Criminal Justice Press.

Lipsey, M. W., & Wilson, D. B. (2001). *Practical meta-analysis*. Thousand Oaks: Sage.

Marchant, P. (2004). A demonstration that the claim that brighter lighting reduces crime is unfounded. *British Journal of Criminology, 44*, 441–447.

Marchant, P. (2005). What works? A critical note on the evaluation of crime reduction initiatives. *Crime Prevention and Community Safety, 7*(2), 7–13.

Lorraine, M., Rombouts, S., & Soole, D. W. (2007). Street-level drug law enforcement: A meta-analytic review. A Campbell Collaboration Systematic review, available at: http://www.campbellcollaboration.org/library.php.

Miethe, T. D. (1991). Citizen-based crime control activity and victimization risks: An examination of displacement and free rider effects. *Criminology, 29*, 419–439.

Pease, K. (1998). *Repeat victimization: Taking stock. Crime detection and prevention paper series paper #90*. London: Home Office.

Petticrew, M., & Roberts, H. (2006). *Systematic reviews in the social sciences: A practical guide*. Oxford: Blackwell.

Rengert, G., Wasilchick, J. (1985). Suburban burglary: A time and a place for everything. Springfield, IL: CharlesThomas

Sherman, L., Gartin, P. R., & Buerger, M. E. (1989). Hotspots of predatory crime: Routine activities in the criminology of place. *Criminology, 27*(1), 27–56.

Weisburd, D., & Green, L. (1995a). Measuring immediate spatial displacement: Methodological issues and problems. In J. Eck & D. Weisburd (Eds.), *Crime and place. Crime prevention studies, vol. 4*. Monsey: Criminal Justice Press.

Weisburd, D., & Green, L. (1995b). Policing drug hot spots: The Jersey City drug market analysis experiment. *Justice Quarterly, 12*(4), 711–735.

Weisburd, D., Wyckoff, L., Ready, J., Eck, J., Hinkle, J. C., & Gajewski, F. (2006). Does crime just move around the corner? A controlled study of spatial displacement and diffusion of crime control benefits. *Criminology, 44*(3), 549–591.

Weisburd, D., Telep, C. W., Hinkle, J. C., & Eck, J. E. (2008). The effects of problem-oriented policing on crime and disorder. A Campbell Collaboration Systematic Review available at: http://test.c2admin.org/index.php?go=browse&sort=title&view=all&search=weisburd&search_criteria=author.

Weisburd, D., Telep, C. W., Hinkle, J. C., & Eck, J. E. (2010). Is problem-oriented policing effective in reducing crime and disorder? *Criminology & Public Policy, 9*(1), 139–172.

References of Eligible Studies

Bellamy, L. C. (1997). The toller lane crime management tool: Tackling crime more effectively. *Security Journal, 9*(1–3), 15–21.

Braga, A. A., & Bond, B. J. (2008b). Policing crime and disorder hot spots: A randomized controlled trial. *Criminology: An Interdisciplinary Journal, 46*(3), 577–607.

Braga, A. A., Weisburd, D. L., Waring, E. J., Mazerolle, L. G., Spelman, W., & Gajewski, F. (1999b). Problem-oriented policing in violent crime places: A randomized controlled experiment. *Criminology, 37*(3), 541–580.

Caeti, T. (1999). Houston's targeted beat program:? A quasi-experimental test of police patrol strategies. PhD. Diss., Sam Houston State University. Ann Arbor MI: University Microfilms International.

Cator, M. (2006). Operation mullion: Reducing anti-social behaviour and crime in and around Mayfield School. Hampshire Constabulary. Tilley Award Finalist.

Chenery, S., Holt, J., & Pease, K. (1997). Biting back II: Reducing repeat victimisation in Huddersfield. Crime Detection and Prevention Series, Paper 82, Home Office.

Clarke, R. V., & Goldstein, H. (2002). Reducing theft at construction sites: Lessons from a problem oriented project. *Crime Prevention Studies, 13*, 89–130.

Clarke, R. V., & Goldstein, H. (2003). Thefts from cars in city-center parking facitlities: A case study in implementing problem oriented policing. *Crime Prevention Studies, 15*, 257–298.

Cohen, J., & Ludwig, J. (2003). Policing crime guns. In J. Ludwig & P. J. Cook (Eds.), *Evaluating gun policy; effects on crime and violence* (pp. 217–250). Washington: Brookings Institute Press.

Cummings, R. (2006). 'What if': The counterfactual in program evaluation. *Evaluation Journal of Australasia, 6*(2), 6–15 (new series).

Curran, K., Dale, M., Edmunds, M., Hough, M., Millie, A., & Wagstaff, M. (2005). *Street crime in London: Deterrence, disruption and displacement*. London: Government Office for London.

Eck, J., & Spelman, W. (1987). Problem-solving: Problem oriented policing in Newport News. Police Executive Research Forum.

Esbensen, F. A. (1987). Foot patrols: Of what value. *American Journal of Police, 6*, 45. Retrieved from www.heonline.org.

Farrell, G., Chenery, S., & Pease, K. (1998). *Consolidating police crackdowns: Findings from an anti-burglary project. Police Research Series Paper 113*, London, UK.

Forrester, D. H., Chatterton, M. R., & Pease, K. (1988). *The Kirkholt burglary prevention demonstration project, Home Office Crime Prevention Unit Paper no. 13*. London: HM Stationary Office.

Green, L. (1995b). Cleaning up drug hot spots in Oakland. *California: The Displacement and Diffusion Effects, Justice Quarterly, 12*, 737.

Grogger, J. (2002). The effects of civil gang injunctions on reported violent crime: Evidence from Los Angeles County. *Journal of Law and Economics, 45*(1), 69–90.

Higgins, D. F., & Coldren, J. R. (2000). *Evaluating gang and drug house abatement in Chicago*. Chicago: Criminal Justice Authority.

Hope, T. J. (1994). Problem oriented policing and drug market locations: Three case studies. *Crime Prevention Studies, 2*, 5–31.

Hopkins, M. (2004). Targeting hotspots of alcohol-related town centre violence: A Nottinghamshire case study. *Security Journal, 17*(4), 53–66.

Kane, R. J. (2006). On the limits of social control: Structural deterrence and the policing of "Suppressible" crimes. *Justice Quarterly, 23*(2), 186–213.

Katz, C. M., Webb, V. J., & Schaefer, D. R. (2001). Assessment of the impact of quality-of-life policing on crime and disorder. *Justice Quarterly, 18*(4), 825–876.

Knutsson, J. (1996). Restoring public order in a city park. *Crime Prevention Journal, 6*, 133–151.

Lateef, A. B. (1974). Helicopter patrol in law enforcement - An evaluation. *Journal of Police Science and Administration, 2*(1), 62–65.

Lawton, B. A., Taylor, R. B., & Luongo, A. J. (2005). Police officers on drug corners in Philadelphia, drug crime, and violent crime: Intended, diffusion, and displacement impacts. *Justice Quarterly, 22*(4), 427–451.

Matthews, R. (1993). Kerb-crawling, prostitution and multi-agency policing. Crime Prevention Unit Paper 43.

Mazerolle, L. G., Price, J. F., & Roehl, J. (2000). Civil remedies and drug control: A randomized field trial in Oakland, California. *Evaluation Review, 24*(2), 212–241.

McCabe, J. E. (2009). The narcotics initiative: An examination of the NYPD approach to drug enforcement, 1995–2001. *Criminal Justice Policy Review, 20*, 170–187.

McGarrell, E. F., Chermak, S., Weiss, A., & Wilson, J. (2001). Reducing firearms violence through directed police patrol. *Criminology & Public Policy, 1*, 119–148.

Morris, S. (1996). Policing problem housing estates. Crime prevention and detection series, paper 74, Home Office.

Novak, K. J., Hartman, J. L., Holsinger, A. M., & Turner, M. G. (1999). The effects of aggressive policing of disorder on serious crime. *Policing: An International Journal of Police Strategies & Management, 22*(2), 171–190.

Press, S. J. (1971). *Some effects of an increase in police manpower in the 20th precinct of New York City. R-704-NYC*. New York: New York City Rand Institute.

Priest, T. B., & Brown Carter, D. (2002). Community-oriented policing: Assessing a police saturation operation (from policing and community partnerships, P 111–124, 2000, dennis J. stevens, ed., – see NCJ-194083). Retrieved from http://www.policetrainings.

Ratcliffe, J., & Makkai, T. (2004). *Diffusion of benefits: Evaluating a policing operation. Trends & issues in crime and criminal justice, 278*. Canberra: Australian Institute of Criminology.

Ratcliffe, J. H., Taniguchi, T., Groff, E. R., Wood, J. D. (2010). The Philadelphia foot patrol experiment: A randomized controlled trial of police patrol effectiveness in violent crime hotspots. Submitted to Criminology.

Roman, C., Cahill, M., Coggeshall, M., Lagerson, E., Courtney, S. (2005). Weed and seed initiative and crime displacement in south florida: An examination of spatial displacement associated with crime control initiatives and the redevelopment of public health.

Segrave, M., & Collins, L. (2005). *Evaluation of a suburban crime prevention team, Technical and background paper series, 14*. Canberra: Australian Institute of Criminology.

Sherman, L. W., & Rogan, D. P. (1995). Effects of gun seizures on gun violence: Hot spots patrol in Kansas City. *Justice Quarterly, 12*, 673.

Skogan, W. G., & Hartnett, S. M. (1997). *Community policing: Chicago style*. Oxford: Oxford University Press.

Smith, M. R. (2001). Police-led crackdowns and cleanups: An evaluation of a crime control initiative in Richmond, Virginia. *Crime and Delinquency, 47*(1), 60–83.

Tilley, N., & Hopkins, M. (1998). *Business as usual: An evaluation of the small business and crime initiative. Police Research Series Paper 95*. London: Home Office.

Tita, G., Riley, K. J., Ridgeway, G., Grammich, C., Abrahamse, A., & Greenwood, P. W. (2003). *Reducing gun violence: Results from an intervention in East Los Angeles*. Santa Monica: Rand Public Safety and Justice, Rand Corporation.

Wagers, M. (2007). Broken windows policing: The LAPD experience, Ph.D. diss., Rutgers The State University of New Jersey - Newark.

Weidner, R. R. (1999). "I won't do Manhattan": A study of the causes and consequences of a decline in street prostitution. Ph.D. diss., Rutgers The State University of New Jersey - Newark.

Weisburd, D., & Green, L. (1995c). Policing drug hot spots: The Jersey City Drug Market Analysis experiment. *Justice Quarterly, 12*(4), 711–742.

Weisburd, D., Wyckoff, L. A., Ready, J., Eck, J. E., Hinkle, J. C., & Gajewski, F. (2006b). Does crime just move around the corner? A controlled study of spatial displacement and diffusion of crime control benefits. Criminology: An Interdisciplinary Journal, 4.

Dr. Kate Bowers is a Reader in the UCL Department of Security and Crime Science.

Dr. Shane Johnson is a Reader in the UCL Department of Security and Crime Science.

Dr. Rob Guerette is Associate Professor of Criminal Justice at the School of International & Public Affairs at Florida International University.

Lucia Summers is a Research Fellow at the UCL Department of Security and Crime Science.

Suzanne Poynton is an Honorary Research Associate at the UCL Department of Security and Crime Science.