ASSESSING THE EXTENT OF CRIME DISPLACEMENT AND DIFFUSION OF BENEFITS: A REVIEW OF SITUATIONAL CRIME PREVENTION EVALUATIONS*

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Few criticisms of situational crime-prevention (SCP) efforts are as frequent or prevalent as claims of displacement. Despite emerging evidence to the contrary, the prevailing sentiment seems to be that crime displacement is inevitable. This study examined 102 evaluations of situationally focused crime-prevention projects in an effort to determine the extent to which crime displacement was observed. The results indicate that of the 102 studies that examined (or allowed for examination of) displacement and diffusion effects, there were 574 observations. Displacement was observed in 26 percent of those observations. The opposite of displacement, diffusion of benefit, was observed in 27 percent of the observations. Moreover, the 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

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than the treatment effect, suggesting that the intervention was still beneficial. Implications for theory and future research are discussed.

We know that they (offenders) will go to other places to commit crime. But as long as it is outside of our jurisdiction, we don’t care.

Police officer

In the aftermath of situational crime-prevention (SCP) efforts, does crime really just go someplace else? It is a research question that has been asked before (Weisburd et al., 2006), but the answer, like most subjected to the scientific endeavor, remains a work in progress. Early conjecture suggested that crime does move, and if this is true, then policing and other prevention efforts focused at the situational level just might be pointless. Certainly, critics of the situational approach contend that they are quickly citing the certainty that crime will simply relocate, albeit without providing comprehensive proof that it in fact does (for review, see Eck, 1993; Hesseling, 1994; Hill and Pease, 2001). Even some practitioners readily concede the likelihood of displacement as a consequence of their efforts and rationalize that as long as crime is controlled within their own jurisdiction or premise, their actions are still worthwhile. Surprisingly, some advocates of the situational approach seem to have accepted its certainty as well (Barnes, 1995).

Others, however, have argued against the sureness of displacement and have offered a theoretical rationale as to why displacement is not likely (Clarke and Weisburd, 1994; Cornish and Clarke, 1987, 1989). At the same time, mounting evidence suggests that crime displacement often does not occur (Barr and Pease, 1990; Bowers and Johnson, 2003; Brantingham and Brantingham, 2003; Eck, 1993; Hesseling, 1994; Reppetto, 1976; Weisburd et al., 2006; Weisburd and Green, 1995b). Although some have asserted that past assumptions of the inevitableness of crime displacement have now been replaced by a new view that displacement is less than definite and often inconsequential (Weisburd et al., 2006: 551), it still seems probable that the inevitability assumption remains most prevalent among criminologists and practitioners alike. The headline from a recent media report on the use of closed-circuit television (CCTV) in Washington, DC, offers a case in point: “Cameras have cut violence, study says. Skeptics suspect crime ‘displacement’” (Klein, 2008). Whatever the case, the burden of proof clearly has been placed on disproving earlier (and arguably still prevailing) suppositions about the inescapability of crime displacement.

1. Statement given during an invited guest lecture to undergraduate students in an Introduction to Crime Prevention class.
Yet, the evidence on displacement remains limited. Previous reviews have been hampered by small numbers of available studies that have examined displacement and by methodologies that have precluded the use of more reliable empirical determinations of the extent of displacement effects. Indeed, most focused empirical examinations of displacement within specific crime-prevention evaluations have been peripheral to the aim of determining program effectiveness (for an exception, see Weisburd et al., 2006). Evaluations of localized prevention efforts can tell us about the extent of displacement or diffusion at that time and place, but the findings are not generalizable.

This study further investigates the issue of crime displacement and entails a systematic review of 102 evaluations of situationally focused crime-prevention projects in an effort to determine the extent to which displacement was observed. It assesses the frequency of the opposite of displacement, which is the diffusion of benefit, among those studies, and it applies recently developed techniques that allow for the determination of overall outcomes of the prevention project while taking into account displacement and diffusion effects (Bowers and Johnson, 2003; Clarke and Eck, 2005). The studies examined were limited to those with a prominent or exclusive situational component, which could be classified using Cornish and Clarke’s (2003) 25 techniques.

DISPLACEMENT AND DIFFUSION OF CRIME-CONTROL BENEFIT

Displacement is the relocation of a crime from one place, time, target, offense, tactic, or offender to another as a result of some crime-prevention initiative. By far, spatial displacement is the form most commonly recognized (Eck, 1993), although the other five are also frequently acknowledged by those who study crime-prevention effects. 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 replacement (new offenders replace old offenders who have been removed or who have desisted from crime).  

2. Although offender displacement is often mentioned as a sixth type, it is more accurate to describe this as offender replacement because it entails new offenders that take 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.

3. For more on this, see Reppetto (1976).
Figure 1. Relationship of Displacement and Diffusion to an Observed Intervention Effect

On the other end of the 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, 1974a; Green, 1995; Miethe, 1991; Weisburd et al., 2006; Weisburd and Green, 1995b). 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, which include 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 because improvements were gained without expending resources in those areas.

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 only resulted in an increase in the
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comparison area of approximately 50 crimes post-intervention. Thus, a net reduction of 50 crimes was still achieved. Furthermore, 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” because it produces less harm.4

Our own elaboration of benign displacement is that reduced harm could take several forms, such as 1) 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); 2) the transference of crime away from more vulnerable groups of the population (e.g., children and the elderly); and 3) the relocation of crime to places where the community impact is less harmful. This could take two forms, as follows: 1) 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 2) 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 that 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 that view crime behavior as a result of influences such as unemployment, subcultural values, strained economic opportunities, and so on, predict that blocking crime opportunities through situational alterations will inevitably lead offenders

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4. The first to note this was Brantingham (1986: 111), 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.”
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.5

Rational choice theory, in contrast, views crime 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 perspective does not view 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. Under this perspective, displacement will not 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, 1987).

The rational choice perspective, then, provides an explanation for both the presence and the 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 after a focused police crackdown on a prostitution market, some prostitutes gave up the trade altogether (see Brisgone, 2004). Similarly, Mathews (1986, 1990) found that many prostitutes engaged in the trade because it was an easy way to make money and gave up prostituting after street closures and a policing crackdown in Finsbury

5. 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 because offenders would be viewed as not possessing the capacity to make reasoned decisions as to when and where to offend to escape detection. However, this understanding may be overly reductionist because 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).
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Park, London, which seemed to offset the ratio among 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 helps to understand whether displacement will occur. This theory holds that displacement may occur in the aftermath of a situational intervention where there are other convergences of suitable targets, motivated offenders, and the absence of capable guardians (e.g., there is crime opportunity), and it will not occur where one or more of these elements is 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 criminologists, 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 that focused on understanding crime as opposed to criminality was at least partly energized by the notorious Martinson (1974) report, which harnessed the field’s thinking about crime reduction through rehabilitation. Recent studies suggest that crime opportunity is not constant but has been shown to cluster in time and place (Branntingham 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 effort (discouragement). Using these derivatives of the rational choice perspective gives explanation as to why diffusion has been observed in places near treatment areas.

PRIOR REVIEWS ASSESSING DISPLACEMENT AND DIFFUSION

The most encompassing type of displacement research includes literature reviews of empirical studies reporting on displacement; yet only three have been published (Barr and Pease, 1990; Eck, 1993; Hesseling, 1994),
and no published systematic reviews of diffusion of benefits have been published (Weisburd et al., 2006). The findings from each displacement review are largely consistent, reporting that displacement was often not observed, and in cases where it was observed, 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 studies (9 percent) reported a substantial amount. Similarly, Hesseling (1994) found that 40 percent of the 55 studies reviewed reported no displacement at all, and of these, 6 reported a diffusion of benefits. Finally, Barr and Pease (1990) take a different approach using a selective review of various crime topics and note that in some cases, even in the minority event of total displacement, a redistribution of crime still achieves a desirable social gain.

Despite these mostly consistent findings, the reviews of displacement research are limited in several ways. First, they have been based on a small number of studies available for review at the respective time. In the 14 years since the last review, many more studies have been produced, notably as a by-product of the increasing popularity the situational approach has garnered. Second, all the reviews have been descriptive in their method. This is mostly because of the lack of data provided by individual study authors, which allow 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 facie. Third, even if sufficient data were reported, the 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).

MEASURING CRIME DISPLACEMENT AND DIFFUSION

The primary difficulty in measuring spatial displacement and diffusion involves identifying suitable displacement areas, and within this, three aspects must be considered (for a thorough discussion of this, see Bowers and Johnson, 2003). The first involves the proximity of the displacement/diffusion catchment area in relation to the treatment area. Conceptually, displacement could occur proximally or distally from the intervention area, although proximal displacement seems more likely and is predicted by some crime theory. The second aspect to consider in selecting the displacement/diffusion catchment area is size (Weisburd and Green, 1995a).

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6. One caveat is that Hesseling (1994) did report on observed diffusion effects in 6 of the 55 studies reviewed as reported herein.
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The catchment area should not be too large or too small so that any relocation or reduction of crime is discernible. The third aspect involves the possible contamination of the displacement/diffusion catchment area (Weisburd and Green, 1995a), which could occur when catchment areas overlap with each other, with control areas, or with other treatment areas.

THE CURRENT STUDY

This study investigates the extent of crime displacement and involves a systematic review of 102 evaluations of situationally focused crime-prevention projects. It also assesses the frequency of diffusion of crime-control benefits among those studies. The studies reviewed were limited to those with a prominent or exclusive situational component, which could be classified using Cornish and Clarke’s (2003) 25 techniques. It seeks to contribute to the literature on displacement in several ways. First, it expands on previous reviews by updating and increasing the number of studies reviewed for assessing the extent to which displacement effects are observed. Second, it examines systematically the extent to which diffusion of crime-control benefits have been observed, something never before completed. Third, it applies a standardized statistical technique that allows for the determination of overall intervention effects while accommodating the effect of displacement and diffusion of crime-control benefits.

METHOD

SEARCH STRATEGY

To search for evaluation studies of SCP initiatives, the following thesaurus of terms was used: alley-gating, auto* theft, vehicle theft, car* theft, barricade*, breathalyzer*, burglary, cameras, closed circuit television or CCTV, crime prevention, deterrence, diffusion of benefit*, displacement, drug dealing, environmental design, fraud, graffiti, homicide, immo#lizer*ers, lighting or illumination, Lojack, neighbourhood watch or neighborhood watch, prostitution, publicity or mass media campaign, reduc*, robber*, security, shoplift*, street closures, surveillance, target hardening, theft, transportation, and vandalism. These terms were used in conjunction with: impact; evaluation; effectiveness; assessment; and case study.

These terms were used in searching the following databases: Criminal Justice Abstracts, National Criminal Justice Reference Service, Sociological Abstracts, Google, Google Scholar, CINCH (an Australian Database),

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7. At least not on a large scale (see footnote 6).
and Academic Search Premier. In addition, the most relevant crime-prevention journals were manually searched in their entirety. These included *Security Journal*, *Crime Prevention and Community Safety: An International Journal*, and all volumes of *Crime Prevention Studies*. Crime-prevention reports from the Home Office were reviewed, and the literature searches performed for each of the Center for Problem-Oriented Policing guides provided an additional resource for manual review.

Bibliographies and reviews of publications addressing specific crime-prevention techniques were also manually searched and included the following: Clark (2002a, 2002b); Cozens, Saville, and Hillier (2005); Deisman (2003); Farrington and Welsh (2002); Nicholson (1995); Nieto (1997); Poyner (1993); Smith, Clarke, and Pease (2002); and Welsh and Farrington (1999, 2002, 2003, 2004).

A similar search was performed for documents relating to the displacement of crime-prevention programs. This included a search of the bibliographies on the topic, as well as a search of *Criminal Justice Abstracts* and the *National Criminal Justice Reference Service* databases. For documents that reviewed the displacement literature, the following publications were particularly useful: Barr and Pease (1990), Bowers and Johnson (2003), Brantingham and Brantingham (2003), Clarke and Weisburd (1994), Eck (1993), Hesseling (1994), and Reppetto (1976). Both published and unpublished reports that were written in English were included. If unpublished reports were not readily available, then an effort was made to secure them from the individual authors or to borrow them from another library. From this search, several thousand potential hits were obtained. After inspecting abstracts of each of these to determine whether they entailed some assessment of a situational measure, 261 articles were physically retrieved.

INCLUSION AND CODING

Each of these 261 articles was then assessed and included in the analyses using the following conditions:

1. It was written in English.

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8. The exact number of articles identified in the preliminary search is not available. This is because much of the search entailed manually looking at the bibliographies of other reviews and manual perusal of publication series (such as crime-prevention studies and the others noted in the text). In manual inspections, electronic “hits” of possible relevant manuscripts were not produced nor were they recorded. Manual inspections of the listed publications series have some strengths for identifying suitable manuscripts over electronic searches alone because the searcher can read titles and abstracts of the articles carefully to determine relevance. Electronic searches may overlook relevant articles in which the listed search terms are not used by the study author(s).
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2. It was published as either a journal article, a government report, an organizational report, or a book (including book chapters).

3. The article reported an evaluation of a crime-prevention effort that was predominantly or exclusively a situational intervention. In some cases, the crime-prevention effort also included dispositional interventions, but the situational intervention(s) was predominant.

4. The situational techniques employed in the intervention could be classified using Cornish and Clarke’s (2003) listing of 25 SCP techniques.

5. Studies that involved targeted police tactics were not included (even though they could have been classified as “strengthening formal surveillance”). Studies that involved a prominent use of situational measures and involved targeted police efforts were included.

6. The evaluation used some quantitative measure of crime.

7. The article reported original research findings. Systematic reviews or other meta-analyses of prevention projects themselves were not included, even though articles that 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.

The net result of this assessment produced 206 studies, and each study was reviewed to identify those that examined or allowed the determination of displacement and diffusion effects. Of the 206, a sample of 102 studies were identified that met this criteria. Each of these 102 studies were then inspected independently by two reviewers to determine 1) what type(s) of displacement and diffusion were analyzed (e.g., temporal, spatial, target, tactical, offense, or perpetrator) and 2) whether any displacement or diffusion was observable or reported by the author(s). The assignment of whether displacement and/or diffusion of benefit occurred was primarily based on the study authors’ reporting, prima facie. In a few instances, the empirical evidence was consistent with displacement and diffusion effects; yet it was not noted by the study author. The findings from each reviewer were compared, and any differences in the coding were discussed by the two reviewers until a consensus was reached.

An additional subsample of 13 studies with 19 observations among them was identified, which allowed for a closer inspection of overall outcomes of the prevention project while taking into account displacement and diffusion effects. These studies were only analyzed for spatial displacement and diffusion and met the following conditions: 1) They presented raw crime counts or rates of crime, and 2) the research design used in the evaluation comprised at least three areas: a treatment, catchment (or buffer),
and a control. An overview of these 13 studies can be found in appendix A.

9. Because of the measurement issues surrounding catchment area selection discussed previously, each study was examined to determine whether the selection of catchment areas was consistent with the following three theoretical propositions identified in the collective displacement literature: 1) The buffer zone was near the treatment area, 2) the size of the buffer zone was logically proportionate to the size of the treatment area, and 3) the buffer zone was free of potential “displacement contamination” (e.g., the increase of crime as a result of a different treatment in a nearby area). Ideally, only those studies satisfying these three theoretical criteria would be used in the analytical inspection; however, most studies failed to meet all three criteria, and many failed to meet or satisfy even two of the three criteria. Because of the low subsample size, we included all of the 13 studies and the 19 observations regardless of whether they satisfied or violated the theoretical provisions for buffer selection.

10. The subsample of 13 studies varied from the other 89 in four ways. First, the 13 evaluations were more recent, and almost half (n = 6) were conducted since 1999. The other 89 evaluations were mostly carried out prior to that time, and 74 percent were conducted prior to 1999. Second, most (n = 9) of the subsample of 13 evaluations were carried out in the United Kingdom, three occurred in the United States, and one occurred in Australia. The other 89 evaluations were carried out somewhat proportionally in the United Kingdom (n = 39) and the United States (n = 32) with the remaining minority of studies dispersed less heavily among the countries of Australia (n = 6), Canada (n = 4), the Netherlands (n = 3), Germany (n = 2), Sweden (n = 2), South Africa (n = 1), and Jamaica (n = 1). Third, the subsample of 13 mostly evaluated situational measures that were implemented in residential settings (n = 9), with three in public ways and one in a transport environment. Although the other 89 also mostly evaluated situational measures within residential settings (n = 29), a substantial portion were also evaluated in transport areas (n = 20), public ways (n = 18), retail (n = 12), human service (n = 7), recreational (n = 4), and education (n = 3), among others. Fourth, most of the subsample of 13 evaluations assessed the outcomes of the situational interventions on property crimes (n = 8), whereas the other 89 were more dispersed among property crimes (n = 38), violent crimes (n = 27), and various crimes (n = 25), among others.

The subsample of 13 evaluations was also similar to the other 89 in several ways. First, both samples mostly evaluated situational crime-prevention techniques, which were designed to increase the risk for offenders (n = 15 for subsample of 13; n = 89 for larger sample), followed by situational interventions that increased the effort for offenders (n = 7 for subsample of 13; n = 52 for larger sample), and those that removed the reward for crime (n = 3 for subsample of 13; n = 19 for larger sample). The subsample of 13 evaluations did not examine any techniques that removed excuses or reduced provocations even though the larger sample did (n = 6 for removed excuses; n = 3 for reduced provocations). Second, and perhaps most importantly, the overall conclusions that were drawn by the study authors in each sample were consistent, with the majority conclusion being that the situational intervention was effective (n = 12 for subsample of 13; n = 68 for larger sample). Just one study of the smaller subsample concluded that the situational intervention was ineffective, whereas nine studies in the larger sample concluded the measures were ineffective. Within the larger sample, six studies
ANALYTICAL APPROACH

The studies were examined both descriptively and analytically. For the
descriptive review of the studies, all types of displacement were inspected.
The proportion of those studies examining the various types and the fre-
quency with which they were observed were computed. In addition to the
descriptive review, a series of quantitative measures were computed using
data derived from the subsample of 13 studies. In this analysis, the inspec-
tion was limited only to spatial displacement. This was done because spa-
tial displacement was the only type universally studied across the
subsample of 13 evaluations. These quantitative measures included the
computation of the gross effect (GE), net effect (NE), the total net effect
(TNE), and the weighted displacement quotient (WDQ), as well as its
constituent parts, which were developed by Bowers and Johnson (2003)
and extended by Eck and Johnson (see Clarke and Eck, 2005). The GE
and the NE are defined as

\[ \text{GE} = R_b - R_a \]  
\[ \text{NE} = \frac{R_b}{C_b} - \frac{R_a}{C_a} \]  

where \( R_a \) is the crime count in the treatment area post-intervention and \( R_b \)
is the crime count in the treatment area pre-intervention.

\[ \text{WDQ} = \frac{D_a/C_a - D_b/C_b}{R_a/C_a - R_b/C_b} \]  

where \( D_a \) is the crime count in the buffer area post-intervention and \( D_b \)
is the crime count in the buffer area pre-intervention. The WDQ can also be

also reported inconclusive findings, and another six reported mixed findings.
Third, the time periods of evaluation for both samples were about the same,
ranging from just a few weeks after implementation to several years. For both
samples, the average post-evaluation time period was around 1 year. Finally, the
types of research designs used for each sample were somewhat comparable. The
most common research design used in each sample was a before–after with com-
parisons (\( n = 11 \) for subsample of 13; \( n = 41 \) for larger sample). The next most
common was some form of a time-series design (\( n = 4 \) for subsample of 13; \( n = 37 \)
for larger sample). Each sample had just one study that used an experimental
design. In addition, the larger sample had 18 studies that used a before–after
design with no comparisons and five studies that used a cross-sectional design.
Note that some totals of categories reported above exceed the subsample \( n \)
because of a few studies that examined more than one measure in more than one
place or country.
broken down into separate measures of scheme success and scheme displacement/diffusion, like so:

\[ \text{Success Measure (WDQ denominator)} = \frac{R_a}{C_a} - \frac{R_b}{C_b} \]

\[ \text{Buffer Displacement Measure (WDQ numerator)} = \frac{D_a}{C_a} - \frac{D_b}{C_b} \]

Additionally, the overall impact of the project was determined using the TNE model, which is defined by the relationship

\[ \text{TNE} = \left[ \frac{R_b}{C_a} - \frac{R_a}{C_b} \right] + \left[ \frac{D_b}{C_a} - \frac{D_a}{C_b} \right] \]

(4)

The advantages of using the WDQ and the TNE are threefold.\(^{11}\) First, the

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\(^{11}\) We were interested in the meta-analytic methods often used in other systematic reviews but could not include such an analysis for the following reasons:

1. The level of heterogeneity of the sample (in terms of both outcome measures used and the contexts of the schemes) would make this inappropriate. As indicated, some figures in the studies were given as crime rates before and after action, and others were given as total numbers of crime. This means that the quantitative scales of the analysis in the different studies are not directly comparable. Pooling them would therefore not be meaningful. Furthermore, some looked at burglary levels and others at theft levels; the interventions undertaken were various in their nature, and the spatial and temporal contexts of the schemes were different. For these reasons, even a random-effects meta-analysis was not suitable. The WDQ measure used here has the advantage of being a relative measure that compares change in the action area with the control as a ratio independent of whether rate or volume is used.

2. Producing odds ratios for individual schemes was also considered. However, a unit of analysis problem still exists here. Unlike an offender rehabilitation scheme, for example, where an action and control group of known sizes are monitored to determine how many in each group reoffend, with situational crime-prevention schemes, outcome analysis involves the consideration of the number of crimes in an area as a whole—not to a series of individuals. It is, therefore, difficult to produce confidence intervals for individual schemes because the denominator (the ultimate sizes of the action and control groups) is often unknown. Farrington and Welsh (2002) propose a solution to this problem by using the number of crimes as the unit of analysis and, thus, produce an estimate of the standard error of each scheme. This method has subsequently been criticized as underestimating overdispersion in the data and, consequently, the standard error of the odds ratio. Subsequent work by Farrington and Welsh includes an overdispersion factor to control for this, but the discussion continues and no universally accepted solution has been found to date. Once again, it is also unclear how these calculations could be done using rates rather than counts.

3. A final complication that prevented the use of standard meta-analytical methods was the theoretical question of how to produce an odds ratio that takes into account displacement and/or diffusion as well as the scheme effect. For example, is it reasonable to use an additive model—or is some scaling required? We might find, for instance, that in an additive model, a large amount of diffusion of benefit might make schemes with only modest
WDQ allows for measurement of the relative size of displacement or diffusion of benefits, and significance testing can also be conducted with the WDQ. 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). Details on the use and interpretation of the GE, NE, WDQ, TNE, and Success and Buffer Displacement measures are provided in appendix B.

RESULTS

DESCRIPTIVE FINDINGS

Table 1 presents the distribution of examined and observed displacement and diffusion of benefit effects. Of the 206 evaluations of SCP measures, 102 specifically examined or presented data that allowed for the determination of displacement or diffusion effects. Because the concept of diffusion of benefits was not formally recognized until recently (Clarke and Weisburd, 1994), each of the two independent reviewers recorded whether evidence was consistent with diffusion or displacement effects among the 102 studies. This technique adjusted for the limited time frame that the term diffusion has been used. Relying on inspections of the data also allowed assignments to be made where the study authors had not attributed displacement or diffusion effects, even though they were apparent in the data presented.

Several studies examined multiple forms of displacement, such as spatial, offense, and target, for example, and many studies looked for each type of displacement in multiple instances (i.e., spatial displacement was looked for at two or three different areas outside the treatment zone or a study evaluated two or three SCP measures, each with its own observation of displacement). As a result, although there were 102 studies, there were 574 different types or instances of displacement studied. Of the 574 observations, the most common form of displacement and diffusion examined success look extremely effective. Whether we would want to show the combined effect like this is a debate in its own right; it assumes that all the decrease in crime within the buffer zone is caused by the action in the scheme area. On the flip side, we could find that displacement cancels out a positive scheme effect—or worse—makes it look ineffective or harmful. It would, of course, be possible to produce an odds ratio for displacement/diffusion zones alone for successful schemes, but this would not account for the magnitude of any success achieved. Here, we use a ratio (the WDQ) to compare the magnitude of success with displacement/diffusion, which by its sign makes it clear whether displacement or diffusion occurred and its magnitude, as well as whether this outweighed the treatment effect.
was spatial (47 percent) followed by offense (24 percent), target (14 percent), and tactical (9 percent). Temporal forms of displacement were examined in only 5 percent of the total examinations, and less than 1 percent of the examinations were for perpetrator displacement.

Table 1. Distribution of Displacement and Diffusion of Benefits (N = 574 Observations)

<table>
<thead>
<tr>
<th>Study Type</th>
<th>Examinations Frequency (%)</th>
<th>Displacement Observed Frequency (%)</th>
<th>Diffusion of Benefit Observed Frequency (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Spatial</td>
<td>272 (47)</td>
<td>62 (23)</td>
<td>100 (37)</td>
</tr>
<tr>
<td>Offense</td>
<td>140 (24)</td>
<td>36 (26)</td>
<td>22 (16)</td>
</tr>
<tr>
<td>Target</td>
<td>80 (14)</td>
<td>26 (33)</td>
<td>19 (24)</td>
</tr>
<tr>
<td>Tactical</td>
<td>49 (9)</td>
<td>11 (22)</td>
<td>6 (12)</td>
</tr>
<tr>
<td>Temporal</td>
<td>31 (5)</td>
<td>11 (36)</td>
<td>5 (16)</td>
</tr>
<tr>
<td>Perpetrator</td>
<td>2 (&lt;1)</td>
<td>1 (50)</td>
<td>1 (50)</td>
</tr>
<tr>
<td>Total Observations</td>
<td>574</td>
<td>147 (26)</td>
<td>153 (27)</td>
</tr>
</tbody>
</table>

* Column percentages are reported (e.g., percent of the overall number of observations, N = 574).

† Row percents are reported (e.g., percent of those observations of specific displacement/diffusion type).

Using the number of different examinations as the denominator (N = 574), aggregate displacement was observed 26 percent of the time or in about 1 of 4 instances.12 Excluding perpetrator displacement, which was examined in only two instances, displacement was consistently observed about one third of the time or less when disaggregated by displacement type. Temporal displacement was most commonly observed (36 percent), followed by target (33 percent), offense (26 percent), spatial (23 percent), and tactical (22 percent). Perpetrator displacement was observed 1 of the 2 times it was inspected (50 percent).

Overall, the diffusion of benefit was observed in 27 percent of the 574 observations. Here, excluding perpetrator diffusion, which was only examined twice, spatial diffusion was most commonly observed at 37 percent, which was observed more often than spatial displacement. Target diffusion was the next most common; it was observed in 24 percent of the examinations. This was followed by offense and temporal diffusion, both of which were observed in 16 percent of the examinations. Finally, tactical diffusion was the least common; it was observed in 12 percent of the observations.

12. No displacement was found in 68 studies, whereas diffusion of benefits was found in 39 of the 102 studies; when displacement occurred, it was never complete.
These descriptive findings provide a general indication of the extent to which the evidence was consistent with the possibility of displacement and diffusion within the reviewed evaluations of situational measures. They should, however, be treated with caution, because most of these studies lacked sufficient research designs to make confident determinations that the crime changes observed in the comparison areas were the result of the intervention (and, therefore, a displacement or diffusion effect). A few studies \( (n = 6) \) did not provide data to show evidence for their reporting of the presence of displacement or diffusion. Also, because the studies did not set out to examine displacement specifically, the designs used were not methodologically suited for detecting it (Weisburd et al., 2006: 556).

ANALYTICAL FINDINGS

From the 13 studies that allowed for more precise estimates of displacement and diffusion effects, 19 observations were used in the analyses. In some cases, more than one observation was used for a single study, because the study had more than one action area for which data had been provided or calculations had been made for different crime types. Within several studies, more than one choice of buffer zone or comparison area was found. In these cases, figures were derived that related to the mean, minimum, and maximum of the possible combinations of action, control, and buffer sites. This result provides an idea of the variation that can be found in outcomes if one control or buffer site is substituted for another. This was useful because it allowed an investigation of the effect of changing the control area or buffer zone on the outcome of the studies, in terms of both the success of the scheme and the level of displacement or diffusion. It is important to note that the studies are investigated in isolation of each other, and only descriptive statistics are given; hence, any dependency in the data should not be a problem. Dependency would obviously be more of an issue if formal statistics were to be produced en masse across all the studies, but because of the small number of cases available, such analyses would neither be reliable nor of sufficient power and are therefore not reported here.

Inevitably, the analytic findings are limited by the methods used in the individual evaluations. The subset of evaluations used here are those that included estimations of the counterfactual by use of a control area. However, because the research designs are quasi-experiment in nature, it is impossible to say with certainty that any measured changes have been caused exclusively by the schemes. For example, whereas some effort was nearly always made to match the control to the action area, it is possible that unmeasured differences were found between the areas that may have caused variation in their crime trajectories. For brevity, we present the
results without always spelling out these caveats below, but they do apply universally henceforth.

Treatment and Displacement/Diffusion Effects

Figure 2a summarizes the value of the Success measure across the 19 distinct observational units. Here, all but 3 of the 19 values were negative, which indicates “successful” schemes where the decrease in the action area outweighed that in the control area.\(^1\) Thus, in 84 percent of the observations, the intervention seemed to result in a crime reduction. Figure 2a also shows little variation in the Success measure when different control or buffer sites were chosen, as the maximum and minimum values were generally close to the mean. In no case did the choice of control/buffer area change the direction of the Success measure.

Figure 2. Treatment and Displacement/Diffusion Effects of Situational Interventions

(a) Success Measure

\(^1\) Note the caveat, however, that we cannot say for sure that the schemes caused the decrease in crime.
(b) Buffer Displacement Measure

Figure 2b shows variation in the Buffer Displacement measure that indicates which interventions showed possible evidence of displacement and which showed possible diffusion of benefit (again we cannot be certain that this was caused exclusively by the scheme). In this case, 8 of the 19 observational units had mean Buffer Displacement values that were positive (42 percent), thus suggesting displacement effects. Figure 2b also reveals that in two situations, choosing an alternative buffer or control area tipped the balance between possible displacement and diffusion of benefit.

Figure 3 shows the mean Buffer Displacement measure plotted against the mean Success measure for each of the 19 observational units. The top left shows cases where there was success but displacement ($n = 8$), the bottom left shows cases where there was success and diffusion ($n = 8$). In three cases, the scheme was not successful according to the Success measure. These cases are shown in figure 3 for completeness, but obviously it makes no sense to consider displacement or diffusion where there has been no scheme effect.
Figure 3. Buffer Displacement Measure Plotted Against Treatment Effects

WEIGHTED DISPLACEMENT QUOTIENT

The WDQ combines the Buffer Displacement measure and the Success measure to produce a measure that gives an overall impression of the degree to which the effect of displacement or diffusion of benefit outweighs, enhances, or negates the reductive effect of the scheme itself (again note the causality caveat). A WDQ number greater than 1 means that a diffusion of crime-control benefit effect amplified the treatment effect. Conversely, a WDQ number less than –1 means that the displacement effect eroded or took away from the treatment effect of the intervention. Between 1 and –1, the diffusion or displacement effects present are less than the overall effect of the scheme but still exist unless the WDQ is exactly zero. It is important to note here that the WDQ is only calculated where there has been some positive effect of the scheme according to the Success measure. Hence, it is only calculated here for the 16 observational units in which this was the case. We also excluded one outlier that skewed the overall mean to the negative, which left 15 observations.

Figure 4 shows a box plot of the mean WDQ with the extreme outlier removed. The mean WDQ across the studies was just positive with a value
of .83 indicating evidence of an overall diffusion of benefit effect among the situational interventions on average. We also examined the variation found in the WDQ as it was influenced by the variation in buffer and control combinations (see appendix C). In three cases, different buffer and control choices caused a change in the sign of the WDQ. In one case in particular (observation number 18, appendix C), a large amount of variation in the values was found for different combinations; this was the same outlier removed from figure 4.

**Figure 4. Box Plot Distribution of the Mean WDQ of Situational Interventions**

GROSS EFFECT, NET EFFECT, AND TOTAL NET EFFECT

Table 2 shows summary statistics for the GE, NE, and TNE of the 19 observational units. The statistics were calculated using different sample sizes reported across the studies. This is because some figures in the studies were given as crime rates before and after action and others were given as total numbers of crime. The NE measure is not sensitive to these differences, as it is a proportional measure of the NE in each case. However, the

14. With the extreme outlier included, the WDQ was −10.00, which is evidence of (at least some) displacement. This distribution is not shown here.
GE and the TNE estimate total numbers of crimes saved and therefore need raw numbers rather than rates as their input.

Table 2. Summary Statistics for GE, NE, AND TNE

<table>
<thead>
<tr>
<th></th>
<th>n</th>
<th>Minimum</th>
<th>Maximum</th>
<th>Sum</th>
<th>Mean</th>
<th>Standard Deviation</th>
</tr>
</thead>
<tbody>
<tr>
<td>GE mean</td>
<td>12</td>
<td>-13.00</td>
<td>245.00</td>
<td>920.50</td>
<td>76.71</td>
<td>75.00</td>
</tr>
<tr>
<td>NE mean</td>
<td>19</td>
<td>-</td>
<td>1.49</td>
<td>16.55</td>
<td>0.34</td>
<td>0.53</td>
</tr>
<tr>
<td>TNE mean</td>
<td>12</td>
<td>-436.86</td>
<td>490.98</td>
<td>1077.54</td>
<td>89.80</td>
<td>216.43</td>
</tr>
</tbody>
</table>

Reported in table 2, the GE of the schemes had an average of 77 and a sum of 921 (this again assumes that the change in crime is caused by the schemes and, hence, should be treated with caution). This finding indicates that purely charting the change in crime in the action area suggested an overall positive effect of the interventions. That is, the crime levels in the action areas decline by an average of 77 crimes each. In terms of the NE of the interventions, while accounting for changes in suitable control areas, we observe that the mean effect is slightly positive. In terms of the TNE, which also takes into account displacement or diffusion of benefit effects, the numbers suggest that there was an average saving of 90 crimes per observational unit, which makes a total of 1,078 crimes overall. Comparing this with the GE shows that accounting for displacement/diffusion seems to make the schemes slightly more effective at reducing crimes on average.15

Taken together, these analytical results suggest that across the 13 studies for which data were available, a tendency was observed for some reduction in the problem targeted. In 84 percent of the observational units analyzed, there was an apparent “positive effect” of the scheme, although the reductions shown were fairly modest. Notwithstanding the issue of causal inference, there was a potential savings of 77 crimes per scheme on average according to the GE mean figures. The data showed evidence of both displacement and diffusion of benefit, with equal numbers of observational units showing each of these phenomena. Overall, displacement and/or diffusion seemed to have little effect on scheme effectiveness/reduction when figures were considered in the aggregate.

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15. This is true for the subset of \( n = 12 \) for which the calculations were possible.
ISSUES RAISED BY STATISTICAL ANALYSIS

INVESTIGATION OF THE OUTLIER

One concern raised by the analysis above was the reason why an extreme outlier was found in the WDQ distribution. Subsequent inspection showed that this was from the study by Skinns (1998), which is detailed in appendix A. In this scheme, the action area was a town center, which made it particularly difficult to find a suitable buffer area. The options taken were a residential area that was adjacent to the scheme and other town centers at a distance of up to 15 miles away from the action site. The first option is problematic because it is unlikely that the residential area will be similar in terms of its opportunities to the action area, and the second because it is potentially too far away to be a viable candidate for local displacement. Overall, the treatment effect was small in comparison with the displacement effect. One explanation for the outlier is that it was a consequence of difficulties in finding an appropriate buffer zone.

CHOOSING BUFFER ZONES AND CONTROL AREAS

It is useful to investigate more generally what can be learned about suitable buffers from the findings presented here. In the Hollander et al. (1979, see also Hollander, 1980) study, in which two different buffers actually demonstrated a change in the sign of the Buffer Displacement measure, it was found that the buffer sites were in fact some of the most carefully selected of the studies investigated. They both were adjacent to the target area, and the crime counts suggested that they were of similar size to the action area. Both these features have been mentioned as desirable qualities of buffer zones in previous work (e.g., Bowers and Johnson, 2003). These results could in fact be demonstrating that displacement and diffusion can coexist. This suggests that an extra layer of complexity might be necessary in examining spin-offs or side effects of a scheme. A single scheme might diffuse benefit to one area and cause displacement to another. Additional research in this area is necessary.

The other study in which large variation occurred in terms of the Buffer Displacement measure was Sturgeon-Adams, Adamson, and Davidson (2005). In this case, the variation was caused by a change in the control area rather than the buffer site. Choosing a large control area (here an entire Police Basic Command Unit) possibility “diluted” the displacement effect. Displacement was found to be greater when a smaller, more tightly defined comparison area was used. This suggests that it is important also to consider the definition of control areas carefully.

One suggestion is to increase accuracy and comparability by defining a control buffer zone in addition to an action buffer zone. This strategy has been used in a limited number of studies (e.g., Weisburd and Green,
It could be argued that this would increase the reliability of the findings by introducing a counterfactual for the displacement zone. In other words, it would be accounting for the “history” of change in crime in the buffer zone, which could have occurred without implementation of the program by tracking changes in a similar buffer zone around the control area. In theory, this idea is plausible, but the advantage of using such a method over the approach taken here is debatable because the method used in this article does provide a counterfactual for the buffer zone; it is simply the control area for the action area. The assumption is that without implementation of a program, the change in the action buffer zone would be similar to the change in the action area itself. Hence, the control area appropriate for the action area would also be suitable for the buffer zone. This assumption is not unreasonable because the buffer zone and action area are adjacent and, hence, are likely to share similar characteristics. This approach has the advantage that data are only necessary for three areas, not four. The main drawback might be in terms of the morphology of the buffer zone; it is likely to be ring shaped because of the nature of its location and, hence, different than the physical structure of the control area. The increase in methodological accuracy of using a control buffer zone needs a more detailed investigation as additional studies that use this concept are published.

DISCUSSION

Because displacement and diffusion were observed somewhat proportionally (at least in the aggregate) among the studies examined, the findings support the notion of reasoning and rationality among offenders, which is a central issue in understanding criminality and is a primary assertion of the rational choice perspective. Had displacement been observed with greater frequency across the studies reviewed, it would have provided more support for the dispositional perspective of offending. But this depends on how one interprets offender dispositions particularly relating to what extent offenders exercise free will and, as such, are influenced by their environment.

The findings also provide continued support for the discontinuity of crime opportunities explained by routine activity theory. Were opportunities for crime infinitely available, then much more displacement would be expected because offenders could easily “move around the corner,” as it is said. Instead, the presence of crime prospects seems less common than was originally conceived by pioneering criminologists who viewed crime opportunities as too numerous, thereby making crime prevention through opportunity reduction impractical (see Clarke and Felson, 1993; Weisburd et al., 2006: 552).
In one of the only studies to assess displacement and diffusion directly in the aftermath of a focused policing initiative, Weisburd et al. (2006: 580) speculated that their observation of diffusion effects could have been the result of incapacitation, although later concluded that it was more likely to be a deterrent effect. The findings from this review add support for the deterrence hypothesis because the evaluations reviewed were predominantly situational and did not involve (at least to any significant extent) the arresting of offenders. As a result, it is unlikely that incapacitation through arrest could be responsible for the observed diffusion effects found in these reviewed studies.

Because most studies included in this review did not set out to examine displacement and diffusion effects specifically, it is not clear how well suited the research designs used were to detect its presence. With most attention focused on determining the impact of the situational interventions, and because only post hoc or peripheral assessments of displacement and diffusion were used, it is likely that either effect (displacement or diffusion) could have occurred and was not recognized (e.g., false negative), or that an effect was documented when it was not a true effect (e.g., false positive). The extent to which either is the case remains unknown.

Additionally, the study designs used to evaluate the situational measures mostly employed a quasi-experimental design, which means it is difficult to say with confidence that the interventions were responsible for the observed crime changes or any observed displacement or diffusion. Accurate determinations of displacement and diffusion are dependent on the evidential strength of the success of the initial crime-control intervention. For a variety of reasons, evaluations of situational interventions have relied on research methods more consistent with the scientific realism perspective than with conventional experimental research designs (for a discussion of this, see Guerette, 2009).

Only one study in this review that examined displacement/diffusion effects used a randomized control trial, and no studies incorporated a design that allowed for a comparative assessment of situational versus nonsituational causes of crime, such as hierarchical linear modeling. The greater use of such designs could strengthen our understanding of the effectiveness of situational interventions and the role that any displacement or diffusion effects might play because it could incorporate the influence of variables often associated with dispositional explanations of criminality (e.g., structural indicators of concentrated poverty, minority racial segregation, single males over 18 years of age, and so on).

Another limitation is the small proportion of studies that allowed for any in-depth analysis of displacement and diffusion effects. Of the 206 studies that met the initial criteria, only about half ($n = 102$, 50 percent) provided or allowed for some observation of displacement or diffusion. A
much smaller proportion ($n = 13$, 6 percent) used a research design that entailed both a buffer and a control area that allowed for the use of the WDQ and determinations of TNEs. It is possible, then, that some exclusionary bias could alter the findings if the other 50 percent of the 206 evaluations were included or if a greater proportion of the studies comprised a research design that would allow for computation of WDQ or TNE. However, given the convergence of these findings with others, it seems less likely that would be the case.

There is also reason for concern regarding the lack of theoretical guidance that was used in the selection of buffer areas among those 13 studies used in the analytical findings. It could be that the comparisons made biased the outcomes or simply made the comparisons unfair. In the spirit of preserving the sample size for the analytical findings, we decided to include all the 13 studies; however, it should be recognized how inappropriate buffer selection could have influenced the findings. For this reason, the analytical findings were presented with sensitivity to how the various observational units were distributed.

The proximity hypothesis of displacement is the outgrowth of theories on crime that remain just that, theories that are largely untested. The studies that used the more rigorous research designs mostly examined displacement and diffusion effects that were adjacent or nearby the intervention areas. Of the 13 studies used in the analytical findings, about 62 percent ($n = 8$) looked for displacement and diffusion in areas that were adjacent to the treatment zone(s), 23 percent ($n = 3$) looked for it at nearby places, and only 15 percent ($n = 2$) used a displacement/diffusion area that was some distance away. Of the last 2 studies, one study found displacement and the other found diffusion.

So although the findings here provide evidence against the displacement hypothesis concerning locations that are proximal to treatment areas, they tell us little about the possibility of displacement to more distant locations. Future research should not only make displacement and diffusion more central to evaluations of situational efforts but also should incorporate designs that examine for displacement and diffusion effects at faraway places. One way to do this would be to increase the use of offender tracking over time in the aftermath of interventions, similar to that used by Mathews (1986).

**CONCLUSION**

This study assessed the extent to which displacement and diffusion of benefits were observed among 102 evaluations of SCP initiatives. The findings provide continued support for the view that crime does not simply
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relocate in the aftermath of situational interventions. Instead, crime displacement seems to be the exception rather than the rule, and it is sometimes more likely that diffusion of crime-control benefit will occur. The findings also implicate that when displacement does occur, on average, it tends to be less than the gains achieved by the situational intervention, which means that the initiatives remained worthwhile.

The analyses were focused on evaluations that entailed primary or predominant situational interventions, but the results are consistent with previous reviews of displacement (Barr and Pease, 1990; Eck, 1993; Hesseling, 1994), as well as with findings from other recent evaluations that assessed displacement and diffusion of benefit effects among broader community development programs (McLennan and Whitworth, 2008; Roman et al., 2005). For instance, in an evaluation of the Weed and Seed program in Miami, Florida, Roman et al. (2005) found that the spatial diffusion of benefits was more common than spatial displacement using both autoregressive integrated moving average time-series analysis and the WDQ. In an evaluation of the New Deal for Communities program in the United Kingdom, McLennan and Whitworth (2008) discovered that among 383 buffer zones, spatial diffusion of benefit was observed in 23 percent, whereas spatial displacement was observed in only 2 percent. The remaining 75 percent showed signs of neither displacement nor diffusion. It was also discovered that offense diffusion across the buffer zones was more common than was offense displacement (between 21 to 25 percent of the zones revealed offense diffusion, compared with 0 to 5 percent for offense displacement).

The findings are also consistent with outcomes from evaluations of predominant police-focused initiatives (Braga, 2007; Braga et al., 1999; Weisburd et al., 2006). In a systematic review of the effects of hotspots policing on crime, Braga (2007: 16) found that of five studies that examined displacement and diffusion effects, none reported “substantial immediate spatial displacement of crime into areas surrounding the targeted locations,” whereas four studies suggested possible diffusion effects. In the aftermath of an intensive policing crackdown at street drug and prostitution markets, Weisburd et al. (2006) noted evidence of spatial diffusion of benefit effects across the two catchment areas studied. They also found diffusion of general social disorder in those areas. More recently, Braga and Bond (2008) presented an analysis of the impact of problem-oriented policing using 17 pairs of hotspots. One member of each pair was randomly assigned a problem-solving police strategy and the other acted as a control. An evaluation revealed significant reductions in crime and disorder calls for service, and critically, an analysis of two-block catchment areas around the matched pairs showed no evidence of significant crime displacement.
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This study provides some unique contributions beyond the consistencies with other research. First, because the study entailed a review, it synthesized findings from a large number of evaluations that were conducted in a variety of settings and across a variety of times, and it evaluated a variety of situational interventions. The evaluations of any one localized prevention effort are limited in their ability to be extrapolated to different times or to different places, or to tell us about the utility of other varieties of intervention techniques beyond those studied. Second, this research expands on the number of studies used in previous displacement reviews and represents the first systematic review of diffusion of benefit effects, at least that has been conducted to any large extent. Third, it retrospectively applied statistical techniques that allowed for determinations of overall situational treatment effects while taking into consideration the effects of displacement and diffusion.

Although these analyses give us a collective picture of the extent to which displacement and diffusion effects were observed in the evaluation of SCP initiatives overall, the findings should not be taken as an exact quantification of such effects. Indeed, many challenges remain for future research in this area, primarily because of the inherent complexity of fully measuring the movement of crime (Barnes, 1995; Barr and Pease, 1990), which requires more appropriate methodological designs. Still, when taken in context, this study offers support for the effectiveness of SCP while considering the influence of displacement and diffusion effects. Additionally, the findings add to the growing body of research evidence that erodes earlier suppositions about the inevitability of crime displacement. It also demonstrates the value of increasing the accuracy and transparency of published evaluations of individual crime-prevention schemes and their associated levels of displacement or diffusion of benefit.

REFERENCES


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### Appendix A. Summary of Subsample Studies Used in Analytical Findings

<table>
<thead>
<tr>
<th>Author</th>
<th>Date</th>
<th>Location</th>
<th>Environment</th>
<th>SCP Techniques</th>
<th>Data Source</th>
<th>Research Design</th>
<th>Time Period</th>
<th>Outcome Measures</th>
<th>Conclusion</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allatt</td>
<td>1984</td>
<td>Northumbria, UK</td>
<td>Residential</td>
<td>Control access</td>
<td>Police records; survey</td>
<td>Time series; before–after with comparison</td>
<td>5-year trend; 1 year post</td>
<td>Attempted and completed burglary</td>
<td>Effective</td>
</tr>
<tr>
<td>Bennett</td>
<td>1988</td>
<td>London, UK</td>
<td>Residential</td>
<td>Extend guardianship; target harden; strengthen formal surveillance</td>
<td>Survey</td>
<td>Before–after with comparison</td>
<td>12-month period</td>
<td>Various</td>
<td>Not effective</td>
</tr>
<tr>
<td>Bowers, Johnson, and Hirschfield</td>
<td>2003</td>
<td>Liverpool, UK</td>
<td>Residential</td>
<td>Target harden; identify property</td>
<td>Police records</td>
<td>Before–after with comparison</td>
<td>2 years pre; 2 years post</td>
<td>Burglary</td>
<td>Effective</td>
</tr>
<tr>
<td>Cummings</td>
<td>2005</td>
<td>Bentley and Morley, AUS</td>
<td>Residential</td>
<td>Target harden; extend guardianship</td>
<td>Interview; police records</td>
<td>Time series; before–after with comparison</td>
<td>12-month period</td>
<td>Burglary</td>
<td>Effective</td>
</tr>
<tr>
<td>Ekblom et al.</td>
<td>1996</td>
<td>United Kingdom</td>
<td>Residential</td>
<td>Target harden; extend guardianship; identify property; assist natural surveillance</td>
<td>Police records; survey</td>
<td>Before–after with comparison; time series</td>
<td>6-year period</td>
<td>Burglary</td>
<td>Effective</td>
</tr>
<tr>
<td>Hollander et al.</td>
<td>1979</td>
<td>Hartford, CT</td>
<td>Residential</td>
<td>Extend guardianship; strengthen formal surveillance; deflect offenders</td>
<td>Police records; surveys; observations</td>
<td>Time series with comparison</td>
<td>5-year period</td>
<td>Burglary; theft; fear of crime</td>
<td>Effective</td>
</tr>
<tr>
<td>Laycock and Austin</td>
<td>1992</td>
<td>Basingstoke, England, UK</td>
<td>Transport</td>
<td>Strengthen formal surveillance</td>
<td>Police records</td>
<td>Before–after with comparisons</td>
<td>12 months pre; 12 months post</td>
<td>Theft of and from cars</td>
<td>Effective</td>
</tr>
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### Appendix A. Summary of Subsample Studies Used in Analytical Findings (continued)

<table>
<thead>
<tr>
<th>Author</th>
<th>Date</th>
<th>Location</th>
<th>Environment</th>
<th>SCP Techniques</th>
<th>Data Source</th>
<th>Research Design</th>
<th>Time Period</th>
<th>Outcome Measures</th>
<th>Conclusion</th>
</tr>
</thead>
<tbody>
<tr>
<td>Madensen and Skubak</td>
<td>2005</td>
<td>Cincinnati, OH</td>
<td>Residential</td>
<td>Extend guardianship</td>
<td>Police records</td>
<td>Before–after with comparison</td>
<td>13 weeks pre; 13 weeks post</td>
<td>Theft; victimization</td>
<td>Effective</td>
</tr>
<tr>
<td>Painter and Farrington</td>
<td>1999</td>
<td>Stoke-on-Trent, UK</td>
<td>Residential; Public ways</td>
<td>Assist natural surveillance</td>
<td>Survey</td>
<td>Before–after with comparison</td>
<td>12 months pre; 12 months post</td>
<td>Victimization; perceptions; fear</td>
<td>Effective</td>
</tr>
<tr>
<td>Sarno, Hough, and Bulos</td>
<td>1999</td>
<td>London, UK</td>
<td>Public ways</td>
<td>Assist natural surveillance</td>
<td>Police records; survey; interviews</td>
<td>Before–after with comparison</td>
<td>2 years before and 2 years after</td>
<td>Various</td>
<td>Effective</td>
</tr>
<tr>
<td>Skinnns</td>
<td>1998</td>
<td>Redton, UK</td>
<td>Public ways</td>
<td>Strengthen formal surveillance</td>
<td>Police records; surveys; interviews</td>
<td>Before–after with comparison</td>
<td>1 year pre; 1 year post</td>
<td>Various</td>
<td>Effective</td>
</tr>
<tr>
<td>Sturgeon-Adams, Adamson, and Davidson</td>
<td>2005</td>
<td>Hartlepool, UK</td>
<td>Residential</td>
<td>Target harden; Extend guardianship; identify property</td>
<td>Police records; survey</td>
<td>Before–after with comparison</td>
<td>2 years pre; 4 years post</td>
<td>Burglary</td>
<td>Effective</td>
</tr>
<tr>
<td>Weisburd and Green</td>
<td>1995b</td>
<td>Jersey City, NJ</td>
<td>Public ways</td>
<td>Strengthen formal surveillance; extend guardianship</td>
<td>Police records</td>
<td>Before–after with controls</td>
<td>7 months pre; 7 months post</td>
<td>Various</td>
<td>Effective</td>
</tr>
</tbody>
</table>
## Appendix B. Use and Interpretation of Coefficients Used in Analytical Findings

<table>
<thead>
<tr>
<th>Coefficient</th>
<th>Use</th>
<th>Interpretation</th>
</tr>
</thead>
<tbody>
<tr>
<td>GE</td>
<td>Determines increase or decrease in treatment area.</td>
<td>Positive number indicates decrease in crime. Negative number indicates increase in crime. Zero means there was no change.</td>
</tr>
<tr>
<td>NE</td>
<td>Determines increase or decrease in treatment area in relation to changes in control area.</td>
<td>Positive number indicates decrease in crime. Negative number indicates increase in crime. Zero means there was no change.</td>
</tr>
<tr>
<td>WDQ</td>
<td>Determines the extent of displacement or diffusion in buffer areas in relation to changes in treatment and control area.</td>
<td>Positive number indicates there was a diffusion effect and any treatment effects were amplified; if number is greater than positive one, then the diffusion effect was greater than the treatment effect. Negative number indicates there was displacement. A negative number between zero and negative one means that the displacement was not greater than the treatment effects, and the intervention still achieved some benefit. A negative number beyond negative one means the treatment effect was eclipsed or erased by displacement. Zero means there was no effect.</td>
</tr>
<tr>
<td>Success measure</td>
<td>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).</td>
<td>Negative number indicates successful schemes where the decrease in the action area outweighed that in the control area. Positive number indicates schemes where the treatment was not effective.</td>
</tr>
<tr>
<td>Buffer Displacement measure</td>
<td>Determines whether the interventions show possible evidence of displacement or diffusion.</td>
<td>Positive number indicates a possible displacement effect. Negative number indicates a possible diffusion of benefit.</td>
</tr>
<tr>
<td>TNE</td>
<td>Determines the overall effect of the intervention in relation to changes in the control area while adjusting for displacement and/or diffusion effects.</td>
<td>Positive number indicates the intervention was effective overall. Negative number indicates that it was not. Zero means there was no change. The greater the number, either positive or negative, the more or less effective the intervention, respectively.</td>
</tr>
</tbody>
</table>
Appendix C. Variations in the WDQ across Control/Buffer Selections