
MEASURING IMMEDIATE SPATIAL DISPLACEMENT: METHODOLOGICAL ISSUES AND PROBLEMS

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Abstract: *While much attention has been paid to the idea of displacement in crime place theory and research, methodological problems associated with its measurement have often been overlooked. We focus on such issues in the context of immediate spatial displacement around hot spots of crime. Using the Minneapolis Hot Spots Experiment (Sherman and Weisburd, 1995) as an example, we identify specific problems investigators are likely to face in documenting displacement effects. We argue that conventional studies are unlikely to provide a powerful research design for examining displacement, in part because of efforts to maximize the identification of main program effects. In conclusion, we suggest that studies specifically designed for measuring displacement (and the related phenomenon of diffusion) must be developed if criminologists are to make significant advances in this area.*

INTRODUCTION

The development of situational and environmental study in criminology has long been impeded by the idea of displacement. At least from the time of Sutherland (1947), the view held by most criminologists has been that opportunities for criminality found in specific places or situations are a minor feature of the crime equation.¹ Criminal opportunities have been assumed to exist in almost unlimited quantities, and offenders themselves

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are often portrayed as driven to crime and thus unlikely to be easily deterred by simply reducing the opportunity structure for offending (Clarke, 1992). Simply put, the prevailing orthodoxy in criminology for much of this century has been that there is little point to concentrating on opportunities for crime if offenders will simply displace the places, times or targets of their offending (e.g., see Reppetto, 1976).

In recent years this prevailing orthodoxy, like many others in criminology, has been the subject of substantial criticism. The idea that criminal opportunities are widely spread through urban areas has been challenged by a series of studies showing that crime is concentrated in time and space (Brantingham and Brantingham, 1981; Bevon, 1984; Sherman et al., 1989; Weisburd et al., 1992; Weisburd and Green, 1994). The portrait of offenders as driven to criminality has also been replaced in good measure by one that recognizes the situational, often serendipitous, character of much offending (Cornish and Clarke, 1986). Even the assumption that displacement is a routine occurrence of focused crime prevention efforts has been replaced by agreement that displacement is seldom total and often inconsequential (Gabor, 1990; Barr and Pease, 1990; Clarke, 1992; Eck, 1993; Hesselning, 1993). Clarke and Weisburd (1994), moreover, suggest that scholars need to take cognizance of just the reverse of displacement. They point to evidence indicating that situational and place-oriented crime prevention strategies often lead to a "diffusion of benefits" to areas outside the immediate targets of intervention.

While much attention has been paid to the idea of displacement, methodological problems associated with its measurement have often been overlooked.² This was to some extent understandable when it was assumed that there would be little overall crime control benefit from situational or environmental interventions, and relatively few academic studies that focused on crime places or situations. But given the substantial growth of such studies in recent years and the growing controversy over the magnitude and nature of displacement, such focus is now warranted.

Our discussion below focuses on methodological issues in the measurement of immediate spatial displacement around hot spots of crime. Using the Minneapolis Hot Spots Experiment (Sherman and Weisburd, 1995) as an example, we identify specific problems investigators are likely to face in documenting displacement effects. We argue that conventional studies are unlikely to provide a powerful research design for examining displacement, in part because of efforts to maximize the identification of main program effects. In conclusion, we suggest that studies specifically

designed for measuring displacement (and diffusion) must be developed if criminologists are to make significant advances in this area.

THE MINNEAPOLIS HOT SPOTS EXPERIMENT

The Minneapolis Hot Spots Experiment was designed to test the deterrent effects of uniformed police patrol (in marked cars) directed at hot spots of crime (Sherman and Weisburd, 1992: 1995). Following upon evidence that there is a substantial clustering of crime events at particular addresses in urban areas (see Sherman et al., 1989), Sherman and Weisburd sought to examine whether a patrol strategy that focused on crime hot spots would have a higher likelihood of success than the more general approach used in prior studies of preventive patrol (e.g., see Kelling et al., 1974). There appeared to be general agreement that preventive police patrol applied across large areas of a city does not do much to deter crime (Gottfredson and Hirschi, 1990; Sherman, 1993). The question raised in the Minneapolis study was whether such patrol focused on crime hot spots is likely to have a more significant impact.

While earlier studies had pointed to the concentration of crime (often measured through emergency calls for service) at discrete street addresses, they failed to take into account that there is a significant degree of clustering of hot-spot addresses one around another. This problem was of particular concern in the Minneapolis study because the crime hot spots identified would be randomly allocated to a treatment group receiving significant additional patrol, and a control group that retained "normal" police service. To randomly allocate addresses immediately adjacent to each other to separate treatment and control conditions would have effectively sabotaged the integrity of the treatments administered. Accordingly, Sherman and Weisburd (1995) used computer mapping techniques to map the location of addresses in which there was a minimum threshold of three or more emergency calls for service for "hard crimes," primarily more serious predatory offenses (see Weisburd et al., 1992), during a one-year period so that they could cluster them into crime hot spots.

In constructing crime hot spots from the 5,538 addresses that met this criteria, Sherman and Weisburd (1995) sought to identify clusters of addresses with frequent crime call activity. The boundaries of each hot spot were limited conceptually as being visible from an "epicenter" (Sherman et al., 1989). In practice the hot spots were, with a few exceptions, limited to one linear block and did not extend more than a half block from either side of an intersection. Some 420 such hot-spot clusters were

identified that met a threshold of 20 or more hard crime calls over the previous year.

A number of these places were excluded after further investigation, and a number of the hot-spot boundaries redrawn. A primary reason for exclusion was the assessment that places would not likely be influenced by increased patrol presence. For example, Sherman and Weisburd (1995) excluded parks, indoor parking garages and indoor malls. In order to maximize the statistical stability of estimates in the study, places that evidenced a great deal of instability in the number of crime call events year to year were also excluded. This exclusion was based on the statistical logic that such variability would make it difficult to identify treatment effects as opposed to natural variation in the number of emergency calls linked to a particular hot spot.

Finally, in choosing the final 110 places for inclusion in the randomized experiment, Sherman and Weisburd (1995) sought to identify the highest activity places in which there was sufficient distance one to another as to prevent treatment contamination of control locations. This meant in practice that the hot spots were more than one block away from one another, and most often not visible one from another.

The 110 hot spots were randomly allocated to control and treatment groups. The treatment hot spots were to receive "extra patrol," defined at the outset as an increase of two or three times over the dosage that would be present in normal police patrol of high-crime areas in Minneapolis. The goal was to provide three hours per day of *intermittent* patrol presence between 11:00 a.m. and 3:00 a.m., the highest crime period. It was expected that control hot spots would likely receive less attention than they would under non-experimental conditions because of the concentration of police efforts in bringing police cars to the treatment locations. Nonetheless, the fact that all of these locations were likely to generate a good deal of emergency call activity meant that some degree of preventive patrol would continue in the control areas as well.

In practice, the Minneapolis Police Department did provide the sought-after extra dosage to the treatment hot spots during the first six and one half months of the study. Extra patrol declined significantly during the remaining period of the study because of unexpected demands on police service. Sherman and Weisburd (1995) report a significant decline in calls for service during the first phase of the study, though as was expected this effect did not continue when the dosage of police patrol declined.³ The level of decline was of moderate size, with a 19% reduction in total crime

calls in the experimental hot spots as compared with a 6% reduction in the control locations.

Sherman and Weisburd (1995) argue that their main task was to identify whether hot-spots patrol could deter crime calls at the treatment (as contrasted with control) locations. Given the prevailing skepticism about the effectiveness of preventive patrol, they placed the emphasis of their research design and evaluation efforts on detection of such impacts if they existed. At the same time, once it was established that such patrol did affect crime calls in the study locations, it was natural to ask whether it had displaced crime calls to other locations.

This concern was commonly stated by police in Minneapolis, who often resented the interference of the study design in patrol activities. While there was relatively little subversion of the study, it was common for police officers to remark that crime was "just moving around the corner." This argument, of course, also served the function of providing a reasonable voice to a basic patrol officer complaint about the study—that it limited their freedom in choosing where to patrol. Nonetheless, it raised what is clearly an important question, once Sherman and Weisburd (1995) had established a treatment impact: Does the very concentrated character of hot-spots patrol result in a simple shift of crime activities to adjacent places? We sought to answer this basic question in the context of an analysis of calls for service in areas surrounding the hot spots examined by Sherman and Weisburd. However, as we illustrate below, the design of a study that seeks to maximize evaluation of direct treatment effects may hinder assessment of displacement effects.

PROBLEMS IN MEASURING SPATIAL DISPLACEMENT

Our first task in measuring immediate spatial displacement was to identify which sites to include in our analyses. Though overall there was a significant decline in crime calls in the experimental as compared to the control sites in Minneapolis, when we examined each of the sites as independent units we did not find a constant change across sites. As would be expected, there was a good deal of variability within the treatment group in terms of the overall mean changes in crime calls recorded. In some experimental sites there was little change, in others a very strong deterrent impact, and in a few others a "backfire" or increase in crime relative to the control sites as a whole. In our view it did not make sense to examine displacement in treatment locations that did not show a deterrent effect.

Accordingly, we chose not to examine all of the 55 treatment hot spots, but only those in which there was a significant decline in crime calls during the experimental period as compared with the pre-experimental year

(using a 5% significance threshold). There were 13 such hot spots in the experimental group. Importantly, there were also seven hot spots in the control group that met this threshold. For comparative purposes these were examined as well.

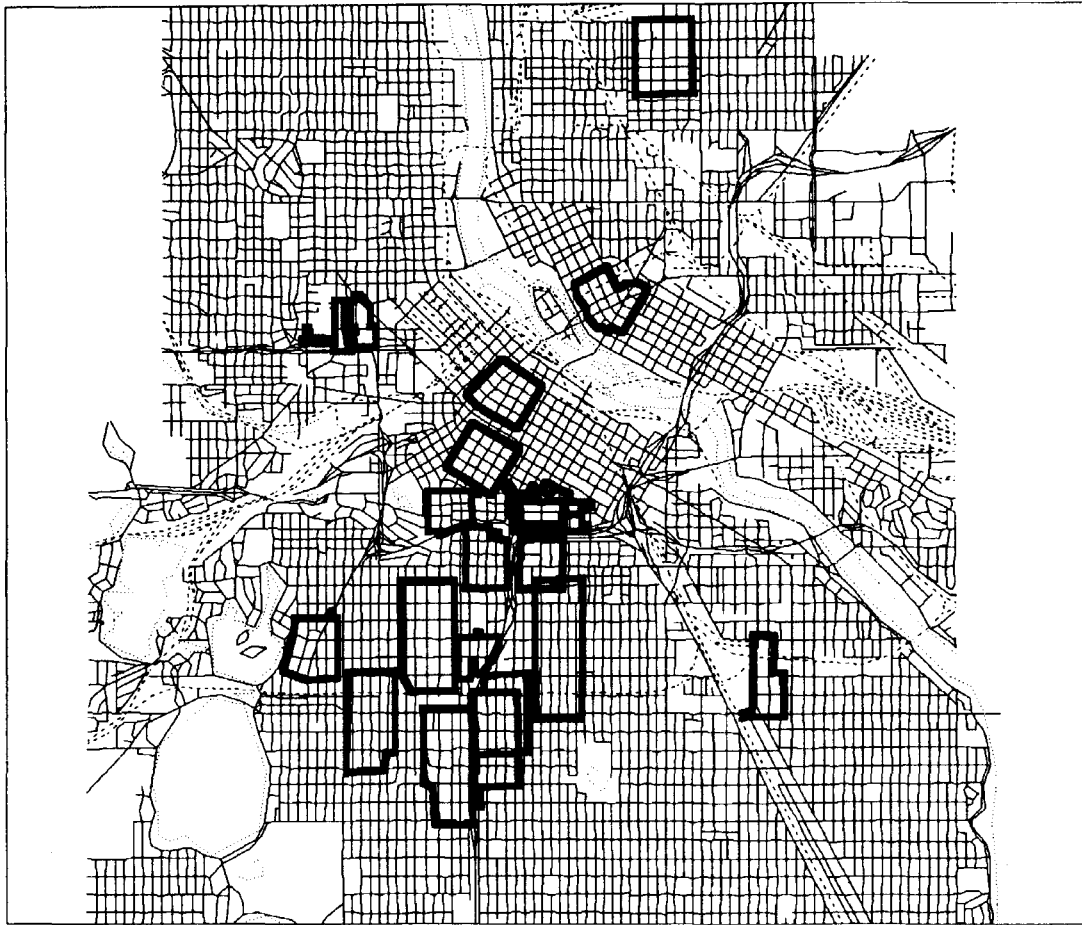
Having chosen these specific sites for analysis, the next concern was to establish a displacement area around them. We decided upon a two-block radius for the "catchment" area because we felt it a reasonable compromise between competing problems of a washout of displacement impact and a failure to provide adequate distance to identify immediate spatial displacement. While we recognized at the outset that we would miss the movement of crime more than two blocks away from a hot spot, given our measure of crime as a general rather than specific indicator we did not think it practical to identify all the potential places that might provide opportunity for displaced offenders. Such a strategy would have been more appropriate in the case, for example, of drug markets (e.g., see Green, 1995).

But even taking this compromise approach, we were faced with some immediate difficulties. While Sherman and Weisburd (1995) designed their study with the goal of reducing *treatment contamination*, they did not focus at the outset on potential *displacement contamination*. Their priority in choosing locations was based on ensuring the integrity of the experiment and not in simplifying measurement of displacement, which would only become a concern once it had been established that hot spots patrol could indeed reduce crime calls in the treatment areas. Sherman and Weisburd thus paid a good deal of attention to ensuring that there was enough distance between study sites to minimize overlap in the application of treatment. But it would have seriously reduced the number of high activity hot spots they could have included in the study if they had required a two-block free area around every hot spot examined.

This point is well-illustrated when we examine the overlap of the displacement catchment areas for the 20 hot spots that showed a statistically significant decline (see Figure 1). Nine of them overlap to some degree, and in some cases the catchment areas of multiple hot spots are found linked to one another. The problem here is not just catchment areas, but also overlap of catchment areas over the hot spots themselves. Accordingly, even taking this relatively conservative approach of only a two-block catchment threshold, one is likely to cover a fairly broad area. We think that this problem is not unique to Sherman and Weisburd's (1995) study, and is likely to plague most hot-spots investigations that are pursued in urban areas.

This problem also illustrates the fact that there is often a conflict between models for evaluation of the primary impacts of a study and

Figure 1: Minneapolis Hot Spots



methods for measuring displacement. The question the investigator must ask is: Is it reasonable to potentially weaken the study design for measuring direct effects, with the purpose of facilitating evaluation of displacement? The problem is that many studies fail to show any program impact, and therefore the investigator is understandably hesitant to do anything that might weaken his or her basic study design.

A second problem that develops from the choice of a two-block displacement area is less apparent but illustrates a more general difficulty in measuring the movement of crime after an intervention. Despite the fact that recent research on the distribution of crime emphasizes that even in "bad" neighborhoods many areas are relatively free of crime (e.g., see Weisburd and Green, 1994), it is quite common for high activity locations to be clustered within such neighborhoods. Using even a relatively small catchment area, the investigator is likely to encounter a large number of events near to treated locations. While a very large program effect would offset this problem somewhat, it is useful to keep in mind that relatively few criminal justice studies show even a moderate impact on crime (see Farrington, 1983; Weisburd, 1993).

The absolute number of calls that account for change in each Minneapolis hot spot year-to-year is small relative to the total number of crime calls in the displacement catchment areas (see Table 1). A change, for example, of 49 crime calls in hot spot D3 leads to a finding of a significant change year-to-year within the hot spot itself, but it is unlikely to amount to very much when it is spread to a surrounding displacement catchment area that includes 1,116 crime calls in a year. Accordingly, despite the fact that we limited the displacement area to two blocks, the number of crime calls in the surrounding blocks make it relatively difficult to define any changes there as resulting from a displacement of offenders or their crimes. Obviously this problem would have been more severe had we chosen to track displacement in a larger area. More generally, Barr and Pease (1990) suggest that the "wider the scope" of a study in terms "of types of crimes and places," "the thinner the patina of displaced crime could be spread across them; thus disappearing into the realm of measurement error" (pp. 23-24).

More recent interest in the question of diffusion of benefits (see Clarke and Weisburd, 1994) complicates the problem of assessing immediate spatial displacement even further. Clarke and Weisburd (1994) define diffusion of benefits, in part, as "the spread of the beneficial influence of an intervention beyond the places which are directly targeted" (p. 169). If, as Clarke and Weisburd suspect, it is often the case that areas immediate to an intervention gain a crime control benefit from it, then measures of displacement will be confounded.⁴ For example, if there is a diffusion-of-

**Table 1: Change in Hot-Spot Call Totals and
N of Calls in Displacement Areas**

Hot Spot	Group	Hot-Spot Data		Displacement Data
		N Change	% Change	Base N
D1	exp	18	-39	1611
D2	exp	33	-19	1259
D3	exp	49	-13	1116
D4	exp	30	-12	1032
D5	exp	64	-26	832
D6	exp	70	-29	2598
D7	exp	23	-18	1089
D8	exp	38	-22	1516
D9	exp	23	-17	884
D10	exp	47	-30	674
D11	exp	21	-19	497
D12	exp	20	-23	789
D13	exp	17	-23	2425
D14	con	26	-34	683
D15	con	19	-34	654
D16	con	99	-34	1085
D17	con	57	-34	3013
D18	con	14	-25	2107
D19	con	36	-25	514
D20	con	31	-13	1819

benefits effect and a displacement effect of equal measure, then we would observe no change in the displacement catchment area. While in some sense this would indicate no displacement, in another it masks the

complexity of two competing processes that cancel each other out (Green, 1995).

IMPLICATIONS

Our examination of data from the Minneapolis Hot Spots Experiment leads us to two basic conclusions. The first is that one must be extraordinarily cautious in analyzing and interpreting data on immediate spatial displacement. Statistics that appear solid on paper may reflect the difficulties of analyzing this process as much as any real substantive findings. In particular, we are concerned about the potential biases here toward the null hypothesis of no displacement in hot-spot studies.

Our second conclusion is that real progress in the study of displacement and the related phenomenon of diffusion cannot be made until we place these problems as central rather than secondary issues of study. For the most part, scholars have encountered the problem of displacement only after they have established a direct program impact. The approach to its study has been primarily defensive, a method for establishing that a gain was really made. The Minneapolis Hot Spots Experiment illustrates what we think is a more general problem in such analyses: there is often a tension between research design for measuring direct and displacement effects.

Resources are generally scarce in criminal justice research. It makes good sense to invest such resources first in establishing a main effect. And, indeed, displacement is potentially so diffuse that the very expensive process of developing good data on it could in this regard overwhelm even a large study (Barr and Pease, 1990). That displacement is of interest only if a main effect is established naturally pushes it to the back burner until resources have been spent on other, more pressing, research problems.

In order to develop a better understanding of displacement and diffusion, however, studies must be initiated that focus specifically on these phenomena. Such studies could be designed to define how far and under what circumstances crime will be displaced. They could at the outset identify problems and places that provide sufficient numbers of cases in target and catchment areas for statistically powerful analyses. Because they would not be constrained by focus on direct effects, they could also design out problems of displacement contamination and differentiate potential displacement and diffusion impacts. Direct program impacts would not be problematic in such studies. Accordingly, investigators would examine displacement in a context in which it is already clear that there is an effect that is possible to displace or diffuse. Their efforts would

be invested in tracking and understanding the nature of that process and not in establishing the efficacy of interventions themselves.

Our point is really a simple one. It is time to move displacement from a secondary to a primary focus of criminological study. Only then will we be able to gain reliable estimates of displacement effects on crime prevention initiatives.



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NOTES

1. Sutherland (1947) for example, recognized that specific circumstances are needed for a crime to occur. Nevertheless, while arguing that the situation does impact crime in many ways, he notes that "the least important [of these] is the provision of opportunity for a criminal act" (p.5). For Sutherland, the task of the criminologist is not to focus on the problem of opportunity itself but on why certain people and not others take advantage of such situational opportunities for crime.
2. For important exceptions to this trend, see Barr and Pease, (1990) and Pease (1993).
3. It is interesting to note that the impacts of the treatment did not dissipate immediately following the implementation breakdown in the study. Sherman and Weisburd (1992) report a degree of residual deterrence.
4. Eck (1993) suggests that the proximity of sites in many evaluation designs is likely to lead to a bias toward finding crime reduction impacts. In this case, crime may be displaced from the treatment to control location, thus exaggerating the impact of the intervention.

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