

# Hot Spots Policing Experiments and Criminal Justice Research: Lessons from the Field

By  
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In this article, more general lessons are drawn from two randomized experiments in hot spots policing that the author helped design and implement in the 1990s: the Minneapolis Hot Spots Experiment and the Jersey City Drug Market Analysis Experiment. Using a case study approach, factors that facilitate and inhibit development and implementation of randomized trials are identified with particular focus on the special problems and/or advantages of place-based experiments. While the author's main comments focus on the success of place-based randomized trials in evaluating hot spots policing approaches, he draws insight as well into the reasons why the successful example of experiments in hot spots policing has not inspired similar place-based experimentation in other areas of policing or criminal justice. Eight specific lessons regarding the implementation and development of place-based randomized trials and experimental methods more generally are identified.

*Keywords:* hot spots policing; experimental criminology; place-based experiments; policing; crime at place; cluster randomized trials

## Introduction

There is widespread acceptance today that randomized experiments provide more valid answers to policy questions than do non-experimental studies (e.g., see Boruch, Snyder, and DeMoya 2000; Campbell and Boruch 1975; Cook and Campbell 1979; Farrington 1983;

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Feder, Jolin, and Feyerherm 2000; Shadish, Cook, and Campbell 2002; Weisburd 2003). As Feder and Boruch (2000, 292) wrote, "There is little disagreement that experiments provide a superior method for assessing the effectiveness of a given intervention." Nonetheless, experiments remain the oddity rather than the norm in evaluations of criminal justice practice. Comparing criminal justice to medicine, Jonathan Shepherd (2003) described a "comparative famine" of randomized trials. And Garner and Visser (2003) and Nuttall (2003) documented the failure of the major American and British criminal justice funding agencies to concern themselves with randomized experimental approaches to program or treatment evaluations.

The marginal status of criminal justice experimentation is all the more remarkable given the growing evidence that experiments can be carried out in a number of different criminal justice settings (Boruch, Snyder, and DeMoya 2000; Dennis 1988; Petrosino 1988; Weisburd 1993). While the number of randomized experiments in criminal justice is still very small compared to the hundreds of thousands of randomized trials in medical research, researchers have documented as many as three hundred randomized studies that are relevant to criminal justice problems (Petrosino 2000). It is just no longer possible to argue that randomized experiments cannot be carried out in criminal justice. Indeed, randomized experiments have been conducted regarding the full range of criminal justice institutions and across a wide array of criminal justice subjects.

What then explains the "famine" of criminal justice experimentation and the failure of randomized experiments to move into the mainstream of criminal justice evaluations? One common explanation for the failure to widely implement experimental study is that randomization presents serious ethical problems that are difficult to overcome in most areas of criminal justice practice (Clarke and Cornish 1972). Even if experiments can overcome ethical barriers, it is often noted that experiments are very difficult to implement in crime and justice and often lead to implementation failures so significant that the advantages of experimental study are brought into question (Clarke and Cornish 1972; Petersilia 1989; Weisburd 1993). Finally, and perhaps most important, critics of experimental approaches have argued that implementation of the experimental method imposes so many limitations on criminal justice practice that even if ethical barriers can be overcome and experiments carried out successfully, they are not likely to have much policy relevance (Eck 2002; Pawson and Tilley 1997).

Place-based randomized trials in the area of hot spots policing appear very much at odds with these common assumptions regarding experimentation in criminal justice. Hot spots policing refers to the concentration of police resources in small discrete areas such as addresses, street blocks, or clusters of addresses or street blocks (Sherman and Weisburd 1995; Weisburd and Braga 2003). It has become a core strategy in American police agencies, and there is good reason to believe that research played an important role in its wide adoption (Weisburd and Lum forthcoming). Importantly, given our discussion of the marginal status of experimentation in criminal justice, randomized experiments have played a central role in the evaluation of hot spots policing strategies (Braga 2001; see also Braga

et al. 1999; Sherman and Rogan 1995; Sherman and Weisburd 1995; Weisburd and Eck 2004; Weisburd and Green 1995).

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Why has experimentation in hot spots policing succeeded in playing a central role in research and policy in criminal justice when experimentation more generally has played a marginal role? How were common ethical dilemmas overcome? How were common implementation problems resolved? What accounts for the widespread policy relevance of these studies, despite the fact that they were implemented within the constraints of an experimental design? To try to gain insight into these questions, I draw upon lessons from two specific randomized experiments in hot spots policing that I helped design and implement in the 1990s: the Minneapolis Hot Spots Experiment (Sherman and Weisburd 1995) and the Jersey City Drug Market Analysis (DMA) Experiment (Weisburd and Green 1995). Using a case study approach, I identify factors that facilitate and inhibit development and implementation of randomized trials with particular focus on the special problems and or advantages of place-based experiments. While my main comments will focus on the success of place-based randomized trials in evaluating hot spots policing approaches, I will draw insight as well into the reasons why the successful example of experiments in hot spots policing has not inspired similar place-based experimentation in other areas of policing or criminal justice.

### The Emergence of Place-Based Randomized Experiments in Crime Hot Spots

In many cases of cluster randomized trials, the choice of “place” as a unit of analysis develops from ethical or practical concerns (Boruch et al. 2004). Randomization, for example, may be carried out at the institutional level because researchers can find no ethically acceptable way of distributing resources randomly at the individual level. Randomization may also be carried out at the place level because of

the practical difficulties of randomization at the individual level. Interventions for children, for example, may be very hard to randomize successfully within a classroom but may be possible to randomize across schools. In the case of hot spots policing, the cluster randomized approach was developed not because of ethical or practical concerns but rather as a direct response to theoretical innovations in criminology and criminal justice. Focus on place was part of a paradigm shift in the ways that criminologists understood the nature of crime problems (see Weisburd 2002; Weisburd and Braga 2003).

Everett Rogers (1995) noted in his seminal work on diffusion of innovation that the emergence of innovation is generally preceded by the wide recognition of a need for change, often provoked by some type of crisis. Without that recognition or crisis, institutions and individuals will often find it simpler to continue just as they were. This model is very much consistent with the emergence of hot spots policing and its diffusion in police agencies.

### *The crisis of confidence in American policing*

The 1970s and 1980s were decades of shock and crisis for American policing and for American police scholars. By the early 1990s, it appeared that every major police strategy to prevent or control crime had come to be “unmasked” by scientific evaluation. For example, there was no more visible approach to crime prevention in policing, or one that involved greater cost, than preventive patrol in cars. The idea that police presence spread widely across the urban landscape was an important method for preventing crime and increasing citizen feelings of safety was a bedrock assumption of American policing. But in a major evaluation of preventive patrol in Kansas City, Missouri, the Police Foundation concluded that increasing or decreasing the intensity of preventive patrol did not affect either crime, service delivery to citizens, or citizen feelings of security (Kelling et al. 1974). Similarly, rapid response to emergency calls to the police was considered to be a crucial component of police effectiveness. Yet in another large-scale study, Spelman and Brown (1984) concluded that improvement in police response times had no appreciable impact on the apprehension or arrest of offenders.

These and other studies in the 1970s and 1980s led scholars to challenge the fundamental premise of whether the police could have a significant impact on crime (see also Greenwood, Petersilia, and Chaiken 1977; Levine 1975). While the police had long considered their role as “crime fighters” as central to the police function (Klockars 1988), the scientific evidence seemed to suggest otherwise. Michael Gottfredson and Travis Hirschi (1990, 270), for example, wrote in their classic book on the causes of crime that “no evidence exists that augmentation of patrol forces or equipment, differential patrol strategies, or differential intensities of surveillance have an effect on crime rates.” David Bayley, a distinguished police scholar, wrote even more strongly in 1994,

The police do not prevent crime. This is one of the best-kept secrets of modern life. Experts know it, the police know it, but the public does not know it. Yet the police pretend

that they are society's best defense against crime. . . . This is a myth. First, repeated analysis has consistently failed to find any connection between the number of police officers and crime rates. Secondly, the primary strategies adopted by modern police have been shown to have little or no effect on crime. (p. 3)

As predicted by Rogers's (1995) model of diffusion of innovation, this period of challenge to the effectiveness of traditional models of American policing was followed by a new openness to police innovation in the 1990s. In part, this openness was reflected in what might be termed an expansion of the police function. For example, community policing defined new tasks for the police, often extending much beyond the traditional crime control function (Goldstein 1987; Greene and Mastrofski 1988; Rosenbaum 1994). While new roles for the police were an important part of police innovation in the 1990s, neither scholars nor practitioners abandoned the idea that police could be more effective in preventing and controlling crime (e.g., see Goldstein 1979, 1990). The emergence of hot spots policing represents one attempt to develop more effective police practices. It can be traced directly to emerging theoretical perspectives in criminology that suggested the importance of place in understanding crime.

### *Crime places as a focus of police crime prevention efforts*

The traditional focus of research and theory in criminology has been upon individuals and communities (Nettler 1978; Sherman 1995). In the case of individuals, criminologists have sought to understand why certain people as opposed to others become criminals (e.g., see Akers 1973; Gottfredson and Hirschi 1990; Hirschi 1969; Raine 1993) or to explain why certain offenders become involved in criminal activity at different stages of the life course or cease involvement at other stages (e.g., see Moffitt 1993; Sampson and Laub 1993).

In the case of communities, criminologists have often tried to explain why certain types of crime or different levels of criminality are found in some communities as contrasted with others (e.g., see Agnew 1999; Bursik and Grasmick 1993; Sampson and Groves 1989; Shaw and McKay 1972), or how community-level variables, such as relative deprivation, low socioeconomic status, or lack of economic opportunity, may affect individual criminality (e.g., see Agnew 1992; Cloward and Ohlin 1960; Merton 1968; Wolfgang and Ferracuti 1967). In most cases, research on communities has focused on the "macro" level, often studying states (Loftin and Hill 1974), cities (Baumer et al. 1998), and neighborhoods (Bursik and Grasmick 1993; Sampson 1985).

Nonetheless, criminologists have almost from the outset recognized that the situational opportunities provided at the "micro" level of place can affect the occurrence of crime. Edwin Sutherland (1947), for example, whose main focus was upon the learning processes that bring offenders to participate in criminal behavior, noted in his classic criminology textbook that the immediate situation influences crime in many ways. For example, "A thief may steal from a fruit stand when the

owner is not in sight but refrain when the owner is in sight; a bank burglar may attack a bank which is poorly protected but refrain from attacking a bank protected by watchmen and burglar alarms" (p. 5). Nonetheless, Sutherland, as other criminologists, did not see "crime places"—small discrete areas within communities (Eck and Weisburd 1995)—as a relevant focus of criminological study. This was the case, in part, because crime opportunities provided by places were assumed to be so numerous as to make concentration on specific places of little utility for theory or policy. In turn, criminologists traditionally assumed that situational factors played a relatively minor role in explaining crime as compared with the "driving force of criminal dispositions" (Clarke and Felson 1993, 4; Trasler 1993). Combining an assumption of a wide array of criminal opportunities, and a view of offenders that saw them as highly motivated to commit crime, it is understandable that criminologists paid little attention to the problem of the development of crime at place.

The period of challenge to police practice noted above was also a period of more general challenge to traditional understandings of the crime problem. Beginning with C. Ray Jeffery (1971) and Robert Martinson (1974), a series of major reviews of criminal justice interventions and treatments supported a more general view that "nothing works" in criminal justice. Summarizing the overall standing of what they defined as traditional "offender centred" crime prevention, Patricia and Paul Brantingham wrote in 1990, for example, "If traditional approaches worked well, of course, there would be little pressure to find new forms of crime prevention. If traditional approaches worked well, few people would possess criminal motivation and fewer still would actually commit crimes" (p. 19).

One influential critique of traditional criminological approaches to understanding crime that was to have strong influence on the development of interest in crime places was brought by Cohen and Felson (1979). They argued that the emphasis placed in criminological theory on the developmental factors that affect criminal motivations failed to recognize the importance of other elements of the crime equation. In their theory of "routine activities," criminal events required not simply a "motivated offender" but also the presence of a "suitable target" and the absence of a "capable guardian" such as a police officer on the street or a doorman in an apartment building. They showed that crime rates could be affected by changing the nature of targets or of guardianship, irrespective of the overall level of predispositions to crime found in society. That Cohen and Felson suggested that crime could be affected without reference to the motivations that individual offenders bring to the crime situation was a truly radical idea in criminological circles in 1979. The "routine activities" perspective they presented established the context of crime as an important focus of study.

Drawing upon similar themes, British scholars led by Ronald Clarke began to explore the theoretical and practical possibilities of situational crime prevention (Clarke 1983, 1992, 1995; Cornish and Clarke 1986). Their focus was on criminal contexts and the possibilities for reducing the opportunities for crime in very specific situations. Their approach, like that of Cohen and Felson (1979), turned tradi-

tional crime prevention theory on its head. At the center of their crime equation was opportunity. And they sought to change opportunity rather than reform offenders. In situational crime prevention, more often than not “opportunity made the thief” (Felson and Clarke 1998). This was in sharp contrast to the traditional view that the thief simply took advantage of a very large number of potential opportunities. In a series of case studies, situational crime prevention advocates showed that reducing criminal opportunities in very specific contexts can lead to crime reduction and prevention (Clarke 1992, 1995).

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One natural outgrowth of these perspectives was that the place where crime occurs would become an important focus for crime prevention researchers. In the mid- to late 1980s, a group of criminologists began to examine the distribution of crime at places. Their findings were to radically change the way many criminologists understood the crime equation, drawing them into a new area of inquiry that was to have important implications for police practice. Perhaps the most influential of these studies was conducted by Lawrence Sherman and his colleagues (Sherman, Gartin, and Buerger 1989). Looking at crime addresses in the city of Minneapolis, they found a concentration of crime at place that was startling. Only 3 percent of the addresses in Minneapolis accounted for 50 percent of the crime calls to the police. Similar results were reported in a series of other studies in different locations and using different methodologies, each suggesting a very high concentration of crime in very specific places (e.g., see Pierce, Spaar, and Briggs 1988; Weisburd, Maher, and Sherman 1992; Weisburd and Green 1994). Such concentrations did not necessarily follow traditional ideas about crime and communities. There were often discrete places free of crime in neighborhoods that were considered troubled and crime hot spots in neighborhoods that were seen generally as advantaged and not crime-prone (Weisburd and Green 1994). This empirical research reinforced theoretical perspectives that emphasized the importance of crime places. It also redirected the attentions of crime prevention scholars to small areas often encompassing only one or a few city blocks that could be defined as hot spots of crime.

## Cluster Randomized Trials at Crime Hot Spots: Two Examples

These emerging theoretical paradigms and empirical findings led Lawrence Sherman and I (Sherman and Weisburd 1995) to explore the practical implications of the hot spots approach for policing. With cooperation from the Minneapolis Police Department, we developed a large experimental field study of “police patrol in crime hot spots.” The study sought to challenge the conclusions of the Kansas City Preventive Patrol Experiment noted earlier, then well established, that police patrol has little value in preventing or controlling crime. But the study also sought to show that the focus of police efforts on crime hot spots presented a new and promising approach for police practice.

The idea of focusing police patrol on crime hot spots represented a direct application of the empirical findings regarding the concentration of crime in discrete places. The Kansas City Preventive Patrol Experiment had looked at the effects of police patrol in large police beats. However, if “only 3 percent of the addresses in a city produce more than half of all the requests for police response, if no police are dispatched to 40 percent of the addresses and intersections in a city over one year, and, if among the 60 percent with any requests the majority register only one request a year, then concentrating police in a few locations makes more sense than spreading them evenly through a beat” (Sherman and Weisburd 1995, 629).

Applying these findings to police practice raised significant questions for police and for criminologists about the overall crime control benefits of a hot spots approach. How would one know if crime prevention benefits gained at hot spots would not simply be displaced to other areas close by? Sherman and Weisburd (1995) noted that displacement was a potential but not necessarily certain occurrence. They argued, moreover, that the first task for researchers was to establish that there would be any deterrent effect of police presence at the hot spots themselves:

The main argument against directing extra resources to the hot spots is that it would simply displace crime problems from one address to another without achieving any overall or lasting reduction in crime. The premise of this argument is that a fixed supply of criminals is seeking outlets for the fixed number of crimes they are predestined to commit. Although that argument may fit some public drug markets, it does not fit all crime or even all vice. . . . In any case, displacement is merely a rival theory explaining *why* crime declines at a specific hot spot, if it declines. The first step is to see whether crime can be reduced at those spots at all, with a research design capable of giving a fair answer to that question. (p. 629)

The results of the Minneapolis Experiment stood in sharp distinction to those of the earlier Kansas City study. The Minneapolis Experiment included randomization of 110 crime hot spots, each “hot spot” about one city block in length, to intervention and control conditions. The intervention sites received on average



between two and three times as much preventive patrol as the control sites. For the ten months in which the experiment was properly implemented, there was a significant and stable difference between the two groups of hot spots in terms of crime calls to the police and observations of disorder in those areas. Crime, or at least crime calls and disorder, appeared to be prevented in the intervention as opposed to the control locations. Sherman and Weisburd (1995, 645) concluded that their results show “clear, if modest, general deterrent effects of substantial increases in police presence in crime hot spots.” They noted that it was time for “criminologists to stop saying ‘there is no evidence’ that police patrol can affect crime” (p. 647).

Before the results of the Minneapolis Hot Spots Experiment were available, the National Institute of Justice (NIJ) decided to support a series of studies that would examine the problem of drug markets in American cities. This Drug Market Analysis Program (DMAP) was developed in good part as a response to the more general concern regarding drug crime that was very much on the public agenda at the time. But it was also strongly influenced by the hot spots findings and the implementation of the Minneapolis experiment.

One of the studies supported by DMAP drew heavily from the methods of the Minneapolis experiment, but its focus was on whether applying a “problem-oriented policing strategy” in drug markets would lead to more effective drug policing than that of more traditional methods. Fifty-six hot spots of drug activity were randomized to experimental and control conditions. The drug hot spots themselves varied in size: most were composed of just one or a group of two to four street segments (see Weisburd and Green 1995).<sup>1</sup> The intervention strategy followed a stepwise approach that sought to engage business owners and citizens in crime control efforts, to apply pressure to reduce drug and drug-related activity through police crackdowns, and to initiate a maintenance program with the assistance of the patrol division of the police department. In line with tactics employed by street-level narcotics units in many other American cities, the strategy used in control group hot spots involved unsystematic arrest-oriented narcotics enforcement based on ad hoc target selection.

Comparing seven-month preintervention and postintervention periods, Lorraine Green and I found consistent and strong effects of the experimental strategy on disorder-related emergency calls for service (Weisburd and Green 1995). We also found little evidence of displacement of crime to areas near the experimental hot spots. Indeed, data suggested a phenomenon opposite to that of displacement, which Ronald Clarke and I have termed “diffusion of crime control benefits” (Clarke and Weisburd 1994). In the case of specific crime call categories (public morals and narcotics), areas immediately surrounding the experimental drug hot spots were found to have significantly lower counts (comparing preintervention and postintervention periods) than areas around the control drug hot spots. We concluded that while there is little evidence that “strategies of crime control broadly defined, do much to solve crime problems,” the “police can be effective when they take a more specific approach to crime and disorder” (Weisburd and Green 1995, 717).

## Why Randomized Experiments?

The fact that hot spots policing emerged from innovations in crime prevention theory does not in itself explain why the first major hot spots studies were conducted as cluster randomized trials. As noted earlier, randomized experiments have, for the most part, remained on the margins of criminal justice evaluation. Why then, do we have a push for randomized experimental evaluation of hot spots practices? Indeed, of nine hot spots policing studies identified in a review by Braga (2001), fully five have been cluster randomized trials. Only one other area of criminology—mandatory arrest policies for domestic violence (see Garner, Fagan, and Maxwell 1995)—has had this type of concentration of experimental studies. I think it not coincidental that they were developed close in time and under the same federal funding agency and administration as the Minneapolis and Jersey City experiments.

In good part, the development of cluster randomized trials in hot spots policing, as the more general innovation of hot spots policing itself, can be traced to the crisis in criminal justice practice of the 1970s and 1980s. As scholars began to assess why so much of the evidence regarding criminal justice practice was negative, they looked not only to the failures of conventional theories and approaches in terms of what works in crime prevention but also to the methods that were used to assess crime prevention programs. A number of critics began to question whether the approach to evaluation taken by criminal justice researchers was itself a major factor explaining why so many programs were found ineffective (Visher and Weisburd 1998). These scholars called for more rigorous evaluation methods in criminal justice.

Perhaps the most influential of these critiques was brought by David Farrington, Lloyd Ohlin, and James Q. Wilson. In a seminal book titled *Understanding and Controlling Crime* (1986), they placed strong emphasis not only on what we know about the crime problem but also on the ways in which we come to gain knowledge. They argued that randomized experiments must be conducted if criminal justice is to draw valid policy conclusions about what works. The fact that Ohlin and Wilson were two of the leading figures of the elder generation of American criminologists, and Farrington one of the leaders of the then-younger generation, added significant weight to the book's conclusions.

In trying to revisit a core police practice—preventive patrol—in hot spots, Lawrence Sherman and I were strongly affected by this critique. We agreed from the outset that a randomized experiment was necessary if we were to develop a study with real authority for influencing what were then commonly held beliefs about the ineffectiveness of police patrol. Our main problem was to gain the cooperation of a major police agency that would be willing to randomly allocate preventive patrol to police hot spots.

This meant that we needed to identify a police executive who not only was committed to innovation and research in policing but who also had sufficient trust in Sherman and me to allow a major intervention in normal patrol operations based

on a scientific research design. Sherman had already developed a strong relationship with Anthony Bouza, who had become chief of the Minneapolis Police Department, in the 1980s. He had previously supported the Minneapolis Domestic Violence Experiment (Sherman and Berk 1984), a groundbreaking study in the application of randomized experimental methods in policing. He recognized the possibilities of hot spots approaches as well as the desirability of randomized evaluations. The relationship between Bouza and Sherman, as well as the support of an innovative mayor and City Council, made the proposal of a randomized study possible.

The choice of an experimental design did not necessarily facilitate the funding of the Minneapolis project. An original proposal for the hot spots study was submitted to the NIJ under the policing and crime control areas. Shortly after the peer review was completed, I had an informal meeting with a NIJ grant monitor who sat in on the review of the Minneapolis Hot Spots Experiment. She noted that reviewers were impressed by the proposal but thought that it led to many unanswered questions regarding the methods and the program. The peer review committee had suggested that we revise and resubmit the proposal for the next NIJ funding cycle. Importantly, as noted in the original proposal, we had only a short time to implement the study before the present chief of police would retire. A revise and resubmit in which we would have to wait for the next funding cycle would have effectively “killed” the study.

My experience suggests that the peer review response to the proposal for the Minneapolis experiment is not uncommon in review of experimental studies in criminal justice. One of the distinct advantages of experimental study is that the methods are transparent and the achievement of confidence in the validity of the comparisons made is based on aspects of design rather than statistical manipulation. But the fact that so much needs to be laid out clearly in terms of design and implementation at the outset in an experiment naturally leads to loose ends in the description of a study. In contrast, nonexperimental studies rely upon statistical approaches to deal with problems such as dosage or subject variability after the study is completed. Nonexperimental studies can be more “cleanly” described in a proposal since the investigator can claim that problems will be addressed “post facto.” Irrespective of whether the nonexperimental approach is in fact convincing in a statistical sense, it is less awkward in description in a proposal. Joel Garner, a former deputy director of research at the NIJ, explains:

I agree that peer review panels may want more details in a proposed experimental design than in a proposed nonexperimental design. . . . In an experimental design, certain features are fixed forever. In a non-experimental design, nothing is fixed, so they can be changed later. If you can't change features later, you want to be sure that they are right at the beginning. (Personal communication 2002)

Consequently, if there is not a “bias” in favor of randomized experiments because of their advantage in terms of ensuring high internal validity, peer reviewers will tend to favor nonexperimental designs. In the case of the Minneapolis Experi-

ment, and I suspect in other experimental studies in criminal justice as well, the choice of experimental methods likely hindered rather than facilitated funding. This is a point I will return to at the end of the article. But at this juncture, it is important to note the irony that scientific review of experimentation in criminal justice may often serve as a barrier to experimental study.

In response to this serendipitous meeting with an NIJ program manager, Sherman met with the then-director of the NIJ, James K. Stewart. Sherman explained the time constraints relevant to the experiment, a point that Stewart, a former police officer, understood. It is also important to note that Stewart had become a strong advocate of the experimental method. Stewart established a special peer review panel for the study. The design was approved with modifications, and the experiment was able to be implemented while Anthony Bouza served as chief of police.

The Jersey City DMA Experiment provides an almost mirror image to that of the Minneapolis Hot Spots study. This experiment was developed in response to a specific solicitation that called for the use of hot spots approaches to examine the problem of drug markets. The original DMAP solicitation encouraged experimental methods as part of James K. Stewart's more general support for randomized studies. It supplied funding both to researchers and to the police agency involved. The proposed funding level of \$450,000 per site in a first wave, to be supplemented in a second wave of funding, made this a major research/program effort in criminal justice at the time.

In the Jersey City Experiment, there was no special relationship between the researcher and practitioner, though the connection to the police department was made through individual officers whom I had come to know. Nor was the Jersey City Police Department's management known for its commitment to innovation in police practices. The police department's choice to participate in the study was based primarily on its desire to become more innovative and to gain recognition through the receipt of federal funding. Moreover, after a decade of increasing drug problems in the city, commanders in the department were interested in reform in their approach to drug markets. More generally, the openness of police agencies to researchers and innovation during this period can also be seen as linked to the crisis in policing described earlier. The agreement to implement a randomized study was made at the outset and was a precondition that I set for a partnership with Rutgers University in the development of an application to the NIJ.

The NIJ had established a special peer review committee for the DMA funding program. While the peer review committee strongly endorsed the Jersey City experiment, it was not one of those sites initially chosen for funding. Ironically, it was NIJ's concern that the Jersey City Police Department was not nationally prominent enough to warrant a major national research effort that held up research funding. Only after a series of discussions with NIJ staff and the director were they convinced that the site would be worthwhile to include among the DMAP participants.

This example suggests that "ordinary" police agencies can be brought on board to participate in experimental study if there is strong governmental encourage-

ment and financial support that rewards participation. A similar experience in the Spouse Assault Replication Program (SARP) reinforces these observations. Joel Garner (2002), who served as program manager for SARP, noted in a personal communication that he knew that the program was a success the “day that we got 17 proposals with something like 21 police agencies willing to randomly assign offenders to be arrested.”

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Why experimentation? Examination of these studies suggests an initial observation regarding the successful development and funding of randomized studies. In a discipline where there is not a presumption for experimentation, experiments are more likely to be proposed when there is strong attack upon conventional practices. In this sense, practitioners and researchers bring out their strongest “weapons” when they are most on the defensive. Ironically, however, if there is not a presumption for experimentation, researchers proposing experimental studies will often find themselves in an inferior position in scientific peer review—in good part because of the necessity of defining clearly a host of practical and methodological questions that are difficult to identify neatly at the outset.

### Place-Based Approaches and Ethical Concerns: Avoiding Traditional Pitfalls and Encountering New Ones

Boruch et al. (2004) suggested that it may be possible to avoid many ethical and moral dilemmas commonly associated with experimentation by randomly allocating at the organizational or place level, rather than randomly allocating individuals.

At first glance, one might question why the change in unit of analysis should affect ethical concerns. Why should it matter, for example, whether students in a specific school are allocated to treatment and control conditions versus all students in specific schools? The end result is the same. Some individuals will gain treatment and others not. However, where subjects do not experience the inequality of treatment directly (e.g., by seeing other students in their school being treated differently), ethical dilemmas may just not be raised. In this context, it may be politically more feasible to conduct cluster or place-based randomized trials rather than individual-level randomized studies.

The general proposition that place-based studies are likely to be faced with relatively fewer ethical objections applies to both the Minneapolis and Jersey City experiments. I do not recall any of the city officials that we were in contact with in these studies raising significant ethical concerns during negotiations over randomly allocating either crime hot spots or drug markets to treatment and control conditions. In recent correspondence with Sherman (2002), he noted as well that “no one ever raised ethical objections” to the Minneapolis Hot Spots Experiment. Moreover, as neither study collected information directly from human subjects, but relied rather on official police data and observations of the sites, they were not subject to significant human subjects review. This contrasts strongly with controversies often surrounding the random allocation of individuals in criminal justice settings.

While the location of treatment at the level of places rather than individuals certainly contributed to an environment in which ethical concerns were not central, the nature of the treatment and control conditions were also an important feature limiting ethical objections. In both studies, accepted police practices were employed. Preventive patrol was a standard strategy in policing, and though applied at an unusual dosage in the Minneapolis Hot Spots Experiment, its use was not controversial. In turn, the basic features of the strategy used in the experimental sites in the Jersey City DMA Experiment were based on well-established principles of problem-oriented policing (see Goldstein 1990).

Moreover, in both studies, the experimental treatment was not compared to a “placebo” condition. In the Minneapolis experiment, the control hot spots continued to receive emergency service from the police; When citizens called the police for service, squad cars were dispatched as was standard practice. The goal in the Minneapolis study was to increase police presence at hot spots to three hours per day and to maintain a ratio between experimental and control sites of at least two to one in patrol presence. Accordingly, what was compared in the experiment was a very high dosage of preventive patrol in the intervention sites to a relatively lower dosage in the control sites. Similarly, in the Jersey City DMA study, the control condition also received “treatment,” though in this case it was the standard package of practices used by the Jersey City narcotics squad. A fair playing field for the study was provided by dividing up the narcotics squad into two separate units of equal size, an experimental unit and a control unit. The experimental unit was charged with “treatment” of the experimental or intervention drug markets and the control unit with “treatment” of the control drug markets.

By not withdrawing all intervention from the control locations, we avoided many of the ethical objections that practitioners and the public ordinarily raise in randomized field trials. This was especially the case in the Jersey City experiment, where the level of police service in terms of officers assigned was equivalent for the two groups. As Frank Gajewski (personal communication 2002), the senior police commander who served as a principal investigator for the study, explained, "We only had a few problems with this since we were not withholding treatment from any of the markets. Arrests were being made citywide and the detectives could still show that they were 'doing something.'" Even in the Minneapolis experiment, neither police nor researchers expected that the level of police service at the control hot spots would change dramatically from prior patterns. Spreading preventive patrol throughout the city naturally led to a relatively small dosage of police presence in any specific location. The hot spots approach allowed the concentration of resources in specific environments at a relatively high level of dosage.

While common ethical objections to random allocation did not surface in the hot spots studies, different types of objections were raised by citizens and the police. The objections are suggestive of more serious problems that might develop in place-based experiments. For example, in Minneapolis, the City Council was asked to approve the reallocation of police resources in the hot spots experiment. One city councilman in a low crime area would not give his approval unless "an early warning crime trend analysis plan" would monitor burglary trends and send more patrols back into his neighborhood if "burglary rashes developed" (Sherman, personal communication, 2002). Monitoring did not reveal such increases in burglary, and thus the experiment was not affected.

In the Jersey City experiment, when a citizens group in one area of Jersey City found out that their neighbors were getting extra police attention, they demanded to be made part of the hot spots study. The police convinced the citizens group that they continued to get good police service but that their problem (to their benefit) was not sufficiently serious to make them eligible to join the experiment. In Jersey City, all of the drug areas that showed consistent and serious activity were included in the study. This coverage, combined with the "equality of police resources" in the experimental and control areas, made it possible to avoid objections that some serious drug markets were receiving more police attention than others. Nonetheless, the rule that "experiments with lower public visibility will generally be easier to implement" (Weisburd 2000, 186) appears particularly relevant to cluster randomized trials.

A more complex problem was raised by police officers participating in the Minneapolis study. Many patrol officers objected to the hot spots approach of "sitting" in or riding through specific areas. My comments here are based on "ride-alongs" in Minneapolis and comments from field researchers. While we tried to draw support from rank-and-file police officers for the experiment through briefings, pizza parties, and the distribution of t-shirts bearing the project logo ("Minneapolis Hot Spot Cop"), many officers argued that the hot spots approach was unethical and violated their obligations to protect the public. In particular, they argued that the approach simply allowed crime to shift around the corners from the hot spots. In

practice, there were not any wide-scale attempts by officers to undermine the experiment, but these objections appear similar to the practitioner concerns that undermined the Kingswood study that forms the basis for Clarke and Cornish's (1972) well-known critique of experimentation in the Home Office. In that study, practitioner beliefs about appropriate treatment led them to divert potential subjects from participation in the experiment. Clarke and Cornish defined this as an ethical dilemma in experimentation. I think it important to note that place-based studies may face similar ethical dilemmas.

### Implementing Place-Based Randomized Trials: The Importance of Monitoring Experimental and Control Conditions and the Problem of Complexity

Boruch et al. (2004) suggested that it may be difficult to monitor "implementation fidelity" in a cluster randomized trial, especially when integration or coordination of a wide variety of services across agencies is required. While cluster randomized trials certainly raise new complexities in terms of the monitoring of treatments, the experience of the hot spots studies is that monitoring may be facilitated by focusing on clearly defined places and the application of treatments in a visible social environment.

Both the Minneapolis and Jersey City experiments placed strong emphasis on monitoring treatment integrity. Earlier reviews had suggested that failures to ensure delivery of treatment, or to ensure differences in treatment dosage between treatment and control conditions, were common in criminal justice (e.g., see Weisburd 1993). In the Minneapolis Hot Spots Experiment, a major effort was made to observe activity at the hot spots during the study: almost sixty-five hundred observations of the intervention and control hot spots were conducted, each of seventy minutes' duration. While one main purpose of the observations was to develop a measure of disorder that was independent of official police data, these social observations were also used to document and describe the level of dosage of police presence in the experimental hot spots and the ratio of that dosage between intervention and control hot spots. In the Jersey City study, day-to-day activities of the narcotics experimental and control units were monitored using several sources. First, weekly random ride-alongs were conducted by project staff. Second, both the control and experimental squads were required to complete daily activity logs. Third, detectives assigned to the intervention sites were required to complete a solo surveillance form that documented their attendance at their individual hot spots. Finally, narcotics arrest reports were monitored to keep track of the places where enforcement action was taken.

The importance of monitoring in both studies was confirmed by identification of "breakdowns" in the application of treatment. In the Minneapolis project, a ratio of greater than two to one in police presence between experimental and control hot spots was maintained until July (the experiment began in December of the previ-



ous year). But in August, a full breakdown of the experimental treatment was identified, with there being no difference in the observed ratio of patrol in the experimental versus control hot spots. The failure to maintain treatment dosage during the summer months is understandable, given the increase in citizen calls and street-level activity in the hotter weather and when children are on summer vacation. Nonetheless, had treatment fidelity not been monitored using observational methods, our findings would have been confounded by a long period in which treatment was not maintained. This confounding would have led us to mistakenly conclude that specific comparisons were not statistically significant (Sherman and Weisburd 1995). Because of monitoring, we decided to structure our analysis to take into account the breakdown of the experiment during the summer months.

In the Jersey City DMA study, the problem observed through monitoring was not a decline in activity over time but rather a failure on the part of the experimental unit to fully implement treatments at the outset of the study. During the first nine months of the study, only nine experimental hot spots received all of the basic components of the experimental intervention. To fully implement the study, the intervention period was increased from twelve to fifteen months. Additionally, a detailed implementation schedule for each site was developed, and the narcotics squad commander was replaced and put under the direct line of command of then-captain and co-principal investigator of the study, Frank Gajewski (later to be appointed chief of police in Jersey City). During the last five months of the study, all of the hot spots received the basic components of the experimental strategy as originally proposed.

Both the Minneapolis and Jersey City place-based trials illustrate the importance of monitoring experimental studies. They also suggest that monitoring place-based studies may be easier than individually based randomization, in part because places are constant, do not move as do individual subjects, and provide a clear locus for assessing treatment implementation. But these examples also illustrate another important component of maintaining the integrity of experiments more generally. Practitioner involvement in the experimental process is crucial for successfully maintaining treatment fidelity.

Given resistance among rank-and-file police officers to the hot spots approach in Minneapolis, one might ask how treatment fidelity was maintained at a high level up until the summer months. I have suggested elsewhere that “it will be easier to develop randomized experiments in systems in which there is a high degree of hierarchical control” (Weisburd 2000, 188). In Minneapolis, for example, the experiment was “facilitated” by a change in the case law that gave the chief of police more control over the four patrol precinct commanders (see Sherman and Weisburd 1995). Whatever the attitudes of individual officers, the hierarchical structure of the Minneapolis Police Department facilitated the chief’s imposition of an experimental design.

In Jersey City, the strong involvement of a senior police commander as a principal investigator in the study played a crucial role in preventing a complete breakdown of the experiment after nine months. As noted above, he took personal

authority over the narcotics unit and used his command powers to carefully monitor the daily activities of detectives in the experiment. This suggests the importance of the integration of clinical work and research work in criminal justice, much as they are integrated in medical experiments (see Shepherd 2003). It also reinforces the importance of practitioner “belief” in the importance and necessity of implementing a randomized study. In the Kingswood experiment described by Clarke and Cornish (1972), they illustrated how doubts regarding the application of the experimental treatment led practitioners to undermine the implementation of the study. In the Jersey City experiment, Captain Frank Gajewski was strongly convinced of the failures of traditional approaches and the necessity of testing new ones. Indeed, he described the traditional narcotics enforcement approach more as a method of maintaining drug markets than closing them down:

One can look at these drug markets as vineyards. The arrests made within their borders can be symbolized as the fruit from the vine. Each vineyard is capable of producing a continual supply of “fruit” as long as the vine is left intact. Some vineyards are larger than others. The arrest strategy sees the pickers (the police) traveling from vineyard to vineyard harvesting the fruit. There are many vineyards so the pickers never stay too long at any particular site. As demand increases from irate citizens . . . the police respond by picking more fruit. Police administrators seeking to assuage the public, display the high harvest numbers as evidence of their commitment and the efficiency of their organization. But the vines are never uprooted, indeed police activity may contribute to their health. (Gajewski 1994, 20)

The extent to which coercive power is needed to ensure treatment fidelity may depend on the complexity of treatments that are brought in a cluster randomized trial. While the Minneapolis experiment involved the entire police force, what was required for successful implementation of the study was very simple: officers were expected to be present at the experimental sites whenever possible. As Sherman and Weisburd (1995) noted,

What the officers did while present at the sites varied widely by officer. During an inspection visit at our invitation, Kelling [the principal investigator of the earlier Kansas City Preventive Patrol Experiment] observed that some were reading newspapers or sunning themselves while sitting on the patrol car, while others were engaging citizens in friendly interaction in community-policing style. The experiment was clearly no test of the content of police presence, only of the amount. (p. 634)

In contrast, the Jersey City DMA Experiment demanded a complex staged treatment for each experimental site:

In step one of the strategy, the officers analyzed the nature and form of the drug problem at experimental sites in order to identify and develop effective strategies for closing down drug locations. In step two they coordinated their enforcement efforts, which culminated in an intensive crackdown on the drug hot spots. In the final stage of the program, officers tried to maintain gains made earlier through continued monitoring of activity in treated locations. (Weisburd and Green 1995, 731)

One lesson that can be drawn from a comparison of the two studies is that when treatments are more complex, it is likely to be necessary to use more coercive mechanisms for maintaining treatment fidelity. In both studies, efforts were made

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*One lesson that can be drawn from a comparison of the two studies is that when treatments are more complex, it is likely to be necessary to use more coercive mechanisms for maintaining treatment fidelity.*

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to gain officer cooperation in the experiment. Indeed, in the Jersey City DMA study, narcotics officers were invited to planning meetings held at conference hotels and were included in site visits to examine “successful” examples of narcotics enforcement in other jurisdictions. Attempts were made to solve basic grievances that were related to implementation of the study in ways that were sympathetic to the detectives. For example, early on in the experiment, the detectives complained that the use of strategies that were not focused on arrests in the experimental squad would reduce their overtime. Since overtime accounted for as much as 50 or 100 percent increases in salary, this was a particularly difficult issue. However, to facilitate support for the experiment, it was agreed that overtime would be maintained at equal levels in both squads. Despite these attempts to encourage commitment to the experiment, in the end treatment fidelity was only maintained by the establishment of strong coercive control over the experimental squad.

### Offsetting Sample Size Limitations in Cluster Randomized Trials

One potential disadvantage of cluster randomized trials is that the choice of organizational or institutional units of analysis may restrict the number of cases available for randomization as compared with trials randomly allocating individuals. It was recognized at the outset that there would be a limited number of hot spots that could be studied in both the Minneapolis and Jersey City experiments. In the Jersey City DMA Experiment, the number of hot spots was limited by the restricted number of drug markets in the city. After placing a considerable degree of effort in defining drug hot spots, only fifty-two were identified that met the crite-

ria established. In Minneapolis, a much larger number of potential sites could be identified, but the police department did not feel it could maintain a high level of patrol activity at a large number of places. Eventually, after negotiation with the police department, an experiment with fifty-five experimental or intervention hot spots and fifty-five control hot spots was approved.<sup>2</sup>

As Boruch et al. (2004) noted, the fact that a limited number of cases can be included in many cluster randomized trials raises significant concerns regarding the ability of randomization to provide for equivalent groups (see also Farrington, Ohlin, and Wilson 1986). To overcome the statistical limitations created by the small number of units in both the Minneapolis and Jersey City experiments, randomization was restricted to create as much equivalence as possible between the experimental and control conditions. It was decided at the outset that not enough was known about the relationship between characteristics of places and the experimental outcomes to match hot spots in pairs. Such matching would have meant a significant loss of degrees of freedom for the analysis without a known proportional benefit in terms of equivalence of the groups. Instead, a compromise solution was taken, in which the hot spots were grouped into statistical blocks (Fleiss 1986; Lipsey 1990; Weisburd 1993). In the Minneapolis Hot Spots experiment, the 110 hot spots were grouped into five statistical blocks of unequal size based on natural cutting points within the distribution of more serious crime calls to the police. In the Jersey City DMA Experiment, the 52 hot spots were divided into four unequal groups based on reported arrests and emergency calls to the police. These are the first criminal justice studies that we could identify that randomized within statistical blocks. Randomization was also restricted to create an equal number of experimental and control units within each block.

Block randomization was also used as a method of increasing the statistical power of the two studies. Sherman and I noted in the design of the Minneapolis study that the “major statistical limitation in all experiments in patrol or neighborhood-level crime reduction is lack of power” (Sherman and Weisburd 1995, 627). In part because of limitations in sample size, in both experiments considerable efforts were made to increase statistical power using other methods. By making the intervention and control groups as similar as possible in terms of official crime data, we sought to increase the ability of each study to distinguish the effects of the intervention from potential error variability.<sup>3</sup>

Statistical power was also a factor in the selection of the hot spots. Analysis of earlier data in Minneapolis suggested that some hot spots showed extreme fluctuations in crime calls year to year. If such fluctuations were indicative of future trends at those places, the inclusion of such “unstable” units in the study would again increase the error variance of the outcomes examined. The fact that a limited number of cases could be included in the study meant that the effects of such “unstable hot spots” might obscure treatment effects. For this reason, hot spots with greater than a 150 percent increase or 75 percent decrease in serious crime calls in a two-year period before the selection year were excluded. In the Jersey City DMA Experiment, it was required that there be evidence of repetitive drug activity over a

six-month period for a street segment or intersection to be included in the selection process.

## Why Have Hot Spots Experiments Failed to Inspire?

Place-based randomized trials have played a leading role in the development of innovation in hot spots policing (Weisburd and Lum forthcoming). This article describes the crisis in American policing and criminal justice more generally that led scholars to focus on the problem of place and how that crisis also encouraged the development of experimental methods to test hot spots approaches. Contrary to the position that experimentation is likely to be blocked by major ethical objections, these studies encountered few ethical barriers either from the institutions charged with carrying out the experiment or the citizens in the communities in which they were implemented. This seems to have been due in part to the use of places as a unit for random allocation, in part to the testing of police strategies that had already gained some legitimacy, and in part to the fact that a placebo control group was not used. The place-based unit of analysis also facilitated careful monitoring of the experiments. This monitoring in turn allowed for a high level of treatment fidelity, therefore enabling successful implementation of treatment and a fair test of the treatments examined.

Recent studies suggest that hot spots policing approaches have become widely diffused in police practice. For example, a recent Police Foundation study found that more than seven in ten police departments with one hundred or more sworn officers reported using crime mapping to identify “crime hot spots” (Weisburd et al. 2001). Examining the diffusion of crime mapping technologies in police agencies, Weisburd and Lum (forthcoming) found that the rapid adoption of computerized crime mapping in police agencies is closely linked to the implementation of hot spots policing programs and that the timing of this adoption in the 1990s follows closely the dissemination of the findings of the hot spots policing experiments.

One might assume from this “success story” that the model of hot spots policing experiments would have been replicated in other criminal justice areas. In practice, the hot spots experiments did not lead to the large-scale adoption of cluster randomized methods either in policing or in criminal justice more generally. Why, then, have the hot spots studies, which appear to have been influential both in defining the effectiveness of a major new policing approach and in encouraging its widespread adoption, failed to inspire the use of experimental methods more broadly for place-based evaluations of police or criminal justice programs?

One explanation can be drawn directly from this review. There must be a predisposition from the outset for the application of experimental methods in field settings. As illustrated above, randomized experiments may face special difficulties in gaining peer review approval. For experiments to succeed in the peer review process, there must be a recognition of the particular advantages of randomized exper-

iments, much as there is today in medical trials, and much as there was in the case of the Jersey City DMA Experiment. As long as the playing field remains even for experimental and nonexperimental methods, experiments will find it more difficult to transverse the peer review process. But we might ask why criminal justice funders have not chosen to exhibit such a preference for experimental studies given the clear benefits of experimental methods in deciding upon the effectiveness of treatments and programs.

Garner and Visser (2003) suggested one general explanation for the failure to encourage randomized experiments in their review of NIJ funding patterns over the past decade. They found that randomized experiments cost about 30 percent more on average than nonexperimental studies. These costs are understandable in the context of the discussion above, where the processes of monitoring treatment fidelity were clearly expensive and at the same time crucial to the success of the experimental methods used. Nonetheless, when funders feel pressured to provide a wide coverage of many different topic areas, they are certainly likely to look to fund efforts that are less costly and allow them to do more. Analyses of existing data, for example, are much less costly than field experiments though their results may be equivocal.

## Conclusions

Place-based randomized trials in hot spots policing provide a promising model for application of experimental methods in criminal justice. Not only have experimental methods been developed successfully, but they have also provided strong policy-relevant findings that have been applied widely in practice. Using a case study approach to examine two of these studies, the Minneapolis Hot Spots Experiment and the Jersey City DMA Experiment, I draw eight specific lessons regarding the implementation and development of place-based randomized trials and experimental methods more generally:

1. A crisis in the legitimacy of conventional practices is likely to facilitate the development of randomized controlled experiments.
2. The final number reflected both the police department's capabilities in bringing adequate dosage to the sites as well as a power analysis suggesting that this number was adequate for the purposes of the experiment.
3. There must be a predisposition toward randomized trials if experimental designs are to succeed either in scientific review or in the political processes that lead to funding allocations.
4. Random allocation of places can lead to fewer ethical objections to experimental study.
5. Monitoring treatment fidelity is essential to successful experimentation. Such monitoring may be facilitated by the use of place as a unit of analysis in cluster randomized trials.
6. Strong hierarchical controls within the institution administering treatment, and a collaborative involvement of an individual able to utilize such authority, are likely to facilitate the implementation of a place-based randomized trial.
7. The more complex the treatment or intervention, the more coercive the mechanisms that are likely to be necessary for maintaining treatment integrity.

8. Place-based randomized trials are likely to face strong limitations in the number of sites that can be identified or treated. Block randomization provides a method for overcoming some problems related to restrictions in sample size, including ensuring the equivalence of groups and maximizing statistical power of tests employed.

## Notes

1. Two main criteria were used for defining the drug hot spots after identifying street segments and intersections with repetitive drug activity: (1) street segments and intersections were linked that evidenced similar types of drug activity, and (2) active segments and intersections were linked only if they were within one block and one intersection of one another. See Weisburd and Green (1995) for details regarding the approach used.
2. The final number reflected both the police department's capabilities in bringing adequate dosage to the sites as well as a power analysis suggesting that this number was adequate for the purposes of the experiment.
3. Block randomization also allowed for the specification of block and block-by-treatment interactions in the models within an experimental context. In this way, error variance in assessing treatment outcomes would be further reduced by distinguishing error variance used for testing treatment impacts from block and block-by-treatment effects. Block-by-treatment interactions were included only if they were found to be statistically significant as suggested by Fleiss (1986).

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