POLICING DRUG HOT SPOTS: THE JERSEY CITY DRUG MARKET ANALYSIS EXPERIMENT*

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This paper reports the results of a randomized experimental evaluation of an innovative drug enforcement strategy developed as part of the Drug Market Analysis Program. Using computer mapping techniques, we identified 56 "hot spots" of drug activity that were randomized in statistical blocks to experimental and control conditions. The experimental 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 department. In line with tactics employed by street-level narcotics units in many other American cities, the control strategy involved unsystematic arrest-oriented narcotics enforcement based on ad hoc target selection. Comparing seven-month pre- and post-intervention periods, we find consistent and strong effects of the experimental strategy on disorder-related emergency calls for service. We also find little evidence of displacement of the crime control benefits of the study to areas near the experimental hot spots. Indeed, through two separate methods, our data suggest a "diffusion of benefits" around the experimental as compared with control locations.

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Policy makers and the public have come to view the police as a central component in efforts to control crime and urban decay. Whether in calling for greater police involvement in communities or for more police officers on the street, the public does not doubt the importance of the police in controlling crime and crime-related problems of disorder (see Bayley 1994; Clinton and Gore 1992; Mastrofski and Greene 1993).

In contrast to American politicians' and citizens' confidence in the police, criminologists have often been wary of an expanded police role in ameliorating crime problems (Sherman 1993). This concern stems in part from fears that more police, or more police involvement in citizens' everyday lives, will lead to constraints on the freedom of ordinary people (Walker 1993). But such skepticism is reinforced by a widely held perception that the police can have little impact on crime. Armed with research detailing the "failures" of traditional policing (see e.g., Kelling et al. 1974; Spelman and Brown 1984), some scholars argue that police are necessary for responding to citizen emergencies and maintaining order but cannot be expected to prevent or control crime. As Gottfredson and Hirschi conclude, "No evidence exists that augmentation of police forces or equipment, differential patrol strategies, or differential intensities of surveillance have an effect on crime rates" (1990:270).

Can police do something about crime? Over the last decade, scholars such as Ronald Clarke (1992) and Herman Goldstein (1979, 1990) have suggested that the police can be effective if they take a more specific, more focused approach to the crime problem (also see Wilson and Kelling 1982). They argue that the failures of the police are related more closely to the ways in which police have carried out their task than to the resistance of crime problems to criminal justice intervention. A series of case studies suggests that when police focus on specific offenses, such as telephone vandalism, graffiti, or street robberies, they can decrease the severity of crime and crime-related problems (see, e.g., Eck and Spelman 1987; Goldstein 1990; Kennedy 1993; Sloan-Howitt and Kelling 1990).

In response to the need for solid research on "what works" in policing, as well as to public pressures to strengthen the "War on Drugs," the National Institute of Justice initiated the Drug Market Analysis (DMA) program. DMA sought to develop new strategies for addressing street-level drug problems and to encourage technological innovations in geographic analysis of crime (see National Institute of Justice 1989). Though street-level drug markets had become the target of an increasing number of law enforcement innovations and initiatives, few researchers had systematically evaluated what police strategies work and under what conditions

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(Hayeslip 1989). The DMA program sought to develop systematic evidence about police effectiveness in combating a major American social problem.

In this paper we report the results of a randomized experimental evaluation of an innovative drug enforcement strategy developed as part of the DMA program in Jersey City, New Jersey. Using computer mapping techniques, we identified 56 "hot spots" (see Sherman, Gartin, and Buerger 1989; Sherman and Weisburd 1987, 1995; Weisburd, Maher, and Sherman 1993) of drug activity, which were randomized in statistical blocks to experimental and control conditions. The experimental 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 at hot spots through police crackdowns, and to initiate a maintenance program with the assistance of the patrol division of the department. In line with strategies employed by street-level narcotics units in many other American cities, the control condition involved application of unsystematic, arrest-oriented narcotics enforcement based on ad hoc target selection.

Comparing seven month pre- and post-intervention periods, we find consistent and strong impacts of the experimental strategy on disorder-related emergency calls for service. We also find little evidence of displacement of crime calls to areas near the experimental hot spots. Indeed, on the basis of two separate methods, our data suggest a "diffusion of benefits" (Clarke and Weisburd 1994) around the experimental as compared with control locations.

DEFINING HOT SPOTS OF STREET-LEVEL DRUG ACTIVITY

As in many other urban centers in the United States, drug problems and community fears of drug-related crime grew dramatically in Jersey City in the 1980s. In 1987, when the drug market project was proposed, 3,116 drug arrests were made in the city, almost three times as many as reported in 1980. To put this figure in perspective, in 1987 Jersey City (which had a population of 230,000) ranked higher per capita in number of drug arrests than Baltimore, San Diego, Newark, Tampa, New York City, Cincinnati, and Atlanta, all among the top 10 cities for drug arrests among those with populations over 250,000 (Jersey City Police Department 1989).

Drawing from a growing body of evidence on the clustering of crime problems into geographically defined "hot spots" (Sherman and Weisburd 1987; Sherman et al. 1989; also see Brantingham and Brantingham 1981), we sought, in the initial phase of our

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study, to locate high-activity drug areas in Jersey City. As a first step, we linked narcotics sales arrests, drug-related emergency calls for service, and narcotics tip-line information over a six-month period to "intersection areas" on a computer map of the city.[1] Of the 1,553 intersection areas in the city, 10 percent evidenced repetitive drug activity.[2] Within these areas, only 226 intersections and street segments (out of 4,404 street segments and intersections in the city overall) recorded any emergency calls, arrests, or tip-line entries.

In developing a method for defining the boundaries of drug hot spot areas, we drew heavily from the perceptions of narcotics detectives in Jersey City about how narcotics sales are organized at the street level. Although several neighborhoods in the city appear to have continuous drug dealing across a large number of streets and intersections, narcotics detectives generally do not view these places as undifferentiated areas of drug activity. For the detectives, a series of blocks, or sometimes even a single block or intersection, may be separated from others on the basis of the type of drug that is sold there. Although initially we were skeptical about this assumption of specialization at discrete places, our own analysis of narcotics arrest information generally confirmed the detectives' conclusion (see Weisburd and Green 1994).

Detectives also distinguish areas that are market centers in the same neighborhoods, but are separated from one another by a few blocks. They argue that dealers generally have a strong territorial sense, which tends to insulate market boundaries. This contention was also supported in our analyses. When we examined the pattern of arrests across the active segments and intersections, we found that very few people arrested more than once for selling narcotics crossed an inactive segment or intersection to sell in an adjacent drug hot spot area (see Weisburd and Green 1994). Indeed, people arrested in two separate areas were most likely to be arrested in different districts of the city.

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[1] Narcotics sales data (N=1,844) include arrests made between June 1, 1991 and November 30, 1991. Emergency calls for service include calls made for narcotics offenses between August 1, 1991 and November 30, 1991 (N=2,196). We defined an "intersection area" as an intersection and its four adjoining street segments (or blocks). Although this unit of analysis meant that one particular street segment could be included in two separate intersection areas, it allowed us to avoid the mistake of making decisions about the direction of drug offenders' movement patterns too early in the process of identifying drug hot spots.

[2] We used three threshold criteria to identify places evidencing repetitive drug problems: at least one drug sales arrest in two or more of the six months examined; at least one emergency call for service for narcotics in two separate months during the six-month target period, and a minimum of seven calls; and multiple narcotics tip-line responses. For a detailed description of the identification process see Weisburd and Green (1994); Weisburd, Green, and Ross (1994).

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Following these observations about drug distribution patterns, we used two basic criteria to construct the boundaries of drug hot spots in the Jersey City DMA experiment. First, taking into account the importance of type of drug in determining hot spot boundaries, we linked street segments and intersections that evidenced similar types of drug activity. Second, recognizing that sellers tended not to drift far from their primary point of sales, we linked only active segments or intersections that were within one block and one intersection of one another. Using these criteria, we identified 56 hot spots for inclusion in the DMA experiment, covering 192 segments and intersections in Jersey City.[3]

CHARACTERISTICS OF DRUG HOT SPOT AREAS

Although all of the hot spot areas we define meet a minimum threshold of drug activity, they vary widely. For example, cocaine was the prominent drug reported for sales arrests at more than half of the hot spots, heroin in six hot spots, and marijuana in three. In 10 other spots we found a relatively even distribution of sales arrests for different drugs. On average about 15 narcotics arrests and almost 20 emergency calls for narcotics were reported at each hot spot during the seven-month period preceding the experiment.[4] However, 11 hot spots were identified through call and tip-line information with no narcotics arrests during that period, and 19 spots recorded fewer than six narcotics calls each (see Table 1). In four of the hot spots, more than 50 narcotics arrests were made, and in six there were more than 50 narcotics calls.

The physical areas covered by the drug hot spots also vary greatly. Most however, are composed of fewer than four segments and intersections. Indeed, 17 consist of just one street segment or intersection. Only two include more than 10 segments and intersections within a single hot spot boundary.

The hot spot areas include a substantial proportion of the arrests and calls for service in the city, for narcotics-related and other crimes (see Table 2). For example, though the hot spots make up only 4.4 percent of the street sections and intersections in Jersey

[3] In addition to applying these criteria we excluded locations in public housing sites and places that we defined, after further review, as unlikely to be drug distribution centers. We excluded housing projects both because they appeared to represent a very different type of problem from other hot spots we identified and because they were receiving special enforcement through the housing authority. We reviewed commentaries on investigation and arrest reports and applied a minimum of three events per hot spot as the criterion for inclusion in the study. These very low-activity places generally evidenced an isolated arrest and were often on the outskirts of larger hot spots. We also found that several of these "hot spots" were locations where the police had set up drug buys, not places with an ongoing drug problem.

[4] Emergency call data were not available in Jersey City before this time. For consistency we examine a similar period for arrest information.

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Table 1. Narcotics Calls and Arrests in the 56 Drug Hot Spots during the Seven-Month pre-Intervention Period

	Narcotics Calls		Narcotics Arrests	
N of Events	N of Hot Spots	% of Hot Spots	N of Hot Spots	% of Hot Spots
0	1	1.8	11	19.6
1 - 5	18	32.2	15	26.8
6 - 10	15	26.8	10	17.9
11 - 25	8	14.3	10	17.9
26 - 50	8	14.3	6	10.7
51 - 100	3	5.3	3	5.3
> - 100	3	5.3	1	1.8
Total	56	100.0	56	100.0

Table 2. Proportion of Calls and Arrests in the Drug Hot Spots (as Percentage of City Totals)

	% of City Total	N of Events in Hot Spots	N of Calls Citywide
Call Category			
Violence	13.6	4,095	30,155
Property	6.5	1,867	28,873
Nuisance	15.5	2,915	18,832
Suspicious persons	11.0	978	8,853
Assistance	9.6	2,407	25,145
Public morals	21.6	516	2,391
Narcotics	45.5	1,130	2,481
Arrest Category			
Robbery	15.6	56	359
Assault	17.4	205	1,177
Burglary	7.0	120	1,725
Vandalism	10.8	18	166
Weapons	19.4	60	310
Public morals	40.7	138	339
Narcotics	46.3	851	1,840

City, they accounted for about 46 percent of narcotics sales arrests and emergency calls for narcotics in the seven-month period preceding the experiment. Crime and disorder problems typically associated with drug activity were also common in these hot spots before the experiment: Between 15 and 20 percent of all robbery, assault, and weapons arrests and more than 40 percent of arrests for public morals offenses were recorded within the hot spot boundaries. In the case of calls for service, more than one in five public morals calls and almost one in six nuisance calls originated within the hot spots.

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THE EXPERIMENTAL STRATEGY

Before the DMA project, the Jersey City Police Department narcotics squad relied on a series of loosely connected and unsystematic drug enforcement tactics (Gajewski 1994a). These included surveillances, arrests, search warrants, and "street pops" (essentially checks of people suspected of dealing drugs on street corners). The department used number of arrests as the defining measure of police officers' performance. Although the number of arrests increased yearly during the late 1980s, ranking officers in the department recognized that arrests had only a minimal impact on the drug problem. Using the analogy of harvesting fruit from trees, Captain Frank Gajewski, a principal investigator for the Jersey City DMA project, suggested that traditional drug enforcement in the city did as much to maintain the street-level drug problem as to combat it:

> 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 1994b:20).

Frustrated by traditional drug control strategies, the Jersey City Police Department sought to use what had been learned about the distribution of street-level drug activity in the first stage of the DMA program to develop more effective enforcement tactics.

Three specific components were merged into the Jersey City experimental drug enforcement strategy. First, drawing from experiences in projects such as the QUAD squad in Tampa (see Kennedy 1993), the department thought that assignment of specific drug hot spots to specific officers would increase accountability for solving drug problems. Second, building on initial analyses of the hot spot areas, which showed considerable diversity in both type and intensity of drug activities, the department recognized that methods of enforcement would have to be adapted to different types of places. Finally, because many experimental programs fail when gains in

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crime control are not maintained, it was decided to include an explicit maintenance stage in the program. These components were integrated into a systematic stepwise drug enforcement strategy.

In Step 1, the "planning stage," individual officers were assigned responsibility for collecting information about the physical, social, and criminal characteristics of specific hot spots. They reviewed computerized crime data via the DMA computerized mapping system and conducted a series of solo surveillances during times when the drug hot spots were likely to be active. They were also expected to meet with business owners and residents, to define the most troublesome areas within the hot spots, and to develop case files for persons identified as "primary sellers."

In Step 2, the "implementation stage," the officer responsible for a hot spot coordinated efforts to close down drug activity. Such pressure culminated in an intensive crackdown on the hot spot, which varied from a "mini" crackdown conducted by part or all of the experimental narcotics squad to a major coordinated departmental effort involving the experimental narcotics squad as well as a dozen or more patrol officers in cars and on motorcycles. Crackdowns generally lasted only a few hours at a specific location, but if problems persisted, the officers continued to return to the site over a few days. The type of crackdown was determined by the physical size of the hot spot, the number of potential offenders and bystanders involved, and the types of drugs and drug market behaviors found. When bars, restaurants, or stores were considered as involved in drug activity, the officers also coordinated efforts with local government agencies such as the sanitation, fire, and building departments. When possible, citations for violating regulations related to health or to alcohol and beverage licensing were brought simultaneously with other aspects of these crackdowns.

In the final step of the strategy, the "maintenance stage," detectives assigned to a hot spot were responsible for efforts to maintain gains that had been made during the implementation stage. In this stage, experimental squad detectives conducted routine solo surveillances of the hot spot and alerted the patrol division to the need for increased police patrol. For the larger hot spots, foot posts were coordinated at the sites for as long as one week after the crackdown.

THE EVALUATION DESIGN

We used a randomized experimental design to evaluate the DMA project in Jersey City. Although experimental program evaluations are difficult to implement, they allow researchers to define unambiguous links between causes and effects (Campbell and Stanley 1966; Sechrest and Rosenblatt 1987). Random assignment of

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subjects into treatment and control groups makes it possible to assume that the only systematic difference between experimental and control subjects lies in the interventions that are studied. In contrast, correlational or quasi-experimental designs are always plagued by the possibility that investigators have not taken some important confounding factor into account (Brody 1978; Farrington, Ohlin, and Wilson 1986).

We randomized cases in the study within statistical "blocks" because of the substantial variation in drug activity in the hot spots we identified. Randomized block designs, which allocate cases randomly within pairs or groups, minimize the effects of variability on a study by ensuring that like cases will be compared with one another (see Fleiss 1986; Lipsey 1990; Weisburd 1993). On examining the distribution of arrest and call activity in the hot spot areas before the experiment, we noticed a series of plateaus across the 56 hot spots. The sample tended to fall into four distinct clusters: very high arrest and call activity, and then high, medium, and low activity. To further explore the distribution of hot spot activity, we created a composite rank for each hot spot by adding the arrest rank of each area to its call rank. This process highlighted the plateaus of activity and allowed us to divide the hot spots into four groups: 10 hot spots that consistently showed evidence of very high call and arrest activity, eight that we classified as high activity, 26 classified as medium activity, and 12 classified as low activity.[5]

Next we allocated the hot spots randomly within each of the blocks to experimental and control conditions. Reflecting the blocking technique used, the groups were very similar on most characteristics (see Appendix). This was the case, for example, for average number of narcotics arrests and calls, mean age of narcotics sales arrestees, average number of segments per hot spot, type of hot spot, percentage of African-American residents, and percentage of those living within the hot spot boundaries under age 18. As is to be expected in a randomized study there are some differences between the experimental and control conditions. For example, 17 cocaine hot spots were allocated to the experimental group and only 12 to the control group; 14 West District hot spots were allocated to the control group and only seven to the experimental group.

[5] We found the greatest variability in activity in the 10 most active drug hot spots. To accommodate these differences and to allow separate evaluation of these hot spots, we paired the 10 most active locations into five independent blocks. In the top 10 markets, the mean numbers per hot spot were 47.6 narcotics arrests and 62.4 calls (in the seven-month period before the experiment); in the high-activity block we found an average of 20.8 arrests and 34.0 calls; in the medium-activity block, an average of 5.4 arrests and 7.4 calls; in the low-activity block, an average of 2.9 arrests and 3.4 calls.

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IMPLEMENTATION OF THE EXPERIMENT

The first task in implementing the experiment was to divide the narcotics unit into two separate groups. For the five years before the study began, the narcotics unit consisted of six squads, each comprising one sergeant and five detectives. The standard shift patterns for the unit allowed two squads to work side by side at any one time. In effect, the narcotics unit consisted of three teams of officers, each with two squads.

The squads within these teams were regarded by the commander of the narcotics unit as similar in their ability to conduct surveillances and make arrests (the primary activities of the squad before the experiment). This perception was reinforced by interviews with narcotics officers and by review of arrest activity. Nonetheless, the three teams differed greatly on these traditional measures of performance. To divide the unit into two groups that would create a relatively even distribution of abilities at the start of the study, we randomly assigned squads to the experimental and the control conditions across the three teams of squads. This randomization step divided teams that had worked together on shifts in the past, but it left the individual squads intact, maximized equality between the experimental and the control groups, and allowed the commanding officer to maintain the same shift roster patterns.

The experimental group was expected to apply the strategies described above to the 28 hot spots assigned to them. Yet we did not withdraw all "treatment" from the 28 control hot spots. We made this decision in part to avoid the practical and ethical concerns involved in not targeting serious drug locations. As in some medical studies, we assessed whether the experimental strategy was more effective than those currently in use in Jersey City. The control strategy thus comprised the mix of tactics used by the narcotics squad in the years leading up to the experiment. It can be characterized as unsystematic, arrest-oriented enforcement based on ad hoc target selection. In one important way, however, this strategy differed from the practices of the narcotics unit before the experiment: the three control squads now were expected to concentrate their efforts in the 28 hot spots in the control condition, and could not spread their activities across other areas of the city.

MONITORING THE INTEGRITY OF THE INTERVENTION STRATEGIES

Recognizing that a number of experiments in criminal justice had failed because of a lack of attention to treatment integrity (see

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Petersilia 1989; Weisburd 1993), we were especially concerned about potential officer subversion in applying the experimental and the control strategies. To reduce the potential for contamination that might result if the control squad "mimicked" the experimental unit, the two squads were separated physically. We also monitored the day-to-day activities of the narcotics unit, using several sources. First, project staff conducted weekly random ride-alongs. Second, both the control and the experimental squads were required to complete daily activity logs. Third, experimental detectives were required to complete a solo surveillance form documenting their attendance at their individual hot spots. Finally, we monitored narcotics arrest reports to keep track of the places where enforcement action was taken.[6]

Despite efforts to maintain the integrity of the experiment, treatments by the experimental group were implemented more slowly than had been expected over the first nine months of the study.[7] Indeed, during that period only nine experimental hot spots received all the components of the experimental strategy. To fully implement the study, we increased the intervention period from 12 to 15 months. In addition, the principal investigators developed a detailed implementation schedule for each site in consultation with the narcotics squad commander. During the last five months of the study, all of the hot spots received the basic components of the experimental strategy.

EVALUATION RESULTS: MAIN EFFECTS

In assessing the effects of the experimental intervention we compare emergency calls for service in seven-month pre-intervention and post-intervention periods.[8] Recent studies suggest that emergency calls are a more reliable measure of crime and crime-related activity than are other official indicators (e.g., Pierce, Spaar, and Briggs 1988; Sherman et al. 1989). In this regard, Sherman et al. argue that emergency calls "provide the widest ongoing data collection net for criminal events in the city" (1989:35). Moreover, as Warner and Pierce note, calls are affected less strongly than arrests

[6] Any enforcement efforts directed at targets outside the markets (e.g., through a district commander's request) had to be cleared with the principal investigator of the study in the department.

[7] See Gajewski (1994a) for a detailed discussion of implementation issues in the experiment.

[8] The pre-intervention period included all citizen-generated calls for service from August 1, 1991 to February 29, 1992. The post-intervention period included citizen-generated calls for service from June 1, 1993 to December 31, 1993. We retrieved these data from the Jersey City Police Department CAD system. Address fields were cleaned and geocoded against 100 percent accurate TIGER files. For call addresses, we achieved a 99 percent match rate, reflecting extensive efforts by the Jersey City Police Department to generate accurate CAD data.

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by "the most criticized element of official measures—police discretion" (1993:498).[9]

We do not examine the intervention period itself because the incidence of calls during that time was likely to be influenced by the nature of the strategies employed (regardless of their crime control impacts). For example, contact with store owners and residents of the hot spots in the planning stage of the strategy may have increased willingness to make emergency calls to the police. Such an effect could have resulted either as a direct response to police officers' encouragement of reporting crime events or simply because increased familiarity gave persons in the experimental hot spots greater confidence in the police.[10] Though biases in crime reporting were likely to be strongest during the intervention period, as we discuss later, some evidence of a long-term reporting effect is found in our analyses.

Random allocation allows us to assume no systematic differences in enforcement activities between the experimental and the control hot spots before the experiment. We recognize, however, that a similar assumption cannot be made for the period following the experiment. After the study was completed, the Jersey City Narcotics Squad was reconstituted in its original form. Officers returned to a primarily ad hoc approach to policing drug problems, though each of the reunited teams was given responsibility for specific Jersey City police districts. Crackdowns like those conducted during the experiment were very rare. Nonetheless, officers drew on their experiences during the experiment to control drug crime subsequently. The question relevant to our analyses is whether such experiences would affect experimental markets differently than control markets.

We have no reason to suspect such a bias in the strategies that would be used by the reconstituted teams. Officers were encouraged to share knowledge gained during the experiment, and such knowledge was expected to be used in approaching problems in whichever drug areas were targeted. Yet the fact that experimental officers were assigned to individual markets during the experiment may have caused differential enforcement in the control and the treatment sites during the post-intervention period. In conversations with officers after the experiment, it was clear that

[9] For this reason, and also because of the relatively low base rate of arrest information for most of the crimes examined, we do not present analyses of changes in arrests in the pre- and post-intervention periods.

[10] This impact cannot be measured directly by comparing levels of crime calls in the experimental and the control groups during the intervention period because potential deterrent effects and reporting biases would occur simultaneously. The actual rates of calls for the two groups were similar during the intervention period when we adjusted for differences in the pre-intervention period.

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some officers in the experimental group continued to focus their efforts on the experimental markets that were assigned to them during the study. The potential effect on our analyses, however, is difficult to estimate. If activity in the experimental markets increased as a result, we might expect an overall increase in official reporting of crime in those places. At the same time, more attention to specific markets may have increased the maintenance of gains made during the study.

In Table 3 we present the mean change (per hot spot) in number of calls within the experimental and the control groups comparing the pre- with the post-intervention period. We also display the statistical significance of the differences between the experimental and control conditions, accounting for the block-randomized design of the study.[11] We assess the changes according to specific call categories, and, in the case of disorder (for which we have four separate measures), across the overall category as well. To depict graphically effects of the treatments, we present a bar graph showing the total number of calls before and after the experiment for each of the significant comparisons.

Table 3. Mean Changes in Calls (per Hot Spot), Pre- Versus Post-intervention Periods (by Group)

Call Category	Mean Change per Group		Significance
	Experimental	Control	p<[a]
Violence	2.07	.96	.237
Property	−2.36	−5.86	.410
Disorder	9.14	25.39	.007
Nuisance	8.71	10.82	.121
Suspicious persons	−.11	5.96	.001
Public morals	−2.14	.89	.032
Assistance	2.68	7.71	.052
Narcotics	−5.18	.18	–[b]

[a] Significance value is derived through a mixed model ANOVA method (see note 11). Results of two-tailed test are reported.
[b] ANOVA results are unreliable and thus are not reported (see note 15).

The experiment did not influence calls for violent[12] or property offenses. For both call categories there was a slight improvement in

[11] We used a mixed-model analysis of variance, taking into account the direct effects of block and group (experimental versus control) variables, as well as the interaction between block and group to assess statistical significance in our study:

SS Total = SS Block + SS Group + SS (Block x Group)

We estimated the models using a unique sums of squares methods, in which the degrees of freedom were 15 for explained variance and 40 for residual variance.

Because the analysis of variance model was likely to be affected strongly by extreme values, we subjected each significant finding to tests for stability. Thus we examined the effects of removing and including blocks of cases, of transforming the distribution of events, and results gained by using less powerful rank-order techniques.

[12] The category includes violence against persons and interpersonal offenses.

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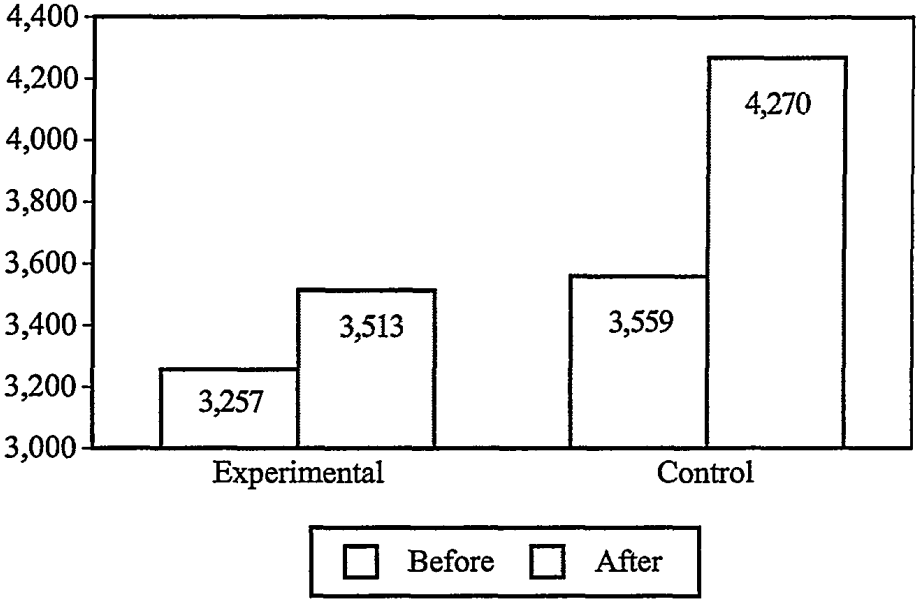
the control condition over the experimental condition, but the effect was not statistically significant. In the case of calls related to disorder, however, the experimental group improved in relation to the control group in all four call categories examined. Overall the impact of the treatment on disorder is statistically significant ($p<.01$): calls in the control group increased by more than 700, but in the experimental group by only 256 (see Figure 1). The fact that both groups registered more calls for disorder in the post-experimental period does not imply that drug enforcement generally increased disorder; rather, it reflects the presence of two summer months with high call activity in the post-intervention period that are not included in the pre-intervention period.[13]

Figure 1. Disorder Calls Pre- and Post-intervention



We found statistically significant differences between the experimental and the control conditions in three of the four disorder-related call categories.[14] In the case of suspicious persons, the experimental group remained relatively stable, with about 500 calls in both the pre- and the post-intervention periods (see Figure 2).

[13] Because the experiment lasted more than one year, the seven-month pre-intervention period begins in August and the post-intervention period in June. Although these seasonal differences make it difficult to assess change between the pre- and the post-intervention periods overall, they do not affect the validity of comparisons between the experimental and the control groups.

[14] Because of the relatively high risk of making a type II error (resulting from the small size of the study sample), we used a .10 significance threshold. Nonetheless, two of the measures are significant at $p<.05$, and another is very close to that threshold ($p=.052$).

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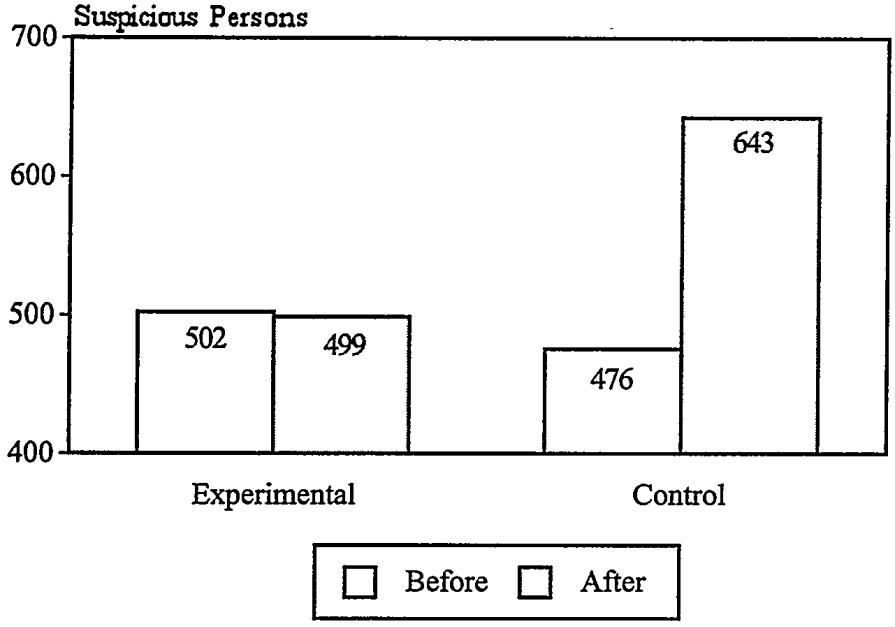
The number of calls in the control group, however, increased from 476 to 643. In the case of public morals (which includes gambling, lewdness, possession of liquor, and prostitution), the number of calls declined from 255 to fewer than 200 for the experimental group, but increased by 25 for the control group. In the much broader "assistance" category (which includes calls ranging from requests for an ambulance, to dead body, to complaints about animals, lost property, or assisting an invalid), both groups registered an increase in calls in the post-intervention period, but that increase was 216 calls in the control group and only 75 in the experimental group.

Figure 2. Calls Pre- and Post-intervention (Significant Comparisons by Group)



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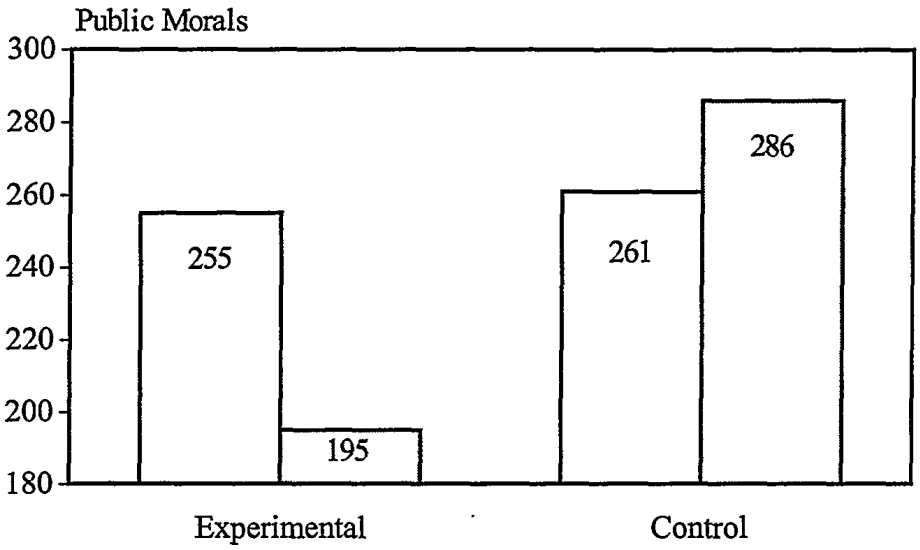

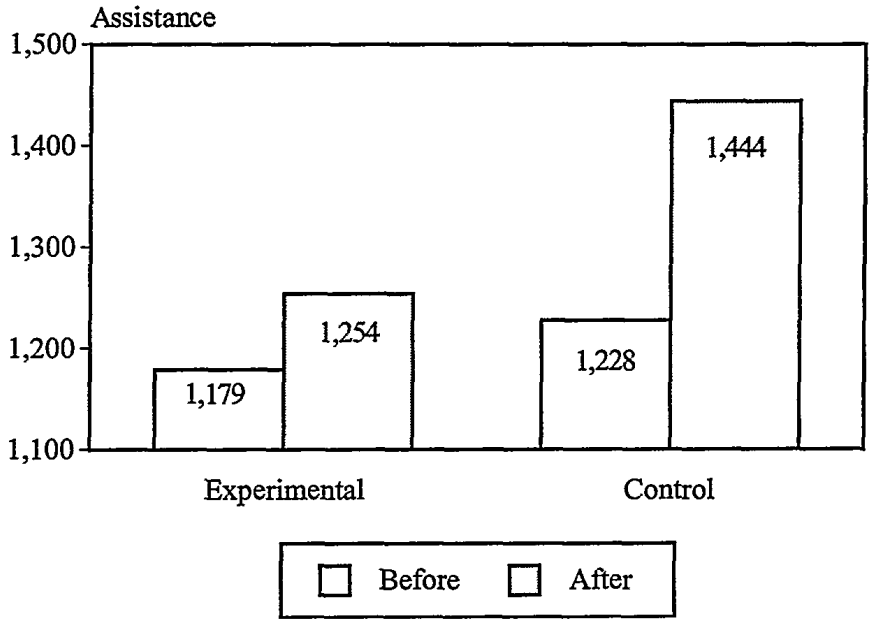

The influence of the experiment on disorder-related calls can be linked to its emphasis on cracking down on street-level drug and drug-related activity. Yet in view of the success of the treatment in influencing calls for disorder, it is surprising that we do not find a consistent effect on narcotics calls. Despite improvement in the experimental markets, the effect of the treatment is evident primarily in very large changes in a few of the most active hot spots included

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in the study.[15] A simple explanation for this finding is that the study affected the general level of disorder in the drug hot spots but did not influence drug activity itself. We suspect, however, that reporting of drug activity in the post-intervention period may have been influenced more strongly by the experimental treatments than were other call categories, a position that is supported by analyses of displacement and diffusion effects in the experiment.

DISPLACEMENT AND DIFFUSION

Interventions that focus on specific places or situations have often been criticized because of the threat of displacement (see, e.g., Reppetto 1976). In recent years this prevailing view, like many others in criminology, has drawn substantial criticism. The assumption that displacement is a routine effect of focused crime prevention efforts has been replaced by agreement that displacement is seldom total and often is inconsequential (Barr and Pease 1990; Clarke 1992; Eck 1993; Gabor 1990; Hesseling 1993). Clarke and Weisburd (1994) suggest that scholars need to recognize the reverse of displacement. They point to evidence suggesting that situational and place-oriented crime prevention strategies often lead to a "diffusion of benefits" to areas outside the immediate targets of intervention.

Although we recognize that displacement is often difficult to assess, in part because it can take so many forms (see Pease 1993), we attempted to measure potential displacement in the Jersey City DMA experiment, using two methods. First, we examined the spread of crime calls in the pre- and post-intervention periods in the areas immediately surrounding the drug hot spots defined in the study. Second, we replicated the process we used to define the drug hot spots in the pre-intervention period, using data from the post-intervention period. In this way we could identify potential new drug locations that emerged during or after the experiment.

The change in crime calls in the areas immediately surrounding the hot spots is the most straightforward measure of spatial displacement. We created a two-block displacement catchment area around each drug hot spot included in the study. Then we compared the overall arrest and call statistics in the pre- and post-intervention periods for the experimental and the control sites.

[15] The largest impacts occur in two of the five matched pairs of high-activity markets in the study. Overall we found an improvement of 150 calls in the experimental group, but these two blocks alone show an improvement of 227 calls. There was an improvement in only three of the seven statistical blocks in the experimental condition, as contrasted with the control condition. Because of the skew of these cases, ANOVA results based on the raw scores or log transformations of the call data provide unreliable estimates of significance.

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Because of the concentration of high-activity places in particular neighborhoods, displacement catchment areas sometimes overlap (see also Green 1995; Weisburd and Green 1995). Nevertheless, randomization of treatment and control hot spots allows us to assume that there is not a systematic bias in this regard, in favor of either the experimental or the control conditions.

We do not find evidence of greater displacement of emergency calls to the areas surrounding the experimental locations (see Table 4). At the same time, there is a diffusion of benefits into the experimental as compared with the control catchment areas in two call categories. For both public morals and narcotics, calls in the catchment areas for the experimental sites decrease significantly as compared with the control sites. In the case of public morals, we find a slight increase in calls in the catchment areas surrounding the control hot spots, but a decrease of more than 300 calls in the areas surrounding the experimental locations (see Figure 3). The difference in calls for narcotics offenses is even more pronounced: almost 150 fewer narcotics calls were made in the experimental catchment areas in the post-intervention period. In contrast, the number of calls in the control catchment areas for narcotics increased by more than 500.

Table 4. Mean Changes in Calls, Pre- Versus Post-intervention Periods, for Hot Spot Catchment Areas (by Group)

	Mean Change per Hot Spot		
Call Category	Experimental	Control	Significance p<[a]
Violence	27.32	33.32	.306
Property	–5.61	–10.89	.194
Disorder	70.86	121.96	.310
Nuisance	37.11	58.14	.628
Suspicious persons	–23.71	–19.79	.667
Public morals	–11.32	.68	.001
Assistance	21.36	43.36	.664
Narcotics	–5.25	18.14	.015

[a] Significance value is derived through a mixed model ANOVA method (see note 11). Results of a two-tailed test are reported.

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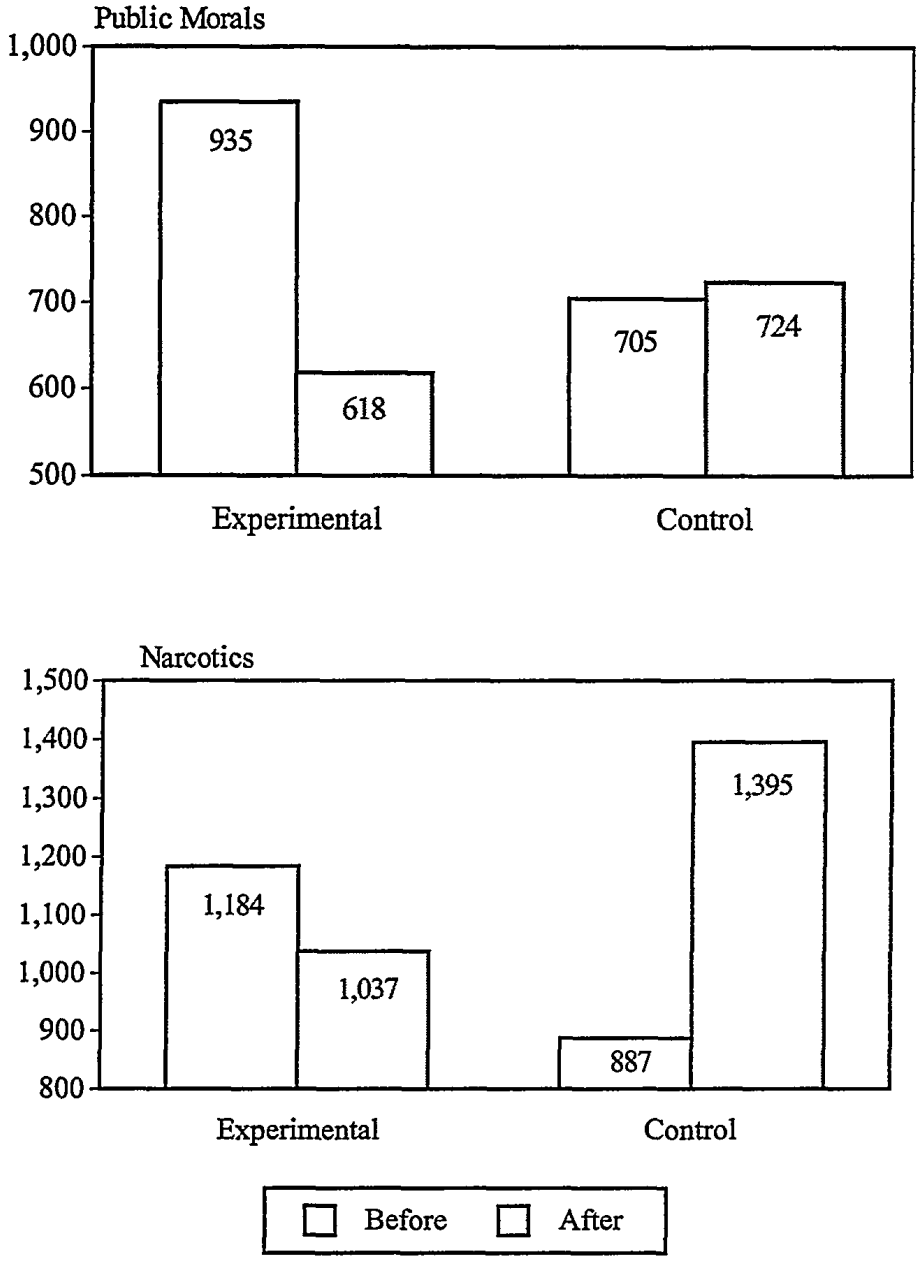
Figure 3. Calls Pre- and Post-intervention for Catchment Areas (Significant Comparisons by Group)



The impact of the experiment on narcotics calls in the catchment areas is particularly important in light of our inconsistent finding for narcotics in analyses of the main effects of the study. We think it unlikely that drug problems were influenced in the catchment areas but not in the hot spots themselves. Rather, we suspect that the impact of the experiment on narcotics calls in the hot spots

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may have been masked by changes in crime call reporting caused by the experimental interventions. For example, officers often encouraged people who lived or worked within the hot spots to report drug activity to the police through the 911 call system. Though our data do not allow us to examine directly the effect of the experimental strategy on calls for drug offenses, we think it reasonable that the biases that developed would have lasted into the post-intervention period.

In our examination of newly emerged drug hot spots, our findings provide additional support for the overall crime control benefits of the experimental strategy. Using a process similar to that employed in defining the original hot spots,[16] we find some displacement of activity into the areas near the experimental sites. Nonetheless, displacement of this type was almost twice as likely to occur around a control as an experimental location (see Table 5). The experimental hot spots generated 19 new active locations within one block, as contrasted with 36 in the control hot spots. In the post-intervention period we found six additional new sites within two blocks of experimental hot spots, as contrasted with nine in the control group.

Table 5. Identifying New Drug Areas in the Post-Intervention Period

N of Street Segments	Location of Segments
19	Within 1 block of experimental drug hot spot
36	Within 1 block of control drug hot spot
6	Between 1 and 2 blocks of experimental hot spot
9	Between 1 and 2 blocks of control drug hot spot
37	Time 1 segment, excluded because of low activity (see note 3)
18	Other newly identified segments

Fifty-five of the newly identified intersections and segments would be candidates for new hot spots, according to our original criteria.[17] We also identified 37 of these as active intersections and street segments in the pre-intervention period, but they did not meet subsequent threshold requirements. These numbers suggest some displacement to other areas of the city during the experiment.

[16] Because tip-line data were not available in the post-intervention period, the identification process used here was based only on arrests and emergency calls.

[17] The next step in the drug hot spot identification process would have been to link the segments according to the rules described earlier.

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Even so, we cannot determine whether these new locations developed as a result of activity by the control squad or by the experimental squad or indeed whether they would have developed even without any enforcement efforts. Whatever the cause, these active street segments and intersections would have generated only relatively few new hot spots. A maximum of 10 new markets could have been constructed from these data, and we estimate that five of these would have been excluded after verification of activity.[18]

CONCLUSIONS

The DMA experiment in Jersey City focused on street-level drug hot spots. Through a stepwise enforcement strategy that was matched in form and intensity to the characteristics of the drug places identified, the experimental program sought to crack down on active drug areas. In Step 1 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 2 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 earlier gains through continued monitoring of activity in the treated locations.

In comparing seven-month pre- and post-intervention periods, we found consistent, strong effects of the experimental strategy on disorder-related emergency calls for service. We also found little evidence that the crime control benefits of the study were displaced to areas near the experimental hot spots. Indeed, our results suggest a diffusion of benefits in the experimental locations, as compared with the control locations.

In conclusion, we focus on three main implications of our findings.

First, police can be more effective when they take a more specific approach to crime and disorder. There is little evidence that strategies of crime control, broadly defined, do much to solve crime problems. However, a growing body of research suggests that specific crime prevention efforts, whether by police or by others, can succeed in preventing or controlling crime (see Clarke 1992 for reports on 22 such efforts). Our findings, developed from a randomized field trial, support this position. They also point to the importance of focusing on specific places as well as on specific types of crimes (also, see Sherman and Weisburd 1995).

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[18] See note 3.

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Although a focused approach to crime is warranted, the actions of police when they target specific problems or places are also important. Both the experimental and control conditions in our study concentrated on specific drug hot spots. Yet a systematic strategy adapted to the characteristics of the hot spots provided for more effective law enforcement. The idea that what the police do is important is very much a part of recent developments in problem-oriented policing (see Goldstein 1990). Our study does not directly test problem-oriented policing, but it provides evidence that tailor-made responses to problems are essential if police are to deal more effectively with crime and crime-related problems.

Finally, enforcement efforts focused on specific places do not necessarily cause displacement of crime problems to surrounding areas. Our study supports the position that displacement is seldom total and is often inconsequential (Clarke 1992; Eck 1993). We found on average that the experimental hot spots were less likely to show displacement than the control locations. Nonetheless, our study indicates the development of some new hot spots in the city in the intervention and post-intervention periods. Whether this number is less or more than would have developed naturally, or is a result of the experimental or the control treatments, could not be gleaned from our data.

Most Americans define the police role primarily in terms of crime control, and look to the police for leadership in crime prevention efforts. To the public, more police means more safety, regardless of the philosophy and tactics that police employ. In contrast, a number of scholars question whether the police can have any impact on crime and crime-related problems regardless of their approach to policing (see, e.g., Gottfredson and Hirschi 1990). Our study suggests a position somewhere between these two contrasting visions of police effectiveness in combating crime and disorder. If police are to affect crime and disorder problems, they must define focused crime prevention efforts that are as varied as the phenomena they seek to address.

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Appendix. Selected Characteristics of Hot Spots, by Group

Characteristics	Experimental	Control
# Indoor Markets	5.0	3.0
# Outdoor Markets	23.0	25.0
Average # Segments	3.3	3.5
Mean Age of Sellers	20.4	17.7
% African-American Arrestees	66.6	62.8
% Male Arrestees	82.7	76.7
# Cocaine Markets	17.0	12.0
# Heroin Markets	2.0	4.0
# Marijuana Markets	2.0	1.0
# Mixed Markets	4.0	6.0
# Citizen-Generated	3.0	5.0
# in North District	5.0	4.0
# in South District	11.0	6.0
# in East District	5.0	4.0
# in West District	7.0	14.0
Mean # Narcotics Arrests	15.6	14.8
Mean # Narcotics Calls	21.8	18.5
Mean # Tips Before	0.5	0.6
% African-American Residents	52.9	52.1
% One-Family Homes	11.3	13.5
% Residents under 18	31.2	29.3
% Housing Occupied	90.6	87.9
% One Person Head of Household	24.1	28.8
% Owner-Occupied	24.7	25.7

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