POLICING CRIME AND DISORDER HOT SPOTS: A RANDOMIZED CONTROLLED TRIAL*

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Dealing with physical and social disorder to prevent serious crime has become a central strategy for policing. This study evaluates the effects of policing disorder, within a problem-oriented policing framework, at crime and disorder hot spots in Lowell, Massachusetts. Thirty-

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four hot spots were matched into 17 pairs, and one member of each pair was allocated to treatment conditions in a randomized block field experiment. The officers engaged "shallow" problem solving and implemented a strategy that more closely resembled a general policing disorder strategy rather than carefully designed problem-oriented policing responses. Nevertheless, the impact evaluation revealed significant reductions in crime and disorder calls for service, and systematic observations of social and physical disorder at the treatment places relative to the control places uncovered no evidence of significant crime displacement. A mediation analysis of the isolated and exhaustive causal mechanisms that comprised the strategy revealed that the strongest crime-prevention gains were generated by situational prevention strategies rather than by misdemeanor arrests or social service strategies.

KEYWORDS: problem-oriented policing, hot spots, disorder, broken windows

Crime policy scholars, primarily James Q. Wilson and George L. Kelling, and practitioners, such as Los Angeles Police Chief William J. Bratton, have argued for years that when police pay attention to minor offenses—such as aggressive panhandling, prostitution, and graffiti—they can reduce fear, strengthen communities, and prevent serious crime (Bratton and Kelling, 2006; Wilson and Kelling, 1982). Spurred by claims of large declines in serious crime after the approach was adopted in New York City, dealing with physical and social disorder, or "fixing broken windows," has become a central element of crime-prevention strategies adopted by many American police departments (Kelling and Coles, 1996; Sousa and Kelling, 2006). The general idea of dealing with disorderly conditions to prevent crime is found in a myriad of police strategies that range from "order maintenance" and "zero tolerance" policing strategies in which the police attempt to impose order through strict enforcement to "community" and "problem-oriented policing" strategies in which police attempt to produce order and reduce crime through cooperation with community members and by addressing specific recurring problems (Cordner, 1998; Eck and Maguire, 2000; Skogan, 2006; Skogan et al., 1999). Although its application can vary within and across police departments, policing disorder to prevent crime is now a common crime-control strategy.

The available research evidence, however, does not demonstrate consistent connections between disorder and more serious crime (Harcourt, 1998; Sampson and Raudenbush, 1999; Skogan, 1990; Taylor, 2001). Evaluations of the crime-control effectiveness of policing disorder strategies also yield conflicting results. In New York City, for example, it is unclear

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whether "broken windows" policing can claim any credit for the 1990s crime drop (Eck and Maguire, 2000; Karmen, 2000) with evaluations reporting significant reductions in violent crime (Corman and Mocan, 2005; Kelling and Sousa, 2001), modest reductions in violent crime (Messner et al., 2007; Rosenfeld, Fornango, and Rengifo, 2007), and no evidence of reductions in violent crime (Harcourt and Ludwig, 2006). These conflicting results have generated questions regarding the crime-prevention value of dealing with physical and social disorder. As others have observed (e.g., Harcourt and Ludwig, 2006; Skogan and Frydl, 2004), given the strong influence of broken windows on the policing field, remarkably little solid research evidence is found on the crime-control benefits of policing disorder.

Most prior studies that examine the effectiveness of broken windows policing in preventing crime suffer from two important limitations. First, many evaluations engage nonexperimental and quasi-experimental designs that infer causation by observing changes in police actions and attempting to account for rival causal factors through statistical controls and elaborate model specification exercises. Although these efforts are laudable, randomized controlled experiments remove many uncertainties associated with the other approaches through their high internal validity and strong ability to demonstrate the effect of one factor over another. Second, most studies use increased numbers of misdemeanor arrests as a proxy measure for policing disorder interventions or a simple dummy variable to represent a package of policing disorder interventions. As will be discussed, dealing with disorderly conditions requires an array of activities, such as securing abandoned buildings, removing trash from the street, and managing homeless populations, which are not captured in one-dimensional misdemeanor arrest measures. Although dummy variables can represent a set of police actions that constitute a policing disorder strategy, such analyses do not unravel the key elements of the strategy that may or may not be associated with observable changes in crime. This study advances our knowledge on the effects of policing disorder on crime by using a randomized block experimental design in conjunction with qualitative indicators on local dynamics to evaluate the effects of policing disorder at crime and disorder hot-spot locations in Lowell, Massachusetts. The study also sheds important insights on the causal pathways of key crime-prevention mechanisms associated with policing disorder approaches: increased misdemeanor arrests, situational prevention strategies, and social service actions.

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THE CRIME-CONTROL EFFECTIVENESS OF POLICING DISORDER

Although local officials and national observers attribute the violent crime drop in New York in the 1990s to the adoption of the broken windows policing strategy, many academics argue that it is very difficult to credit this specific strategy with the surprising reduction in violent crime. The New York Police Department (NYPD) implemented the broken windows strategy within a larger set of organizational changes framed by the Compstat management accountability structure for allocating police resources (Silverman, 1999). As such, it is difficult to establish the independent effects of broken windows policing relative to other strategies implemented as part of the Compstat process (Weisburd et al., 2003). Other scholars suggest that several rival causal factors, such as the decline in New York's crack epidemic, played a more important role in the crime drop (Blumstein, 1995; Bowling, 1999). Some academics have argued that the crime rate was already declining in New York before the implementation of any of the post-1993 police reforms and that New York's decline in homicide rates was not significantly different from declines experienced in surrounding states and in other large cities that did not implement aggressive enforcement policies during that time period (Karmen, 2000; Eck and Maguire, 2000).

Because the NYPD implemented its post-1993 changes as a citywide crime-prevention strategy, it was not possible for evaluators to engage a rigorous evaluation design such as the "gold standard" randomized controlled experiment (Campbell and Stanley, 1966; Cook and Campell, 1979). However, a recent series of sophisticated statistical analyses have examined the effects of policing disorder on violent crime trends in New York City (Corman and Mocan, 2005; Harcourt and Ludwig, 2006; Kelling and Sousa, 2001; Messner et al., 2007; Rosenfeld, Fornango, and Rengifo, 2007). These studies represent very careful attempts to determine whether broken windows policing can be associated with the crime drop in New York City by statistically controlling for rival causal factors, such as the decline in New York's crack epidemic and relevant sociodemographic, economic, and criminal justice changes over the course of the 1990s. These studies generally can be distinguished by differences in modeling techniques, dependent variables, time-series length, extensiveness of control variables included in the analysis, the functional form of control variables, and measurement levels (e.g., precincts versus boroughs). These studies commonly use increases in misdemeanor arrests or combined ordinance-violation and misdemeanor arrests (Rosenfeld, Fornango, and Rengifo, 2007) as the key measures of the NYPD policing disorder strategy. With the exception of the Harcourt and Ludwig (2006) study, these analyses

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have found statistically significant associations between the NYPD strategy and decreased violent crime, with the effects ranging from small (Messner et al., 2007; Rosenfeld, Fornango, and Rengifo, 2007) to large (Corman and Mocan, 2005; Kelling and Sousa, 2001).[1]

As Kelling and Sousa (2001) admit, the number of misdemeanor arrests is a very limited measure of the content of broken windows policing strategies. Increasing misdemeanor arrests for panhandling, smoking marijuana, and public drinking represents only one dimension of maintaining public order on the street and does not capture other actions that are necessary to deal with disorderly neighborhood conditions that generate serious crime problems. A broader set of responses to deal with physical and social incivilities, such as installing improved street lighting, cleaning up vacant lots, razing abandoned buildings, and evicting problem residents, requires activities that go far beyond making misdemeanor arrests. Strategic partnerships with city agencies, social service agencies, local business owners, community groups, and tenant associations are often necessary to deal with physical deterioration and social order problems in neighborhoods (Braga, 2002; Skogan, 2006; Taylor, 2006). In addition to the uncertainties associated with determining causal effects in nonexperimental research designs, the limited measurement of policing disorder treatments through one-dimensional misdemeanor arrest proxies obscures crime-prevention benefits that may be associated with the full approach. To estimate the effectiveness of multidimensional programs properly and to understand the causal effects associated with varying mechanisms, the specific treatments in the program need to be identified and accounted for in an evaluation design (Rossi, Lipsey, and Freeman, 2006).

Supporters of broken windows approaches (e.g., Bratton and Kelling, 2006) point to one experimental evaluation as the strongest available evidence that policing disorder strategies have considerable crime-prevention value. In Jersey City, New Jersey, a randomized controlled experiment found that a problem-oriented policing strategy focused on social and physical disorder resulted in significant reductions in citizen calls for service and crime incidents in violent crime hot spots with little evidence of immediate spatial displacement (Braga et al., 1999). However, a key shortcoming of this study involved the use of a dummy variable to represent the

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1.   Other macrolevel analyses have generated results supportive of policing disorder strategies. In California, controlling for demographic, economic, and deterrence variables, a county-level analysis revealed that increases in misdemeanor arrests were associated with significant decreases in felony property offenses (Worrall, 2002). An analysis of robbery rates in 156 American cities revealed that increased arrests for disorderly conduct and driving under the influence reduced the number of robberies (Sampson and Cohen, 1988).

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set of police actions that comprised the policing disorder intervention.[2] Twenty-eight different types of problem-oriented responses were implemented to alleviate crime and disorder problems at the treatment violent crime hot spots; these responses included increased misdemeanor arrests for public drinking, boarding and fencing abandoned buildings, code inspections of taverns and apartment buildings, and finding shelter and substance abuse treatment for the homeless (Braga et al., 1999). It is unknown whether the crime-control gains were generated by arrest-based order maintenance tactics, situational prevention strategies that modified the criminal opportunity structure at treatment places, or social service strategies that attempted to create opportunities for high-risk individuals that populated the targeted locations. Given the widespread popularity of broken windows policing, considerable need exists to conduct additional rigorous evaluations of its crime-control effectiveness and to develop some much needed empirical evidence on the key elements of the approach that generate observable preventive benefits.

PROGRAM DESIGN

The program and evaluation design of this study borrows from the Braga et al. (1999) study that generally followed the well-known steps of the scanning, analysis, response, and assessment (SARA) model used in many problem-oriented policing projects (see Eck and Spelman, 1987). Lowell, Massachusetts, is a small city of some 105,000 residents located about 30 miles northeast of Boston. During the scanning phase, computerized mapping and database technologies were used to geocode all 2004 crime and disorder emergency citizen calls for service and to identify the densest clusters of these calls in Lowell.[3] Simple temporal analyses and

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2. Two other studies used dummy variables to represent a policing disorder intervention. A quasi-experimental evaluation of a quality-of-life policing initiative that focused on social and physical disorder in four target zones in Chandler, Arizona, did not find any significant reductions in serious crime associated with the strategy (Katz, Webb, and Schaefer, 2001). A less rigorous evaluation of a 1-month police enforcement effort to reduce alcohol-related and traffic-related offenses in a community in a midwestern city also did not find any significant reductions in the amount of robberies or burglaries that took place in the targeted area (Novak et al., 1999).

3. The 2004 crime and disorder calls for service were geocoded using Mapinfo Professional 8.0 mapping software (Pitney Bowes, Troy, NY). The spatial distribution of citizen crime and disorder calls for service was examined using Spatial and Temporal Analysis of Crime (STAC) and kernel density analytic tools available from the National Institute of Justice and through Mapinfo's Vertical Mapper software. These analytic tools identified the locations of the densest clusters of calls in each of Lowell's eight neighborhoods: Pawtucketville, Centralville, Belvidere, South Lowell, The Highlands, Back Central, Downtown, and The Acre. These clusters were digitized (polygons were drawn manually around

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ranking procedures were used to identify preliminary hot-spot areas that had consistently high levels of citizen crime and disorder calls for service over time. Qualitative data on place characteristics, local dynamics, and Lowell Police Department (LPD) patrol officer perceptions of crime problems were used to determine hot-spot area boundaries. To measure immediate spatial crime displacement and "diffusion of crime-control benefits" effects, all final hot-spot areas were required to include a two-block catchment area around the perimeter of the place.[4] This process left 34 discrete crime and disorder hot-spot areas in Lowell for inclusion in the experiment. The hot spots accounted for 2.7 percent of Lowell's 14.5 square miles. In 2004, these places generated 5,125 citizen calls for service (23.5 percent of 21,810 total crime and disorder calls to the LPD), including 1,214 violent crime calls (29.3 percent of 4,140 total violent crime calls to the LPD), 1,942 property crime calls (25.1 percent of 7,725 total property calls to the LPD), and 1,969 disorder calls (19.8 percent of 9,945 total disorder calls to the LPD). After the 34 violent crime places were identified, they were matched into 17 pairs for evaluation purposes (i.e., to be allocated to control and treatment groups).[5]

STAC ellipses and the darkest portions of the kernel density grids) into a boundary file of preliminary hot-spot locations. The details of the hot-spot identification process are available by request from the authors.

4. The two-block catchment area was borrowed from other studies designed to measure immediate spatial displacement and diffusion (e.g., Braga et al., 1999; Weisburd and Green, 1995b). As Weisburd and Green (1995a: 354) describe, "we decided upon a two-block radius for the 'catchment' area because we felt it a reasonable compromise between competing problems of washout of displacement impact and a failure to provide adequate distance to identify immediate spatial displacement. While we recognized at the outset that we would miss the movement of crime more than two blocks away from a hot spot, given our measure of crime as a general rather than specific indicator we did not think it practical to identify all potential places that might provide opportunity for displaced offenders" (see also Green, 1995; Weisburd et al., 2006).

5. Simple but deliberate matching exercises ensure that any peculiarities found in one sample will most likely occur in the other as well (see Blalock, 1979; Rossi, Lipsey, and Freeman, 2006). Our matching method was primarily a qualitative exercise informed by simple quantitative analyses of the official crime data. The 34 violent crime places were grouped initially based on similar numbers of 2004 crime and disorder calls. Within these groups, using the qualitative information gathered by the officers during the scanning phase, places were compared by the types of problems at the place (e.g., street fights vs. shoplifting), the known dynamics of the place (e.g., presence of disorderly groups or an active drug market), and the physical characteristics of the place (e.g., presence of park or school). Final matches were made based on the degree of similarity across these key qualitative dimensions. Sociodemographic data for the places were considered during the matching process but did not provide much additional information about the places because most locations were in minority, low-income neighborhoods.

The analysis phase of the problem-oriented policing program started with the random allocation of the initial places for treatment. The 17 pairs of places were presented to LPD Superintendent Edward F. Davis III, and a coin was flipped by the research team to determine randomly which of the places within the pair would receive the problem-oriented policing treatment. The locations that were not selected from each of the pairs were control places.[6] On September 1, 2005, Superintendent Davis assigned ultimate responsibility for the implementation of the problem-oriented policing intervention at the treatment places to the captains that managed Lowell's three police sectors. After receiving their assigned treatment hot spots, the captains were required to submit a report for each place that detailed the results of their problem analyses and listed situational and enforcement responses that were linked logically to the underlying conditions that caused these problems. Within each sector, lieutenants and sergeants spent time analyzing official data sources and discussing problems with community members. The intervention period lasted for 1 year, officially ending on August 31, 2006.

The captains were held accountable for the implementation of the problem-oriented policing interventions through a monthly Compstat-like process (Moore and Braga, 2003; Silverman, 1999; Weisburd et al., 2003). The activities at the monthly meetings represented an ongoing SARA process. At each monthly meeting, the LPD Crime Analysis Unit presented simple trend analyses of citizen calls for service in each of the treatment hot spots to determine whether crime and disorder problems were being positively impacted. If the data revealed that calls for service were decreasing in their hot spots, then Superintendent Davis praised the captains and their officers for their hard work and asked them to explain why they believed their actions were producing the desired effects and what else could be done to keep calls for service decreasing. If the analysis revealed that the number of citizen calls for service had remained the same or increased, then Superintendent Davis peppered the captains with questions about their plans for dealing with recurring problems (i.e., whether they were making use of particular activities, such as increased order maintenance approaches, and alleviating identified physical disorder problems). Careful notes on the implemented interventions discussed in these meetings and observed at treatment locations during weekly researcher ride alongs were maintained by the research team.

The resulting treatment was a collection of specific problem-oriented tactics that could be broadly categorized as a "policing disorder" strategy. Like many problem-oriented policing projects, the problem analysis

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6.   A table that presents a comparison between the control and treatment places on key qualitative dimensions is available by request from the authors.

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engaged by the LPD was generally weak with many initiatives accepting the definition of a problem at face value, using only short-term data to unravel the nature of the problem, and failing to examine the genesis of the crime problems adequately (Clarke, 1998; Read and Tilley, 2000; Scott, 2000). As result, the interventions implemented by the LPD officers were much less nuanced than the carefully designed responses advocated by scholars such as Ronald V. Clarke (1997) and Herman Goldstein (1990). The experiences of the LPD officers closely reflect the nature of problem solving and problem-oriented policing as it is currently practiced in the field. Many academics have considered the gap between the rhetoric and the reality of problem-oriented policing and have observed that many projects generate interventions that could be called "shallow" problem-solving responses (Braga and Weisburd, 2006; Cordner and Biebel, 2005; Eck, 2006).

Situational interventions broadly designed to modify disorderly conditions at a place were implemented at all 17 treatment places. On average, 4.4 situational strategies were implemented per place (range = 2 to 8 strategies per place). The strategies varied according to the nuances of the problems at places (e.g., cleaning and securing vacant lots, razing abandoned buildings, improving street lighting, adding video surveillance, performing code inspections of disorderly taverns, and the like). The LPD officers also implemented "social service" strategies at 12 treatment places, such as connecting problem tenants suffering from mental health problems to social workers, working with local shelters to provide housing for homeless individuals, and increasing youth recreational opportunities in local parks. On average, one social service strategy was implemented per place (range = 0 to 2 strategies per place). All treatment locations also experienced several aggressive order maintenance interventions to control the social disorder of the place. These tactics included making repeat foot and radio car patrols, dispersing groups of loiterers, making arrests for public drinking, arresting drug sellers, and performing "stop and frisks" of suspicious persons. The weekly mean number of misdemeanor arrests in the treatment places significantly increased by 17.7 percent from a preintervention mean of 12.9 arrests per week to a mean of 15.2 arrests per week during the intervention period (t (76) = 2.356; p = .021).

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The control hot-spot areas were not identified to the captains. Over the course of the experiment, they had no knowledge of control-area locations. As such, these places experienced the routine amount of police strategies that such areas in Lowell would experience without focused intervention—arbitrary patrol interventions, routine follow-up investigations by detectives, and ad hoc problem-solving attention. To monitor

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police intervention at the control locations, a researcher attended the LPD's monthly citywide Compstat meetings (see Willis, Mastrofski, and Weisburd, 2004). Field researchers also visited the control hot spots on a monthly basis to monitor whether changes in the physical conditions or social dynamics at the place were observable. The research team maintained notes on all situational prevention and social service actions taken by the LPD at the control hot spots. Researchers noted situational strategies at 9 control places (mean = .7 per place; range = 0 to 2) and social service strategies at 7 control places (mean = .4 per place; range = 0 to 1). Although several instances of short-term problem-solving actions were found in the control places that resembled the interventions being applied to treatment hot spots, none of these actions involved sustained, focused attention over the course of the experiment. The number of misdemeanor arrests in the control places also did not significantly change over the course of the intervention. Misdemeanor arrests decreased by 6.8 percent from a preintervention mean of 12.5 arrests per week to a mean of 11.7 arrests per week during the intervention period $[t\ (76) = -.709; p = .480]$.

IMPACT EVALUATION DATA AND ANALYTICAL TECHNIQUES

Our study used citizen emergency calls for service data as official indicators of crime and used systematic observation techniques to collect pretest and posttest data on physical and social incivilities at the treatment and control places. Although call data are widely used for assessing trends and patterns of crime (see, e.g., Sherman and Weisburd, 1995; Weisburd and Green, 1995b), these data do have shortcomings. Call data are subject to both underreporting (e.g., a lack of phones in poverty-stricken places) and overreporting (e.g., five separate calls reporting the same incident risk being counted as five distinct events; see Klinger and Bridges, 1997; Sherman, Gartin, and Buerger, 1989). Call data, however, are suggested to be more reliable measures of crime and crime-related activity than incident data or arrest data (Pierce, Spaar, and Briggs, 1988; Sherman, Gartin, and Buerger, 1989). Most notably, citizen calls for service are affected less heavily by police discretion than other official data sources (Warner and Pierce, 1993). Therefore, call data are regarded as "the widest ongoing data collection net for criminal events in the city" (Sherman, Gartin, and Buerger, 1989: 35; but see Klinger and Bridges, 1997).

Like many evaluations of crime-prevention initiatives implemented at specific crime hot-spot areas (e.g., Braga et al., 1999; Sherman and Weisburd, 1995), this study developed alternative performance measures to detect potential changes in social and physical disorder at treatment places

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relative to control places. For both social observation and physical observation data, our methodologies were derived from a developing literature that suggests places have standing patterns of behavior or rhythms of recurring behavior and activity that are somewhat predictable and routine (see Felson, 2006; Taylor, 1997). Green Mazerolle, Kadleck, and Roehl (1998) suggest that the reliability and validity of onsite observations increase as the unit of analysis decreases. Their research proposes that street blocks and other small units of analysis have fewer and less complex patterns of street activity than neighborhoods, communities, or other larger units of analysis that have more complex and varied patterns of social behavior. For both pretest and posttest time periods, it is important to note that the field researchers were not aware of whether they were conducting social and physical observations in control or treatment places.

During the pretest and posttest periods, maps of each control and treatment hot-spot area were created, and every block face that comprised each hot spot was photographed. For example, if the hot spot consisted of a street intersection area with four adjoining street segments, then the researcher took a photograph of each of the four blocks that comprised the area. To ensure consistency in data collection, the pictures were always taken at the midpoint of the block from the opposite side of the street. The resulting pictures were viewed, and the physical characteristics of the places were reproduced onto maps. All physical disorder at the place was recorded; this included abandoned buildings, vacant lots, trash, graffiti, abandoned cars, and other physical incivilities. These maps were coded and entered into a database for analysis. To ensure coder reliability, three trained research assistants separately viewed and coded the photographs and maps. Subsequent analysis revealed no significant differences in the perception of physical characteristics among the three coders for pretest or posttest data.[7]

Social observation data were collected at both control and treatment places to examine variations in social incivilities such as drinking in public and loitering. Systematic social observations have long been used in criminological research to understand and measure deviant behavior (see, e.g., Sampson and Raudenbush, 1999; Reiss, 1971; Weisburd et al., 2006). The objective of the social observations was to get a measure of the amount and types of social activity that occurred in the places during times they

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7.   For instance, for total counts of physical incivilities per place, $r = .89$ for coders 1 and 2, $r = .93$ for coders 1 and 3, and $r = .88$ for coders 2 and 3. For all correlations $p < .05$, this measure suggested a high degree of agreement among the coders.

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were known to be criminally active. These data were collected at both con-
trol and treatment places during the pretest and posttest periods by mak-
ing three separate visits to each place. Citizen calls for service data at each
place were analyzed for temporal variations in criminal activity. All places
were visited for 5 minutes at the time of day (morning, afternoon, or
night) and day of the week that the location was most active. For example,
if the call data indicated that the place was active only at night, then all
three observations occurred at night. However, if the place was active
mostly at night but also during the afternoon, then the area was visited
twice at night and once in the afternoon. Drawing on the methods used by
Sherman and Weisburd (1995), a researcher, driven by a plainclothes
police officer in an unmarked car, parked in an unobtrusive area that had
a clear view of the place's "epicenter" of activity (1995: 636). After park-
ing, the researcher counted the number of people engaged in disorderly
activities, such as loitering and drinking in public, over the next 5 minutes.
These data then were coded and entered into a database for analysis.

ANALYZING RESULTS

Main Effects

Randomized experimental designs allow researchers to assume that the
only systematic difference between the control and treatment groups is the
presence of the intervention; this assumption permits a clear assessment of
causes and effects (Campbell and Stanley, 1966; Cook and Campbell, 1979;
Sechrest and Rosenblatt, 1987). This randomized trial tested the overall
effectiveness of policing disorder at treatment places as compared with
control places. To assess the effects of the policing disorder intervention
on the treatment places relative to the controls, citizen calls for service
were compared for 6-month preintervention and postintervention periods.
The intervention period was not examined because the incidence of calls
presumably was biased by the strategies implemented at the treatment
places. For example, community members at the treatment places were
strongly encouraged by the officers to report criminal activity. The
randomization procedure allows the assumption to be made that no sys-
tematic differences occurred in the policing activities between treatment
and control groups during the 6 months before the experiment.

A randomized complete block design was used to assess the main effects
of the intervention on citizen calls for service. Thirty-four places were
matched into 17 homogeneous blocks, and one member of each block was
then randomly allocated to treatment conditions. The blocking process

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increases the power of the experimental design to reject the null hypothesis when an effect is actually present (Weisburd, 1993).[8] According to Daniel (1974), "the objective in using the randomized complete block design is to isolate and remove from the error term the variation attributable to blocks, while assuring that the treatments will be free of block effects" (1974: 198). The block effects were treated as fixed because the blocks did not represent a random sample of the population. Although it was necessary to control for block to examine treatment effects, the block effect was not of substantive interest in this analysis. A significant result for block would only indicate that the matching procedure did well in finding homogeneous blocks that differed from each other.

The pretest and posttest counts of citizen emergency calls for service in the treatment and control places were distributed in the form of rare event counts. Well-documented problems are associated with treating event count variables, which are discrete, as continuous realizations of a normal data-generating process (Gardner, Mulvey, and Shaw, 1995; King, 1989). Rather, Poisson and negative binomial regression models are generally used to estimate models of the event counts (Long, 1997). Chi-square goodness-of-fit tests run after exploratory Poisson regression models revealed that the counts of assault calls, robbery calls, and burglary/breaking and entering calls were distributed as Poisson processes and that the larceny/theft calls, disorder/nuisance calls, and total calls were distributed as negative binomial processes.[9] The basic analytic model was as follows:[10]

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8. Statistical power is a very complex problem, especially in experimental research. Power estimates are often based simply on the number of cases in the study. By this measure, our estimate for power is relatively low. Using a standard sign test with 34 cases (alpha = .05, two tails), our statistical power to detect a small effect size was about .24; a medium effect size was about .56; and large effect size was about .99 (Lipsey, 1990). However, as Weisburd (1993) points out, the number of cases is often a misleading measure. He finds that the smaller the experiment, the better control of variability in treatment and design. Statistical power may be, in fact, larger than expected.

9. The tests confirmed Poisson distributions by failing to reject the null hypothesis that no difference is found between the observed distribution and a Poisson distribution for assault calls ($X^2 = 21.576$ with d.f. = 15; $p = .119$), robbery calls ($X^2 = 8.714$ with d.f. = 15; $p = .891$), and burglary/breaking and entering calls ($X^2 = 7.498$ with d.f. = 15; $p = .942$). The tests confirmed negative binomial distributions by rejecting the null hypothesis that no difference is found between the observed distribution and a Poisson distribution for larceny/theft calls ($X^2 = 25.677$ with d.f. = 15; $p = .041$), disorder/nuisance calls ($X^2 = 45.053$ with d.f. = 15; $p = .000$), and total calls ($X^2 = 59.229$ with d.f. = 15; $p = .000$).

10. We realized that the number of events in the pretest period should be treated as a random effect. Because only a single measurement was made during each period, sufficient degrees of freedom were not found for the estimation of a random effect. Therefore, we concluded that a fixed-effects model was more appropriate for these data.

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Count of Call Events in Posttest = Intercept + Effect Due to Group +
Effect Due to Block + Count of Call Events in Pretest + Error

STATA 8.2 statistical software (StataCorp, College Station, TX) was
used to calculate the maximum likelihood estimate of the parameters for
each group (treatment relative to control conditions) and to compute the
associated probability values; this calculation provided estimates of the
effects of the problem-oriented policing treatment at the experimental
locations as compared with the control locations. The group parameters
were expressed as incidence rate ratios (i.e., exponentiated coefficients).
Incidence rate ratios are interpreted as the rate at which things occur; for
example, an incidence rate ratio of .90 would suggest that, controlling for
other independent variables, a 1-unit increase in the selected independent
variable was associated with a 10 percent decrease in the rate at which the
dependent variable occurs. To ensure that the coefficient variances were
robust to violations of the homoskedastic errors assumption of linear
regression models, Huber/White/sandwich robust variance estimators
were used. Following social science convention, the two-tailed .05 level of
significance was selected as the benchmark to reject the null hypothesis of
"no difference."

To unravel specific crime-prevention pathways at work in the Lowell
policing disorder strategy, we statistically examined the isolated and
exhaustive causal mechanisms that comprised the overall treatment strat-
egy and their independent effects on total citizen calls for service in treat-
ment places relative to control places (Morgan and Winship, 2007; Pearl,
2000). The three isolated and exhaustive crime-prevention mechanisms
examined were misdemeanor arrests, situational prevention strategies, and
social service strategies in both treatment and control places. Mediation
analysis then was used to examine the mediating, or "indirect," effects of
these key program elements in reducing total calls for service at the treat-
ment places as compared with the control places (MacKinnon and Dwyer,
1993). Negative binomial regression models, described above, were used
to model the causal pathways in this analysis.

In the first stage of the analysis, in separate regression models, the treat-
ment dummy variable was used to predict the differences in the various
police actions at control places relative to treatment places. In the second
stage of the analysis, the total calls for service outcome variable was
regressed on the treatment dummy variable and variables that accounted
for the three crime-prevention mechanisms to identify specific mediating
factors that determined any statistically significant changes in the depen-
dent variable. The difference between the treatment dummy variable in
the general main effects model and the model with differentiated crime-
prevention mechanisms represented the total mediated effect (MacKinnon
and Dwyer, 1993). The mediating effects of the three crime-prevention

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mechanisms then were compared by multiplying the coefficient that related each mediator to the treatment dummy variable by the coefficient that related the outcome to the same mediator and then calculating appropriate standard errors for significance testing of the specific mediating factors (see MacKinnon and Dwyer, 1993).

For the social and physical observation data, significance testing was performed using the nonparametric sign test. In experiments with a small number of cases, it is appropriate to use an application of the binomial distribution known as the sign test (Blalock, 1979). This test examines the probabilities of getting an observed proportion of successes from a population of equal proportions of successes and failures. Sign tests assume independence of trials; this requirement was met by the random allocation within the pairs. Physical and social observations were analyzed to determine whether positive or negative changes occurred at the treatment locations as compared with the control locations.

DISPLACEMENT AND DIFFUSION EFFECTS

Under traditional dispositional views of crime and criminality, policing strategies that are focused on specific types of events or treat specific locations are thought to result in the displacement of crime (Repetto, 1976). However, in recent years, a growing body of evidence suggests that displacement is never complete and often inconsequential (for a review, see Hesseling, 1994). Several scholars have suggested that crime-prevention efforts may result in the complete opposite of displacement—that anticipated crime-control benefits may be greater than expected and "spill over" into places beyond the target areas. Generally referred to as "diffusion of benefits," these unexpected benefits have been reported by several studies on crime-prevention measures (for a review, see Clarke and Weisburd, 1994). A recent controlled study of displacement and diffusion effects generated by intensive police interventions in two hot-spots areas in Jersey City, New Jersey, found that the most likely outcome of focused crime-prevention efforts was a diffusion of crime-control benefits to the surrounding areas (Weisburd et al., 2006).

Displacement can take many different forms (Gabor, 1990) and is a complex phenomenon to measure (see Barr and Pease, 1990). Most studies of crime-prevention efforts are designed to measure main effects, and the measurement of displacement is often neglected until it is time to defend claims of crime-control gains. Some researchers suggest that evaluations should be planned to study both main effects and possible displacement or diffusion effects (Weisburd and Green, 1995a; Weisburd et al., 2006). Although in this study the evaluation design was focused on the direct effects of treatment, this study also was designed specifically to measure immediate spatial displacement and diffusion effects. As mentioned,

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a two-block catchment area was constructed around each of the 34 places; pretest and posttest official crime data in the areas surrounding control and treatment locations were compared to assess the diffusion and displacement effects for crime types that were affected by the intervention at the treatment places.[11] Proximate spatial effects were measured using the same analytical techniques (that is, the randomized complete block design and Poisson and negative binomial regression models)[12] as the analysis of main effects.

IMPACT EVALUATION RESULTS

EMERGENCY CITIZEN CALLS FOR SERVICE DATA

Table 1 presents the parameter estimates of the effects of the policing disorder treatment on total and disaggregated categories of citizen calls for service. According to the incidence rate ratio, the total calls for service were reduced by a statistically significant 19.8 percent ($p = .009$) in the treatment places relative to the control places. It is important to note that these strong reductions in the total number of calls for service were not driven by a large reduction in only one crime type. All the crime-type categories at the treatment places were reduced to varying degrees as compared with control places. Robbery and nondomestic assault calls were reduced by a statistically significant 41.8 percent ($p = .033$) and a statistically significant 34.2 percent ($p = .000$), respectively, at the treatment places relative to the control places. Although burglary/breaking and entering calls were reduced by a statistically significant 35.5 percent ($p = .000$) at the treatment places relative to control places, larceny/theft calls only experienced a nonstatistically significant reduction in the treatments places relative to the control places.

Although the intervention comprised tactics to reduce social and physical disorder, the treatment generated a 14 percent reduction in disorder/

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11.   To set the widest possible net for examining potentially complex displacement and diffusion effects, all crime data were analyzed. However, it is important to recognize that if a crime category at the place did not experience crime-control benefits from the treatment, then it is not reasonable to attribute displacement or diffusion effects in the surrounding areas to the intervention.

12.   The tests confirmed a Poisson distribution by failing to reject the null hypothesis that no difference is found between the observed distribution and a Poisson distribution for robbery calls ($X^2 = 10.173$ with d.f. = 15; $p = .808$). The tests confirmed negative binomial distributions by rejecting the null hypothesis that no difference is found between the observed distribution and a Poisson distribution for assault calls ($X^2 = 52.416$ with d.f. = 15; $p = .000$), larceny/theft calls ($X^2 = 70.381$ with d.f. = 15; $p = .000$), burglary/breaking and entering calls ($X^2 = 58.160$ with d.f. = 15; $p = .000$), disorder/nuisance calls ($X^2 = 99.012$ with d.f. = 15; $p = .000$), and total calls ($X^2 = 188.440$ with d.f. = 15; $p = .000$).

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Table 1. Main Effects—Citizen Calls for Service

Dependent Variable	Incidence Rate Ratio Estimate for Treatment	Coefficient	Robust SE	Z	p Level	95% CI
Assault	.658	−.418	.102	−4.10	.000**	.538–.830
Robbery	.582	−.541	.254	−2.13	.033**	.353–.957
Burglary/Breaking and Entering	.645	−.438	.118	−3.69	.000**	.511–.814
Larceny/Theft	.893	−.113	.076	−1.48	.139	.769–1.037
Disorder/Nuisance	.860	−.169	.106	−1.75	.081*	.762–1.018
Total Calls	.802	−.220	.084	−2.60	.009**	.679–.947

ABBREVIATIONS: CI = confidence interval; SE = standard error.
*p < .10; **p < .05.

nuisance calls at the treatment places relative to control places at a less restrictive .10 significance level (p = .081). Regardless of any actual reductions in social and physical incivilities at the place, the disorder call categories were necessarily influenced by policing activity at the experimental places during the posttest period. After the intervention period ended and active problem solving ceased, the LPD officers attempted to maintain the crime-prevention gains at their places and encouraged community members to report disorderly activity. As such, disorderly behavior may have been more likely to be reported by the community during the posttest period because of an awareness that such complaints would be taken seriously by the police. Given the limitations of the official data in measuring the effects of the treatment on disorder, the physical and social observation data provide more reliable and valid performance measures as to whether the strategy actually controlled incivilities at the treatment places.

Figure 1 presents the mediation analysis of crime-prevention mechanisms in the Lowell policing disorder strategy.[13] The first stage of the analysis estimated the magnitude of key crime-prevention activities in the treatment places relative to the control places. During the intervention

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13.   The calculations presented in figure 1 followed the mediation analysis procedure outlined by MacKinnon and Dwyer (1993). To illustrate the process, the calculations for the mediated effects of situational prevention strategies on total calls for service are presented here. In the first stage of the analysis, the treatment dummy variable predicted the difference in the number of situational strategies at the treatment and control places. This model estimated a coefficient of α = 1.919 (IRR = 6.818) with a standard error of σ_α = .278. In the second stage of the

period, the policing disorder strategy increased the total number of misdemeanor arrests by 29.4 percent ($p = .000$) in the treatment places relative to the control places. Relative to the control places, the treatment places experienced 6.8 times the number of situational strategies ($p = .000$) and 2.4 times the number of social service strategies ($p = .000$) during the intervention period. In the second stage of the analysis, the variables that measured the three crime-prevention mechanisms were added to the main effects model to determine the indirect effect on total calls for service at the treatment places relative to the control places that was mediated by these key program elements. Controlling for the other covariates, the treatment variable was associated with a nonstatistically significant 1.7 percent increase in total calls for service at the treatment places relative to control places (*IRR* = 1.017; see figure 1). This finding suggests that the 19.8 percent reduction in total calls for service estimated by the main effects model (table 1) was generated entirely by the identified key program elements: misdemeanor arrests, situational prevention strategies, and social service strategies. The total mediated effect of the key program elements was an estimated 21.5 percent reduction in total calls for service at the treatment places relative to the control places [or the difference between the treatment coefficients in the two models: $-.198 - (+ .017)$].

After multiplying the coefficient that related each mediator to the treatment dummy variable by the coefficient that related the outcome to the same mediator and then calculating appropriate standard errors for significance testing, the analysis revealed that the number of situational prevention strategies generated the strongest crime-control gains. Controlling for the number of misdemeanor arrests, social service strategies, and the treatment dummy variable, the number of situational strategies was associated with statistically significant decreases ($p < .05$) in total calls for service in treatment places relative to controls. Using a less-restrictive significance level ($p < .10$), increased misdemeanor arrests also generated crime-prevention gains. Controlling for the number of situational strategies, social

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analysis, the total calls for service outcome variable was regressed on the treatment dummy variable and on variables that accounted for the three crime-prevention mechanisms to identify specific mediating factors that determined any statistically significant changes in the dependent variable. For the effects of situational strategies on total calls, this second model estimated a coefficient of $\beta = -.112$ (IRR = .894) with a standard error of $\sigma_\beta = .049$, controlling for the other covariates. The mediated effect then was determined by multiplying the coefficient that related situational strategies to the treatment dummy variable by the coefficient that related total calls to situational strategies ($1.919 \times -.112$). This calculation produced a coefficient of $\alpha\beta = -.215$ (IRR = .807). To calculate the appropriate standard errors for significance testing of the specific mediating factors, the following formula was used: $\sigma_{\alpha\beta} = \sqrt{\alpha^2\sigma_\beta^2 + \beta^2\sigma_\alpha^2}$. The hypothesis test ($\alpha\beta/\sigma_{\alpha\beta}$) yielded $Z = -2.148$ ($p < .05$).

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service strategies, and the treatment dummy variable, increasing the number of misdemeanor arrests was associated with notable decreases in total calls for service in the treatment places relative to the control places. Controlling for the other covariates, social service strategies did not generate statistically significant crime-prevention gains at the treatment places relative to the control places. Given the importance of situational strategies in reducing crime in the hot-spot areas, this analysis suggests that research studies that rely solely on misdemeanor arrests as a proxy measure of policing disorder strategies seem likely to specify program effects incorrectly.

Figure 1. Mediation Analysis of Crime-Prevention Mechanisms in Policing Disorder Treatment

NOTE: Coefficients expressed as incidence rate ratios.

*p < .10; **p < .05.

OBSERVATIONS OF SOCIAL AND PHYSICAL INCIVILITIES

Table 2 presents the aggregate pretest and posttest mean numbers of persons engaged in various disorderly behaviors across three observations in the treatment and control places. Table 3 presents the aggregate pretest and posttest counts of various types of urban blight for the treatment and control places. Although social and physical disorder problems were not completely eliminated at the treatment places, the summary tables reveal that a noteworthy reduction in disorder was found relative to the control places. As described, comparative assessments were made for social and physical observation data collected at places in each of the 17 pairs. The full pairwise comparison tables are available from the authors by request.

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Social disorder was alleviated at 14 of the 17 (82.4 percent) treatment places as compared with the control places. This result was statistically significant; the observed sign test proportion was .824, and the exact binomial two-tailed probability was .013. Physical disorder was alleviated at 13 of the 17 (76.5 percent) treatment places relative to the control places. This result was also statistically significant according to the sign test; observed proportion was .765, and the exact binomial two-tailed probability was .049. According to the systematic observation data, the implemented strategy significantly reduced social and physical disorder at the treatment places relative to the controls.

Table 2. Aggregate Pretest and Posttest Means from Three Social Observations of Disorderly Activity at Treatment Places Versus Disorderly Activity at Control Places

Social	Treatment Places			Control Places		
Incivilities	Pretest	Posttest	% Change	Pretest	Posttest	% Change
Loiterers	128	36.3	−71.6%	155.3	134.7	−12.9%
Public Drinkers	38.3	10.3	−73.1%	31.7	34	+ 7.3%
Drug Sellers	9.7	3.7	−61.9%	8.7	7.7	−11.5%
Homeless	6.3	3	−52.4%	5.7	6.3	+10.5%

NOTE: The pairwise comparison revealed that social disorder was alleviated at 14 of the 17 (82.4 percent) treatment places as compared with the control places (sign test proportion = .824; *p* = .013).

DISPLACEMENT AND DIFFUSION EFFECTS

Displacement and diffusion effects were assessed by comparing citizen calls for service and reported crime incidents in the two-block catchment areas immediately surrounding the control and treatment groups for the 6-month preintervention and postintervention periods. Table 4 presents the parameter estimates of the effects of the problem-oriented policing treatment on the selected categories of citizen calls for service in the areas immediately surrounding the treatment places relative to the areas immediately surrounding the control areas. The displacement and diffusion experimental analyses revealed that all call categories did not experience significant displacement or diffusion effects because of the problem-oriented strategy in the target areas. Robbery calls, assault calls, burglary/breaking and entering calls, larceny theft calls, disorder calls, and total calls were not significantly displaced into the areas immediately surrounding the treatment places relative to the areas immediately surrounding the

Table 3. Aggregate Pretest and Posttest Counts of Physical Incivilities at Treatment Places Relative to Control Places

Physical Incivilities	Treatment Places			Control Places		
	Pretest	Posttest	% Change	Pretest	Posttest	% Change
Street Segments with Trash	41	30	−26.8%	44	49	+11.4%
Structures with Graffiti	13	10	−23.1%	16	19	+18.8%
Damaged Structures	8	3	−62.5%	4	3	−25.0%
Unkempt Vacant Lots	7	2	−71.4%	3	3	0%
Abandoned Cars	4	0	−100%	1	0	−100%
Unsecured Abandoned Buildings	3	1	−66.7%	2	2	0%

NOTES: "Damaged structures" included fences, buildings, light posts, recreational equipment, and other physical structures at the place that needed to be repaired. "Unkempt" vacant lots were filled with trash and/or overgrown vegetation. The pairwise comparison revealed that physical disorder was alleviated at 13 of the 17 (76.5 percent) treatment places as compared with the control places (sign test proportion = .765; p = .049).

control places. However, it is noteworthy that all call categories experienced varying nonsignificant increases in the treatment catchment areas relative to the control catchment areas. Although this pattern may suggest some minor immediate spatial displacement of crime and disorder problems, these nonsignificant increases do not outweigh the statistically significant crime-control gains experienced in the treatment places. As raised in the Methods section, these findings must be interpreted with caution; immediate spatial displacement is a very complex phenomenon to measure, and as Weisburd and Green (1995a: 358) observe, "statistics that appear solid on paper may reflect the difficulties of analyzing this process as much as any real substantive findings."

CONCLUSION

The results of this randomized controlled experiment contribute to the growing body of evidence that the more focused and specific the strategies of the police are and the more tailored the strategies are to the problems

Table 4. Displacement and Diffusion Effects—Citizen Calls for Service

Dependent Variable	Incidence Rate Ratio Estimate for Treatment	Coefficient	Robust SE	Z	p Level	95% CI
Assault	1.143	.133	.100	1.33	.184	.938–1.392
Robbery	1.191	.174	.130	1.34	.180	.922–1.538
Burglary/Breaking and Entering	1.150	.139	.167	.83	.405	.827–1.598
Larceny/Theft	1.155	.144	.069	1.50	.134	.956–1.395
Disorder/Nuisance	1.041	.040	.088	.46	.648	.875–1.238
Total Calls	1.057	.055	.111	.50	.617	.850–1.314

ABBREVIATIONS: CI = confidence interval; SE = standard error.
*$p < .10$; **$p < .05$.

the police seek to address, the more effective the police will be in controlling crime and disorder (Braga, 2002; Skogan and Frydl, 2004; Weisburd and Eck, 2004). Although the magnitude of the effects of the Lowell policing disorder strategy was inconsistent across the crime call categories, all indicators experienced noteworthy reductions in the treatment places relative to the control places. Systematic observation data also revealed that indicators of social and physical disorder were significantly reduced at the treatment places relative to the control places. Moreover, this study bolsters the position that focused enforcement efforts do not necessarily cause crime problems to displace to surrounding areas (Barr and Pease, 1990; Gabor, 1990). As many observers suggest (Eck, 1993; Hessling, 1994), displacement is not an inevitable consequence of focused crime-prevention efforts.

These results do lend considerable credibility to Wilson and Kelling's (1982) perspective that policing disorder can generate crime-prevention gains. However, it is also important for police executives, policy makers, and academics to understand the nature of the Lowell Police Department's policing disorder strategy. This research offers at least three key operational elements to consider. First, the approach was focused at specific high-activity crime and disorder places in the city. Instead of a broad-based policing disorder strategy diffused across the city landscape, the intervention was concentrated in a few hot-spot locations that generated a disproportionate amount of crime (Sherman, Gartin, and Buerger, 1989;

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Weisburd, Maher, and Sherman, 1992). Given the growing scientific evidence on the crime-prevention effectiveness of hot spots policing (Braga, 2001, 2005; Skogan and Frydl, 2004), broadly implemented and unfocused order maintenance strategies do not seem well positioned to generate consistent crime-control gains. Second, in monthly meetings, the LPD officers were held accountable for reducing citizen calls for service and for ameliorating social and physical incivilities in targeted hot-spot areas. In the absence of relevant measurement systems, police executives experience difficulty motivating their managers and line-level officers to change their approach toward policing (Moore and Braga, 2003). To ensure that problem places receive the appropriate amount of police attention, performance measurement and accountability systems need to be in place. In New York City, Compstat meetings served as a mechanism to identify hot-spot locations in precincts and to hold commanders accountable for addressing these persistent problem locations (Silverman, 1999; Weisburd et al., 2003). If the NYPD deserves some credit for New York's crime drop, as the evidence suggests it does, then Compstat probably played an important role in driving the organization in its implementation of the broken windows policing strategy.

Third, and perhaps most important, the strongest crime-prevention benefits were driven by situational strategies that attempted to modify the criminal opportunity structure at crime and disorder hot-spot locations. Changes in the physical environment may discourage potential offenders from frequenting an area by altering criminal opportunities at a place (Clarke, 1997). The presence of abandoned buildings, for instance, attracts offenders to places (Spelman, 1993). The LPD officers' strategies to ameliorate physical incivilities and improve surveillance at places (thereby changing site features and facilities) may have diminished the number of easy criminal opportunities and, thus, discouraged offenders from frequenting the experimental places (Eck and Weisburd, 1995). According to Wilson and Kelling (1982), reductions in physical and social incivilities at places send clear signals to potential criminals that law-breaking will no longer be tolerated. Offenders make choices about the places they frequent based on cues at the site and are likely to select places that emit cues of where risks are low for committing crimes (Eck and Weisburd, 1995). Changing the perceptions of potential offenders by controlling disorder may reduce their numbers at the place. Increased misdemeanor arrests generated smaller crime-prevention gains. These tactics may have increased the certainty of detection and apprehension at the place, communicated that disorderly behavior would no longer be tolerated at the place, and raised the potential offender's perceptions of risk at the place. These perceptions of increased risks may have influenced the behavior of an array of would-be offenders. Although social service strategies did not

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generate any short-term crime-prevention gains, these actions might have beneficial effects in the long term and could generate desirable side benefits for the police such as improved relationships with community members.

These findings suggest that when adopting a policing disorder approach to crime prevention, police departments should work within a problem-oriented policing framework and adopt a community coproduction model rather than drift toward a zero tolerance policing model that focuses on a subset of social incivilities, such as drunken people, rowdy teens, and street vagrants, and seeks to remove them from the street via arrest (Taylor, 2001, 2006). Misdemeanor arrests obviously play a noteworthy role in dealing with disorder; however, arrest strategies do not deal directly with physical conditions. In devising and implementing situational strategies to deal with a full range of disorder problems, police must rely on citizens, city agencies, and others in numerous ways. As Ralph Taylor (2001, 2006) suggests, incivility reduction is rooted in a tradition of stable relationships with the community and responsiveness to local concerns. Community coproduction requires the police to build partnerships with other organizations and the community, which brings its own set of challenges (Crawford, 1997; Rosenbaum, 2002). Nonetheless, this research suggests that a sole commitment to increasing misdemeanor arrests is not the most powerful approach to community crime prevention and, according to many observers (e.g., Taylor, 2006), may undermine relationships in low-income, urban minority communities where coproduction is most needed and distrust between the police and citizens is most profound.

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