CRIMINOLOGY

DOES WHAT POLICE DO AT HOT SPOTS MATTER? THE PHILADELPHIA POLICING TACTICS EXPERIMENT*

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Policing tactics that are proactive, focused on small places or groups of people in small places, and tailor specific solutions to problems using careful analysis of local conditions seem to be effective at reducing violent crime. But which tactics are most effective when applied at hot spots remains unknown. This article documents the design and implementation of a randomized controlled field experiment to test three policing tactics applied to small, high-crime places: 1) foot patrol, 2) problem-oriented policing, and 3) offender-focused policing. A total of 81 experimental places were identified from the highest violent crime areas in Philadelphia (27 areas were judged amenable to each policing tactic). Within each group of 27 areas, 20 places were randomly assigned to receive treatment and 7 places acted as controls. Offender-focused sites experienced a 42 percent reduction in all violent crime and a 50 percent reduction in violent felonies compared with their control places. Problem-oriented policing and foot patrol did not significantly reduce violent crime or violent felonies. Potential explanations of these findings are discussed in the contexts of dosage, implementation, and hot spot stability over time.

This study adds to the evidence base by conducting a randomized, controlled field experiment of three approaches to hot spots policing (foot patrol [FP], problem-oriented policing [POP], and offender-focused [OF] policing) at 60 violent crime hot spots and 21 control hot spots. All three treatments were applied by the same police department, in

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the same city, over approximately the same time period, providing greater consistency than systematic reviews of studies implemented across different cities and types of crime problems.

Advances in data availability and information systems have enabled researchers and police practitioners to examine spatial crime patterns at progressively smaller spatial units (Eck and Weisburd, 1995; Sherman, 1997). Using these tools, researchers have demonstrated that crime is concentrated in particular microlevel places, such as street segments (Weisburd et al., 2004), intersections (Taniguchi, Ratcliffe, and Taylor, 2011), and addresses (Pierce, Spaar, and Briggs, 1986; Sherman, Gartin, and Buerger, 1989), and that crime levels often vary from street to street (Groff, Weisburd, and Yang, 2010; Weisburd, Groff, Yang, 2012). The concentrations of crime at particular places are termed "hot spots" (Eck et al., 2005).

This development has spurred an evolution in policing from random patrol (Weisburd and Eck, 2004) to focusing police resources on high-crime places (Sherman, 1997; Sherman and Weisburd, 1995). Focusing resources on small, high-crime places has become widely known as "hot spots policing" (Braga, 2001; Weisburd and Braga, 2003). As Braga (2008: 7) noted, "[t]he appeal of focusing limited resources on a small number of high-activity crime places is straightforward. If we can prevent crime at hot spot locations, then we might be able to reduce total crime." Cumulative evidence suggests that concentrating police resources in small areas can reduce crime (Braga, Papachristos, and Hureau, 2012; Lum, Koper, and Telep, 2011; Sherman, 1997); however, little is known about which policing tactics are best suited for policing small areas (Telep and Weisburd, 2011). Evaluations of multiple tactics using robust experimental designs are scarce (for exceptions, see Braga and Bond, 2008; Taylor, Koper, and Woods, 2011).

THEORETICAL AND EVIDENTIAL BASIS FOR HOT SPOTS POLICING

Several theories offer plausible explanations for why concentrating police efforts at small, high-crime places would reduce crime more effectively than unfocused patrol strategies. Deterrence theory holds that crime occurs when the perceived risk of committing a crime is lower than the perceived reward from it (Beccaria, 1963 [1764]; Bentham, 1948 [1789]). Police presence is assumed to influence the risk–reward calculus of would-be criminals by increasing the perception that being caught and punished is more likely, and thus, the costs of committing a crime now outweigh the potential benefits. In this way, police create an "unremitting watch" (Shearing, 1996: 74) through which arrest risk is increased. A careful watch by police is more likely to be noticed and perceived as a deterrent in smaller areas.

Opportunity theories (Brantingham and Brantingham, 1991 [1981], 1993; Cohen and Felson, 1979; Felson, 1987) emphasize that the characteristics of places structure the routine activities of individuals and produce criminogenic combinations of offenders, targets, and guardianship levels at certain places and times. Different policing tactics might reduce crime in different ways, for example, by increasing the level of guardianship at places through police presence, improving informal social control, or altering perception of the built environment to change offenders' risk–reward calculus.

Hot spots policing is a promising strategy (Braga, Papachristos, and Hureau, 2012; Lum, Koper, and Telep, 2011; Sherman, 1997). The National Research Council Committee to Review Research on Police Policy and Practices (2004: 250) concluded that, "studies that focused police resources on crime hot spots provide the strongest collective evidence of police effectiveness that is now available." With regard to specific tactics, directed patrol (Caeti, 1999; Crank et al., 2010; Sherman and Rogan, 1995; Sherman and Weisburd, 1995), fixed patrol presence (Lawton, Taylor, and Luongo, 2005), FP (Ratcliffe et al., 2011), order maintenance (Braga and Bond, 2008; Caeti, 1999; Weisburd and Green, 1995b), gun enforcement (Sherman and Rogan, 1995), and POP (Braga and Bond, 2008; Braga et al., 1999; Caeti, 1999; Hinkle and Weisburd, 2008; Mazzerolle, Price, and Roehl, 2000; Sherman, Buerger, and Gartin, 1989) have all been found to be effective at addressing different types of crime hot spots.[1]

Agencies report that different tactics are effective for different types of problems, and the policing tactic used often is dependent on the type of hot spot (also see Koper, 2014). Police practitioners report employing a wide variety of tactics in hot spots ($n = 18$) (Police Executive Research Forum, 2008). However, little research has compared the relative effectiveness of different policing tactics in hot spots.

We uncovered only two experimental studies evaluating multiple policing tactics implemented in hot spots. The earliest was a randomized controlled trial evaluating the implementation of different tactics within a POP framework in Lowell, Massachusetts, over a 1-year time period (Braga and Bond, 2008). Although the focus of the experiment was on "policing disorder" using POP, a mixture of activities was undertaken including situational crime prevention, social-service–oriented actions, and order maintenance enforcement. Overall reductions in treatment versus control places were found for robbery, nondomestic assault, and burglary incidents. Their mediation analysis revealed that situational activities produced the largest decreases in calls for service, followed by order maintenance enforcement focused on making misdemeanor arrests.

In Jacksonville, Florida, researchers randomly assigned 83 hot spots to either a directed patrol ($n = 21$), POP ($n = 22$), or control (routine patrol, $n = 40$) condition (Taylor, Koper, and Woods, 2011). The experiment ran for 90 days, and the hot spots received treatment 7 days a week. Contrary to prior research, directed patrol produced only nonsignificant reductions in crime or calls for service during and after the intervention. POP hot spots experienced a significant reduction in street crime.

Therefore, building on the theoretical and empirical foundations just reviewed, this study incorporates practitioners' perceptions of "what works" and evaluates three different policing tactics in hot spots using an experimental design: 1) FP, 2) POP, and 3) OF policing. The next sections describe these tactics in terms of their theoretical and empirical potential for crime reduction at small, high-crime areas.

FOOT PATROL

Deterrence is achieved by increasing the perceived certainty of apprehension (Beccaria, 1963 [1764]; Bentham, 1948 [1789]). FP, the original form of policing (Klockars, 1985), accomplishes this by providing a localized police presence and increasing the perceived

1. Several comprehensive reviews of studies at hot addresses as well as hot spot areas have been published (Sherman, 1997).

likelihood that a potential offender will be recognized. Given their limited mobility, FP officers are more likely to be around and to be familiar with people who use a small, concentrated area such as a crime hot spot.

Research into the effectiveness of FP during the 1970s–1990s found that it improved citizens' feelings of safety and citizen–police relations but did not reduce crime. The Newark Foot Patrol Experiment (Police Foundation, 1981) found no significant differences among treatment and control beats for reported crime or official arrests; subsequent studies also failed to find an effect for crime incidents (Bowers and Hirsch, 1987; Esbensen and Taylor, 1984) or calls for service (Cordner, 1991; Pate, 1989). In contrast, two recent FP studies found significant violent crime reductions of 23 percent in Philadelphia, Pennsylvania (Ratcliffe et al., 2011), though this reduction dissipated after FPs were removed (Sorg et al., 2013), and 30 percent in Newark, New Jersey (Piza and O'Hara, 2012), when FP was focused in micro-crime hot spots.

PROBLEM-ORIENTED POLICING

POP represents a shift from the standard model of policing (Weisburd and Eck, 2004) to a proactive approach that emphasizes identifying and solving problems (Goldstein, 1979, 1990). A problem is defined as "a recurring set of related harmful events in a community that members of the public expect the police to address" (Clarke and Eck, 2006: 40). POP emphasizes the use of non–law-enforcement solutions as well as traditional responses to crime problems based on the results of an in-depth analysis of the problem.

POP asks police officers to conduct applied science to address problems (Eck, 2006). This strategy can be challenging for police officers who have not traditionally been trained for or tasked with obtaining a deep understanding of problems. As a result, the level of POP implemented is sometimes described as "shallow" and tends to use law-enforcement responses (Braga and Bond, 2008). Even so, initiatives using POP often have achieved measurable crime reductions (see Weisburd et al., 2010), especially when focused at small places (Clarke and Eck, 2005; Weisburd and Eck, 2004; Weisburd et al., 2008). Most POP evaluations, however, suffer from weak designs (Weisburd and Eck, 2004; Weisburd et al., 2008). In studies where POP is applied at hot spots, it is critical that other types of focused policing take place at hot spots for comparison. Without such controls, it is difficult to disentangle whether the application of POP, the focus on small areas, or a combination of the two drives crime reduction (Weisburd and Eck, 2004; Weisburd et al., 2008; also see Eck, 2003, for discussion of when experimental designs are inappropriate for evaluating POP).

OFFENDER-FOCUSED POLICING

Focusing on offenders has deep roots in policing. Seminal criminological research demonstrating that a small percentage of offenders are responsible for most crime (Wolfgang, Figlio, and Sellin, 1972) has fostered the idea that focusing on the most prolific offenders can have a substantial impact on crime (Ratcliffe, 2008). After all, "[i]f a few individuals are responsible for most crime or disorder, then removing them should reduce crime" (Clarke and Eck, 2005: 18). OF is an important part of an intelligence-led policing framework (see Ratcliffe, 2008). Intelligence-led policing is a policing model that uses crime information and criminal intelligence within an operational framework to identify and focus police resources on problem areas and on prolific and serious criminals

(Ratcliffe, 2008). This approach stresses the importance of merging crime analysis with criminal intelligence (Ratcliffe, 2007) and the proactive targeting of repeat offenders. In a recent study, intelligence-led drug-dealing targets were significantly different from drug traffickers identified and arrested by other means, and these individuals tended to be the more prolific and persistent dealers in the neighborhood (Kirby, Quinn, and Keay, 2010). The potential importance of targeting specific offenders was recently highlighted in a report by the National Research Council Committee to Review Research on Police Policy and Practices (2004). Sherman (1997: 229) noted:

> Like directed patrol, proactive (police-initiated) arrests concentrate police resources on a narrow set of high-risk targets. The hypothesis is that a high certainty of arrest for a narrowly defined set of offenses or offenders will accomplish more than low arrest certainty for a broad range of targets.

As with FP, OF primarily relies on deterrence theory. Increasing the certainty of arrest for a small group of identified offenders could discourage both the targeted individuals and others who witness or hear about the arrests. Of course, incarcerating prolific offenders is likely to have some incapacitative effects on crime rates as well (Blumstein, Cohen, and Nagin, 1978). Even if the targeted offenders are not arrested, the extra police attention received by both targets and their associates may make it too risky to continue their criminal activity. The extra attention also may make it riskier for nontargeted offenders to operate in a particular area.

Although this theory is promising, we could not find any studies evaluating OF tactics deployed in microlevel hot spots.[2] Two citywide evaluations (Washington, D.C., and Phoenix, Arizona) found that offenders targeted by the repeat offender units were more likely to be arrested and to receive longer custodial sentences than offenders targeted with standard police practices; however, the evaluations did not measure the units' impact on overall crime rates (Abrahmse et al., 1991; Martin and Sherman, 1986), nor did they specifically target offenders responsible for crimes at specific hot spots, which as explained next, was the focus of the OF intervention here.

METHODOLOGY

This study was designed and conducted as part of a continuing researcher–practitioner partnership with the Philadelphia Police Department. Philadelphia is located in the northeastern part of the United States. At the time of the study, the Philadelphia Police Department was the fifth largest in the United States with approximately 6,500 officers and covered an area of roughly 143 square miles. Despite recent steady reductions, Philadelphia still has higher violent crime rates than most other large American cities (Zimring, 2011). The Police Commissioner and management team were actively involved in the planning of the experiment so that the experimental design would approximate how hot spots policing would occur naturally in a large urban police department.

2. One study, using several policing tactics including offender focus, was conducted at microlevel hot spots in Jersey City, New Jersey, and found a crime reduction (Weisburd et al., 2006). But they did not develop a specific list of offenders as was the case in this study. Rather, they targeted prostitutes and those involved in the drug trade.

IDENTIFYING HOT SPOT AREAS FOR RANDOM ASSIGNMENT

Identification and selection of hot spot areas followed a multistep process. First, violent crime hotspots were mapped using 2009 incident data[3] and spatial statistics (see appendix A in the online supplementary information[4]). Second, 81 potential deployment areas were delineated from the map of hot spots. To increase buy-in for the study and to reflect as closely as possible the actual work flow of a hot spots policing initiative, this step was done by District Captains (local police commanders). The commanders used their operational knowledge to delineate the deployment area boundaries (Braga and Bond, 2008; Braga et al., 1999). The command team was asked to identify 27 areas suitable for each of the three treatment modalities ($N = 81$).

In subsequent meetings with the police department's Regional Operations Commanders (executive-level commanders, two ranks above the District Captains), the deployment area boundaries were revised to balance police operations with research priorities (i.e., achieving geographic separation of the target areas to examine spatial displacement and diffusion effects). The final 81 hot spots contained an average of 3 miles of streets, .044 square miles, and 23.5 intersections, making them larger than the size of hot spots in recent experiments (see table 1). For ease of comparison, the National Football League's regulation American football field is roughly .002 square miles (.005 square kilometers) or 360 feet long and 160 feet wide (110 and 49 meters, respectively) (Goodell, 2012). The average experimental area was roughly the size of 22 American football fields. The average size of the hot spots in two recent experiments by Braga and Bond (2008) and Taylor, Koper, and Woods (2011) were .012 square miles (6 football fields) and .02 square miles (10 football fields), respectively.

EXPERIMENTAL DESIGN AND RANDOM ASSIGNMENT

The experiment used a stratified randomized design with an unequal randomization ratio of 3:1. This design accommodated Philadelphia Police Department's (PD's) desire to address 60 hot spot areas (20 per intervention type) concurrently to have the largest possible impact on violent crime. This priority precluded the use of equal numbers of control and treatment sites for each tactic (10 treatment and 10 control). A seemingly straightforward solution would be to increase the number of sites to 40 per tactic (20 treatment and 20 control), but this approach had a significant drawback. Specifically, despite comparatively high violent crime levels, there were a limited number of places with sufficient levels of crime to be reasonably included in the study. A previous study in Philadelphia found crime reductions only in the top 40 percent most violent hot spot areas. By limiting control hot spots to 7 per tactic, we accommodated Philadelphia PD's desire to address 20 hot spots per intervention type while reducing the chances that a lower crime area would

3. Violent crime incident point data were extracted from the Philadelphia Police Department's 2009 incident database using Uniform Crime Report classification codes. Violent crime was defined as homicide, robbery, aggravated assault, and misdemeanor assault (see Table S.1 in the online supporting information). The Philadelphia Police Department's automated system achieves geocoding hit rates in excess of 98 percent, so the data are suitable for spatial data analysis (Ratcliffe, 2004).

4. Additional supporting information can be found in the listing for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2015.53.issue-1/issuetoc.

Table 1. Descriptive Statistics for Analysis Variables Across All Areas Across All Observations

Model Variables	Minimum	Maximum	Median	Mean	SD
Outcome Variables[a]					
All violent crime	.000	15.000	1.000	1.785	1.833
Violent felonies	.000	12.000	1.000	1.069	1.364
Exposure Variable[b]					
Geographic area (square miles)	.004	.239	0.039	.044	.033
Treatment Effect Contrast Codes [a,c]					
Foot patrol	−.528	.472	−.028	.000	.196
Problem-oriented policing	−.521	.479	−.021	.000	.226
Offender-focused policing	−.564	.436	−.064	.000	.262
Control Variables					
Problem-oriented policing[b]	.000	1.000	.000	.333	.472
Offender-focused policing[b]	.000	1.000	.000	.333	.472
Foot patrol posttreatment[a]	.000	1.000	1.000	.542	.498

ABBREVIATION: SD = standard deviation.
[a]Univariate statistics computed across 1,539 level 1 observations.
[b]Univariate statistics computed across 81 level 2 units.
[c]The treatment effect contrast code univariate statistics were computed after the effects were mean centered.
Source: Philadelphia Police Department Incident Database.

be selected for treatment. Thus, the experiment accurately reflected the practical focus of police on the highest violent crime areas.

Police commanders classified the 81 hot spots into three groups based on their qualitative assessments of the hot spots' amenability to the three different tactics. We expected that hot spots appropriate for each tactic might be more similar to one another than to places selected for the other tactics, and thus, these groups were used as qualitative strata in the experimental design (Shadish, Cook, and Campbell, 2002: 304–5). Randomization was performed separately for each stratum, resulting in 20 areas being assigned to treatment and 7 areas being assigned to control (figure 1). The District Captains responsible for implementing the treatments did not participate in the randomization process, and the control area locations were withheld from them.

ASSESSING PRETREATMENT DIFFERENCES

The analysis that will be described in later sections incorporates the randomization process. Treatment effects are estimated by contrasting outcome measures between only the treatment and control hot spots within each treatment modality. Randomization fidelity was assessed using univariate statistics (see table 2) and Mann–Whitney *U* tests to compare the treatment and control areas within the three randomization strata on five measures: 1) geographic area in square miles, 2) street length in miles, 3) number of intersections, 4) 90-day pretreatment all violent crime counts, and 5) 90-day pretreatment violent felony counts. The Mann–Whitney *U* test is a nonparametric test of the similarity of the distribution of a variable between two groups (Field, Miles, and Field, 2012). The Mann–Whitney *U* tests shown in table S.2 of the online supporting information confirmed that the randomization process was successful in creating treatment and control areas that

Figure 1. Map of Philadelphia and Experimental Areas by Type

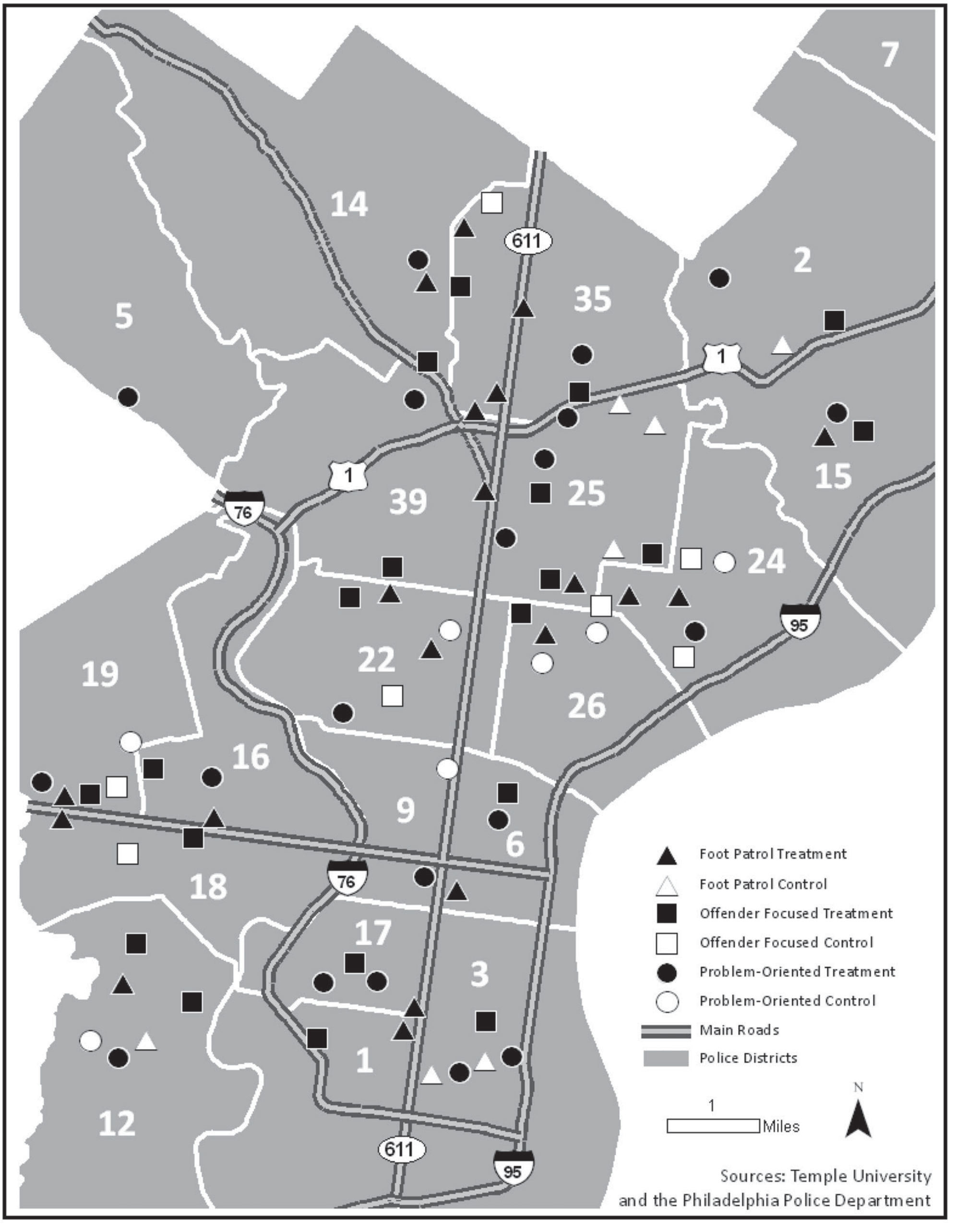


Table 2. Univariate Statistics for Experimental and Control Area Comparisons Prior to Implementation

		Foot Patrol Areas		Offender-Focused Areas		Problem-Oriented Areas	
Variable	**Statistic**	**Treatment***	**Control†**	**Treatment***	**Control†**	**Treatment***	**Control†**
Geographic area (square miles)	Median	.04	.03	.04	.03	.02	.04
	Mean	.05	.04	.06	.03	.03	.04
Street network length (linear miles)	Median	3.38	2.85	2.92	2.78	2.34	2.90
	Mean	3.21	2.84	3.63	2.37	2.52	2.95
Intersection count	Median	24.50	24.00	20.50	23.00	16.00	24.00
	Mean	25.30	22.43	28.25	20.00	18.70	23.00
Pre-90 days all violent	Median	11.50	13.00	9.50	13.00	10.00	13.00
	Mean	14.35	11.00	13.40	14.00	10.35	10.86
Pre-90 days violent felonies	Median	7.00	7.00	6.00	7.00	6.00	6.00
	Mean	8.50	7.14	8.20	7.86	5.90	5.43

NOTES: Because the implementation dates varied across treatment areas, the pre-90 day outcome/output variable counts for the treatment areas are based on the 90 days immediately preceding implementation. The pre-90 day outcome/output variable counts for control areas are based on the 90-days immediately preceding the implementation of first treatment area. Therefore, the pre-90 day outcome/output variable counts are measured with differential precision.

*Twenty treatment areas per treatment modality.

†Seven control areas per treatment modality.

Source: Philadelphia Police Department Incident Database.

were statistically equivalent on the three geographic composition and two violent crime measures.

EXPERIMENTAL CONDITIONS

To provide the best representation of how hot spots policing tactics would be implemented under normal conditions within a large urban police department, researcher involvement was minimal during the implementation. The investigation of three different tactics made it difficult to report a single, consistent metric for measuring treatment fidelity, so we drew from multiple sources: 1) interviews with officers and their peer mentors as well as the project director, 2) observations, and 3) official police data. Because the activities undertaken within a hot spot policing tactic are not always mutually exclusive, each type of deployment was evaluated according to the primary policing tactic undertaken.

Control

Control deployment areas received “business-as-usual” policing consisting of random patrol between calls for service. Although there was no prohibition against assigning officers to patrol on foot, to look for creative solutions to solve problems, or to focus on particular problem offenders, those tactics are infrequently employed as part of normal policing by the Philadelphia PD because the resources (e.g., personnel) are rarely available. Even if these tactics were implemented in the control areas, the implementation would not have reached the levels applied in the treatment areas. Given the scarcity of police resources and the length of time between selecting the hot spots and implementing the experiment, it is highly unlikely that commanders had additional resources to allocate to police control areas even if they remembered their locations.

Foot Patrol

FP areas received treatment for a minimum of 8 hours per day, 5 days per week, for 12 weeks. Beyond this basic minimum, District Captains were given discretion to determine how many officers would patrol, which days and times officers would patrol, and other operational decisions. Because specific individuals were assigned to each FP deployment area, we checked daily logs, reviewed incident databases, and interviewed at least one FP officer from each area to ensure compliance. In all but one target area, officers patrolled in pairs and worked one shift, 5 days per week. District Captains varied the timing of the FPs based on crime problems and union-contracted shift times.[5] Officers volunteered for FP in 9 of the 20 areas. The rest were chosen by their immediate supervisors. Whereas only rookie officers were assigned to FP during the Philadelphia Foot Patrol Experiment (Ratcliffe et al., 2011), in this experiment officers with varying years of service were assigned to FP. The interviews revealed that approximately one third of the FPs coordinated with other agencies to solve problems and some mentioned focusing on problem people ($n = 4$).

5. The specific start and end times varied by target area and reflected the crime patterns. Generally, eight sites worked during the day shift (between 7 A.M. and 6 P.M.), nine worked the evening shift (between 4 P.M. and 2 A.M.), and three alternated between day and night shifts weekly.

Problem-Oriented Policing

The tenets of POP introduced by Goldstein (1979, 1990) and a modified problem-solving process (Clarke and Eck, 2005) framed the work conducted in the POP areas. POP was conducted by teams of district officers in collaboration with community members and supported by personnel from police headquarters. Because POP was still relatively new at Philadelphia PD, all POP team members attended 1 day of training on the theoretical foundation of POP, the problem-solving process, and examples of POP in practice. Each step in the SARA process was addressed: scanning (gathering data to identify and understand the problem), analysis (analyzing the data that were gathered), response (developing tactics to address the problem), and assessment (evaluating what, if any, effect the tactics have had on the problem). Class instructors were police department officers and civilians who subsequently mentored the POP teams. The POP teams and mentors met periodically during the implementation to discuss the team's efforts and modify their responses as needed. Consistent with the implementation in most departments, officers assigned to the POP teams were drawn from patrol and remained responsible for answering radio calls. POP was conducted between other duties.

Treatment fidelity was evaluated through 1) a review of the action plans submitted, 2) interviews of the headquarters personnel and the program director, and 3) field visits to the sites. Each District Captain was required to submit and continuously update an action plan documenting the progress in each area.[6] Specific actions taken at each site varied with the problem identified. POP included partnerships with other agencies as well as law-enforcement actions, such as looking for known offenders (four sites) and FP (three sites). Fieldwork and fidelity surveys showed that even with the additional support of a mentor from headquarters, POP teams undertook relatively "shallow" analyses. This finding is consistent with the experience in many agencies (Bullock, Erol, and Tilley, 2006). As a result of analysis, at least eight POP areas switched to a focus on nonviolent crime and quality-of-life issues. At least four sites focused on narcotics in addition to a specific violent or nonviolent crime problem. Another site was located in the territory of the "Kensington Strangler," a serial rapist and killer who was active during the study period (Rawlins, 2011). In the end, just over half of the POP sites remained consistently targeted to violent crime problems.

Offender-Focused Policing

The OF tactic was based on the tenets of intelligence-led policing (Ratcliffe, 2008). OF consisted of identifying repeat violent offenders who either lived in or were suspected of being involved in violent crimes in the target areas and focusing extra attention on them. Offenders qualified for the initiative if they had a history of violent offenses and criminal intelligence suggested they were involved in a criminal lifestyle. The novel element of the tactic was the partnerships between the district OF teams and centralized intelligence analysts. Dedicated teams of officers drawn from the districts' tactical operations squads and an intelligence analyst identified and maintained a list of individuals thought to be causing

6. The action plans were a reporting form that was already being used within the Philadelphia PD to guide problem solving. The form is structured to follow the SARA process and requires a detailed description of the problem, documentation of responses (including naming those responsible for carrying out tasks), and periodic evaluations of outcomes.

the problems in the hot spot deployment areas. The OF teams then provided the identified offenders with extra attention. Officers assigned to the OF teams from the tactical operations squads have an organizational reputation for being proactive, and they were exempt from answering radio calls. This specific combination of elements was unique to the OF tactic's implementation.

The OF component was introduced during a meeting with Philadelphia PD executive command staff, district commanders and officers assigned to implementing the tactic, the police department's Central Intelligence Unit, and the research team. In OF areas, treatment fidelity was measured via reports filed by officers working those areas. The reports provided lists of the individuals they were targeting and the number of times the targeted individuals were questioned. According to self-report data from the OF team members and patrol officers, officers made frequent contact with these prolific offenders ranging from making small talk with a known offender to serving arrest warrants for a recently committed offense. The most frequent tactic used was surveillance followed by aggressive patrol and the formation of partnerships with beat officers. OF team members in some districts used flat-screen televisions in their roll-call rooms to display photos and convey other intelligence gathered on these prolific offenders to all district personnel.

STUDY PERIOD

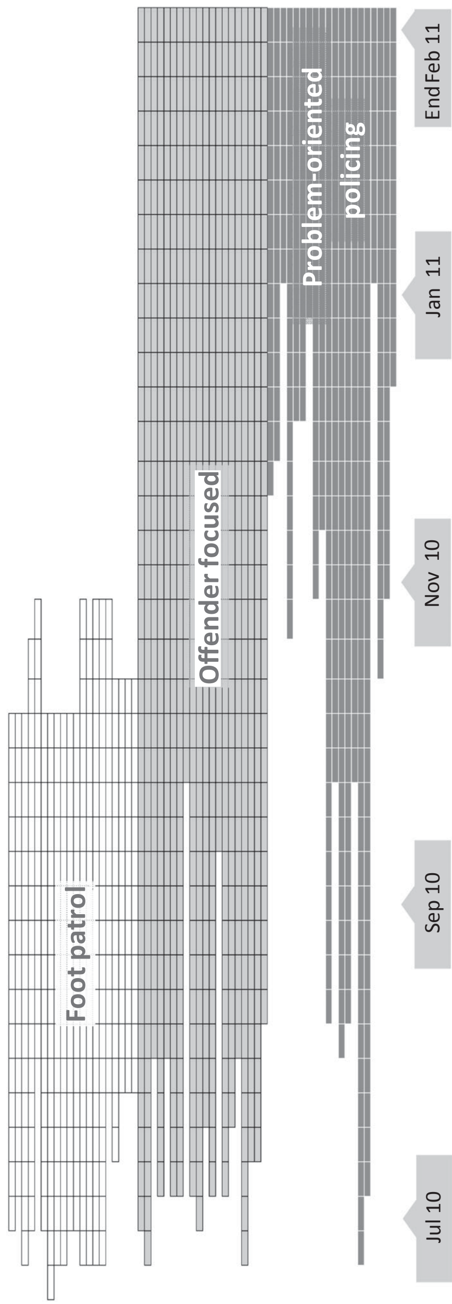
The experiment was designed to have the interventions start simultaneously at the beginning of June and conclude in August to address the spike in crime that occurs during the summer months. Actual implementation varied. Figure 2 shows the implementation schedule. FP was the easiest to implement. Most FP sites were operational by mid-June. OF and POP officers received training on schedule; however, both tactics were more difficult to implement because they required more intra-agency coordination and training after the experiment began. The OF treatment was delivered slightly later in the summer with all sites operational by October 2010. Considerable within-group variation in the OF areas' start dates was found. POP sites took the greatest amount of mentoring to get underway. It took until November 2010 for all POP sites to be operational. The original timeline for both OF and POP areas was extended to ensure at least 12 weeks of implementation. Overall, each tactic ran for a minimum of 12 weeks and a maximum of 24 weeks at any single target area.

ANALYTIC PLAN

REPEATED MEASURES MULTILEVEL MODELING

Random assignment in experimental designs can control for extraneous variables (Shadish et al., 2002). The varying treatment periods in the Philadelphia Policing Tactics Experiment (see figure 2) possibly introduced extraneous temporal effects into the study after random assignment occurred. A repeated-measures multilevel analysis was used to control statistically for those potential extraneous temporal effects. Repeated-measures multilevel models are appropriate for experimental study designs (Bryk and Raudenbush, 1987; Raudenbush and Bryk, 2002). Such models describe changes in an outcome measured at multiple time points for a given unit and account for the inherent dependence among repeated observations. The level 1 model is specified using time-varying covariates to describe within-unit changes. Level 1 parameters and the intercept can then be

Figure 2. Implementation Periods by Type of Policing Tactic and Week



specified as random effects and predicted using level 2 variables. The level 2 model describes differences across units (Raudenbush and Bryk, 2002). We nest biweekly outcome observations within treatment and control areas.

OUTCOME MEASURES

We examine two separate violent crime outcome measures: 1) all violent crime and 2) violent street felonies. The all violent crime outcome includes homicides, robberies, aggravated assaults, and simple (nonfelony) assaults. The violent street felonies outcome excludes simple assaults. The outcomes are biweekly counts for each experimental and control area. The analysis period began June 7, 2010, and it ended February 27, 2011, for a total of 19 biweekly observation periods ($t_0 \ldots t_{18}$). The outcome measures came from the Philadelphia PD's crime incident database. This official crime event database is susceptible to the limitations of official police data (Wolfgang, 1963). Univariate statistics are shown in table 1.

TREATMENT EFFECTS

A contrast coding scheme is used to capture the treatment effects of the three hot spots policing tactics. Contrast coding is used to test specific comparisons across multiple groups (McClendon, 1994). Contrast coding allows us to deal with two important components of the experiment's design: 1) the fact that randomization occurred within each of the three qualitative strata demarcated by the police commanders who designed the deployment areas and 2) the varying implementation periods within the treatment modalities. Specifically, we designed a treatment effect contrast coding scheme that only contrasts treatment and control areas within each tactic's randomization pool during treatment time periods, but it ignores differences between the treatment and control areas for a respective tactic during nontreatment time periods and never contrasts the treatment and control areas across randomization pools during any time periods.

For the FP contrast, all FP areas were coded "+.5" during the biweekly time periods in which FP was implemented. The seven FP control areas were coded "–.5" for all biweekly observations from the implementation of the first FP treatment to the termination of the last. During nontreatment observations, both the FP treatment and control areas are coded "0." POP and OF treatment and control areas were coded "0" on the FP treatment effect for all time periods. The same coding scheme with the respective combinations was used for the POP and OF treatment effects. Next, to ensure that the contrast codes were orthogonal (McClendon, 1994), each of the three treatment contrast codes was centered on its mean, which effectively reduced the correlations among the three contrast codes to approximately zero.[7] In sum, the three treatment effect contrast codes provide a comparison between treatment and control areas only within each of the three randomization

7. Prior to centering, control areas were coded "–.5" and treatment areas were coded "+.5" when treatment was turned on and "0" when the last treatment was turned off for the respective treatment contrast codes. All other areas for the other two treatment modalities were coded "0" across all observations on the treatment contrast codes. The new codes for the FP, POP, and OF treatments changed from "–.5," "0," and "+.5" to "–.528," "–.000," and ".472" versus "–.564," ".000," and ".436" versus "–.521," ".000," and ".479", respectively.

strata and only during the biweekly observations when the treatments were implemented (i.e., 20 treatment vs. 7 control areas).

CONTROL VARIABLES

Three sets of control variables were included in the models: 1) level 1 time indicators, 2) a level 1 FP posttreatment dummy, and 3) level 2 randomization strata dummies. Because of the varying implementation schedule, it is possible that extraneous temporal effects, such as seasonality, impacted the biweekly outcome counts during the experiment. To control for possible extraneous temporal effects, time indicators were entered into the model at level 1 (Sorg and Taylor, 2011). The 18 time indicators ($t_1 \ldots t_{18}$) were coded "1" if an outcome observation was from the time period a particular dummy variable represented and "0" otherwise. The first biweekly observation (t_0) served as the referent (Twisk, 2003). All FP implementations terminated between biweekly observation t_8 and t_{10}, whereas the other two treatments were still ongoing, so the FP posttreatment period *just* for FP locations was isolated using a dummy variable. As specific FP treatment locations had treatment "turned off," the experimental areas were coded "1" on the FP post dummy. By t_{10}, all FP implementations had terminated, so all FP treatment and control areas were coded "1" from t_{10} until t_{18}. This process ensured the outcome variance for the FP areas during their posttreatment periods was isolated. The three randomization strata were controlled for at level 2 using two dummy variables. All FP treatment and control areas were coded "1" on the FP stratum dummy, and all other areas for the other two treatments were coded "0." The same logic was used for the OF stratum control. The POP treatment and control areas served as the referent. This level 2 control ensured outcome variance across the three treatment strata was isolated from treatment effects.

MODEL SPECIFICATION

Mixed-effects negative binomial models (menbreg in Stata 13; StataCorp, College Station, TX) generated model estimates. The exposure variable was geographic area in square miles to control for differences in the hot spot sizes. Incidence rate ratios (IRRs) are exponentiated β coefficients and reflect, after multiplying the difference between the IRR and 1 by 100, the expected percentage change in the predicted count of the outcome variable (Rabe-Hesketh and Skrondal, 2012). A Bonferroni correction was applied to reduce the chance of making a type 1 error for multiple outcomes. Because we analyzed two outcome measures, the traditional p value of .05 was divided by 2 and statistical significance for the treatment effects was interpreted at $p < .025$ (Tamhane, 2009).[8] Overall, this model specification was developed to provide the most rigorous estimation of the three treatment effects. Additional details about the models' sensitivity to other specifications and development in response to the peer review process can be found in appendix B of the online supporting information.

8. It was not possible to separate crime incidents handled or actions performed by officers participating in the experiment from those administering business-as-usual policing, so all crime incidents and actions taken by police in the hot spot areas are included in the outcome measures.

POST HOC STATISTICAL POWER ANALYSIS

Although views vary from field to field, in many social sciences, statistical power levels ($1 - \beta$) of at least 80 percent, corresponding to a type 2 error rate of 20 percent, are considered acceptable, as is the 4/1 ratio between type 2 and type 1 errors (Cohen, 1988, 1992). Power estimation complexities arise in multilevel designs. In other words, the two sample sizes are as follows: level 1, every 2 weeks within hot spots, and level 2, hot spots across all treatment categories (Snijders and Bosker, 2012: 178). Furthermore, the statistical power associated with a particular design varies for different parameters in a multilevel model (Snijders and Bosker, 2012: 82).

A post hoc multilevel power analysis was conducted for a model that closely matched what was described previously (Hox, 2010: 239–40). The software package MLPowSim (Browne, Lahi, and Parker, 2009) generated macros, which were then analyzed via MLwiN (Rasbash et al., 2009). The researcher can specify the number of simulations over which power is tested. This software can accommodate a count outcome and binary predictors. Gelman and Hill (2007) described a similar simulation method using *R*.

Two changes were required to the model described because of the MLPowSim software limitations. First, it was not possible to include the vector of dummies for the biweekly data because no more than 20 predictors total, including the constant, could be entered in a model. Second, the software did not allow for a response variable with a negative binomial distribution. These limitations necessitated that a mixed-effects Poisson model was run (mepoisson in Stata 13) with only seven predictors (two stratification identifiers, three treatment codes, FP posttreatment indicator, and exposure variable in logged form), plus the constant, so that coefficients and other resulting model parameters (e.g., level 2 residual variance) as well as descriptive information about the predictors can be used as input for the simulation estimates of statistical power. All resulting model parameters were used as input to the simulation, with one exception. For each of the three treatment variables, a coefficient of –.35, equivalent to approximately a 30 percent reduction in expected counts (IRR = .704), was substituted for the obtained model results. These specific coefficients represent the alternative hypothesis for each type of treatment effect.

Four hundred simulations were run for each possible combination of observation periods, ranging from 10 to 30 (21 biweekly periods were included in the current study), in steps of ten, and observation sites, ranging from 30 to 90 (there were 81 in the current study), again in steps of ten. Alpha was set at .0125 because each treatment was expected to reduce violent crime counts and there are two outcomes.

The power estimation results were as follows for 20 biweekly periods and 80 study areas. The reported numbers are the lower bound (LCL) of the zero/one or standard error estimate method, whichever was lower. For the all violent crimes outcome: FP = .75, OF = .23, and POP = .5. For the violent felonies outcome: FP = .73, OF = .419, and POP = .58.

These results suggest that power to observe such an effect—a 30 percent expected reduction in crime counts—with an alpha of .0125 was close to adequate for the FP treatment and was less than adequate for the other two treatment types with the design employed and with the size treatment effect described previously. Therefore, *should* either of the other two non-FP treatments emerge as significant, that result would attest to the size of the effects observed with either one.

Furthermore, should FP *not* emerge as significant in the results, this is *not* a result of poorer statistical power associated with this treatment compared with the other two treatment types.

RESULTS

ALL VIOLENT CRIME

The results for the repeated measures multilevel models are shown in table 3. Offender-focused policing was the only treatment effect to reach statistical significance. The OF tactic reduced expected all violent crime counts by roughly 42 percent relative to the OF control areas (IRR = .577; 95 percent confidence interval [CI] = .410, .813) even after controlling for the temporal effects, FP posttreatment observation period, and randomization strata membership. The FP (IRR = .951; 95 percent CI = .709, 1.276) and POP (IRR = 1.155; 95 percent CI = .819, 1.631) treatments did not reach statistical significance.

VIOLENT FELONIES

A significant treatment effect is again found for the OF tactic. The expected violent felony counts in the OF areas were approximately 50 percent lower than the expected violent felony counts observed in the seven OF controls areas during implementation biweekly periods (IRR = .503; 95 percent CI = 331, .766) after holding the temporal, FP posttreatment observation period, and randomization strata membership effects constant. Again, FP (IRR = 1.147; 95 percent CI = .784, 1.676) and POP (IRR = 1.200; 95 percent CI = .779, 1.849) treatment effects did not reach statistical significance.

DISPLACEMENT

Although crime displacement can occur in many forms (Eck, 1993; Reppetto, 1976), immediate spatial displacement is one of the most frequent criticisms of hot spots policing initiatives (Rosenbaum, 2006). Recent empirical evidence suggests immediate spatial displacement is less of a concern and a diffusion of crime control benefits is more likely (Guerette and Bowers, 2009; Weisburd et al., 2006). We evaluated immediate spatial displacement using Bowers and Johnson's (2003) weighted displacement quotient (WDQ) measure.

The WDQ equation requires crime counts from three separate areas, treatment locations (denoted "A"), buffer displacement locations (denoted "B"), and control areas (denoted "C"), both before (denoted "t_0") and during (denoted "t_1") the crime prevention initiative. It follows that if geographic displacement occurred, then crime in (A) would have decreased and crime in (B) would have increased relative to (C) during the treatment period. Conversely, if the initiative's crime-control benefits diffused into the immediately adjacent areas, then crime in both (A) and (B) would have decreased relative to (C) during the treatment period. The WDQ is expressed mathematically as follows:

$$\text{WDQ} = \frac{\text{B}t_1/\text{C}t_1 - \text{B}t_0/\text{C}t_0}{\text{A}t_1/\text{C}t_1 - \text{A}t_0/\text{C}t_0} = \frac{\text{Buffer Displacement Measure}}{\text{Treatment Success Measure}} \tag{1}$$

Table 3. Repeated Measures Multilevel Model Results for Outcome Variables

	All Violent Crime				Violent Felonies			
Variable	**β**	**SE**	**IRR**	**95% CI**	**β**	**SE**	**IRR**	**95% CI**
Intercept	3.634*	.137	37.892*	28.952 – 49.593	3.001*	.173	20.114*	14.329 – 28.234
Problem oriented	.034	.147	1.035	.776 – 1.380	.014	.171	1.014	.725 – 1.418
Offender focused	−.014	.148	.986	.738 – 1.318	−.025	.173	.975	.695 – 1.367
Foot patrol	−.051	.150	.951	.709 – 1.276	.137	.194	1.147	.784 – 1.676
Offender focused	−.550*	.175	.577*	.410 – .813	−.686*	.214	.503*	.331 – .766
Problem oriented	.144	.176	1.155	.819 – 1.631	.182	.221	1.200	.779 – 1.849
Foot patrol posttreatment	−.031	.094	.969	.806 – 1.167	−.046	.123	.955	.750 – 1.217
Global model parameters	**Estimate**	**SE**		**95% CI**	**Estimate**	**SE**		**95% CI**
lnalpha	−2.442	.288		−3.006 – −2.878	−1.739	.251		−2.230 – −1.247
Level 2 variance component	.217	.042		.149 – .316	.265	.054		.177 – .396

NOTES: Outcomes are biweekly counts with models specified as negative binomial distribution and an exposure variable of area (square miles). All models include 1,539 level 1 observations and 81 level 2 units. All models include 18 time indicator variables ($t_1 \ldots t_{18}$) with the first time period in the time series (t_0) serving as the referent at level 1.

ABBREVIATIONS: CI = confidence interval; d.f. = degrees of freedom; IRR = incidence rate ratio; SE = standard error.

*$p < .025$, Bonferroni-corrected p value based on two outcomes.

†*Source*: Philadelphia Police Department Incident Database.

This quotient can be interpreted in terms of the success of the intervention as well as of whether displacement or a diffusion of benefits occurred (Bowers and Johnson, 2003: 286).

Because of the theoretical process underpinning crime displacement, a WDQ can only be computed when an initiative produces a measureable crime reduction and prevention benefit (Bowers and Johnson, 2003); therefore, we only examine displacement or diffusion of benefits for the OF treatment areas on the all violent crime and violent felonies outcomes. We use the same 20 OF areas (A) and the 7 control areas (C) from the repeated-measures multilevel models, and we use 20 buffer areas (B) developed specifically for the WDQ analysis. The 20 buffer areas were developed by three researchers performing systematic social observations of the 81 experimental areas during the summer of 2010 (the treatment period). Given the precedent established in the hot spots policing evaluation literature (Braga and Bond, 2008; Braga et al., 1999; Weisburd and Green, 1995a, 1995b; Weisburd et al., 2006), the field researchers were instructed to create buffer areas extending roughly two blocks in all directions from each treatment area's perimeter. The researchers were then permitted to alter the displacement area boundaries to accommodate features of the local environment that might affect the occurrence of displacement. For example, if a public transportation line made it highly unlikely that crime could be displaced within the two-block guideline area, then it was amended to reflect this restriction. Each of the 20 displacement areas was entirely independent from any other treatment, control, or displacement area.[9]

In the analysis that follows, the pretreatment period (t_0) is the 90 days prior to the implementation of the OF tactic in each area. Recall, the treatment period (t_1) varies across the OF treatment areas (A), but the buffer areas (B) received the same treatment period as their respective treatment area. The treatment period is uniform across the seven control areas (C), spanning from the first and last implementation dates of all 20 OF areas (June 21, 2010–February 27, 2011).

During the 90-day pretreatment period (t_0), the counts for the all violent crimes outcome across the OF treatment (A), buffer (B), and control (C) areas were 268 (At_0), 380 (Bt_0), and 98 (Ct_0), respectively. For the treatment period (t_1), the OF treatment (A), buffer (B), and control (C) areas recorded counts of 522 (At_1), 713 (Bt_1), and 260 (Ct_1) on the all violent crimes outcome, respectively. These values are reflected in the WDQ equation:

$$\text{WDQ} = \frac{713/260 - 380/98}{522/260 - 268/98} = \frac{-1.135}{-.727} = 1.56 \tag{2}$$

The success measure of –.727 indicates that violent crime in the OF treatment areas increased during the treatment period but that the control areas increased even more. The buffer displacement measure of –1.135 indicates that crime decreased in the buffer

9. The displacement areas were drawn prior to the analysis stage. At that point, treatment and displacement effects were still hypothesized for all three treatment tactics. Therefore, displacement areas were drawn for all 60 treatment areas. In some instances, two or three of the displacement areas overlapped based on the predetermined two-block radius used. When the displacement areas overlapped during the design phase, the overlapping displacement areas were split so that each treatment area's displacement area was of equal distance from the edge of the respective treatment areas.

areas during the initiative. Standard interpretation of the overall WDQ of 1.56 suggests a diffusion of crime-control benefits. Also, it is possible that, given the nature of the treatment, spillover of the treatment into the displacement areas occurred and generated the crime reduction (Sorg et al., 2014). Nevertheless, we find no evidence of immediate spatial displacement. The WDQ for violent felonies outcome was 1.33, which has the same substantive interpretation as the all-crime outcome displacement analysis.[10] In sum, immediate spatial displacement does not seem to have occurred during the experiment for either significant outcome.

DISCUSSION

Research has demonstrated hot spots policing is effective at reducing crime; however, relatively little attention has been paid to the types of tactics police employ in hot spots (see Braga and Bond, 2008; Taylor, Koper, and Woods, 2011, for exceptions). In our examination of the effectiveness of three policing tactics, the OF sites produced significant decreases in violent crime, with decreases in 42 percent for all violence and 50 percent for violent felonies. Rather than finding immediate spatial displacement around the OF sites, we detected evidence of a diffusion of benefits. This section discusses the study's findings and suggests critical avenues for future research.

FOCUSING ON OFFENDERS IN HOT SPOTS REDUCED VIOLENT CRIME

When police focused on offenders causing problems in hot spots, they reduced violent crime by 42 percent and violent felonies by 50 percent compared with the controls. Theoretically, the OF tactic is based on the criminological axiom that a small percentage of offenders is responsible for a disproportionate amount of crime (Esbensen et al., 2010; Wolfgang, Figlio, and Sellin, 1972); however, the success of the OF treatment also has several possible practical explanations.

Although focusing on repeat offenders is a traditional policing tactic (Weisburd and Eck, 2004), its implementation in the OF sites had unique elements. The OF sites had a clear mission and a dedicated team of highly motivated officers who were exempt from answering calls for service and were supported by an intelligence analyst. Once the dedicated intelligence officer identified a list of offenders, the OF teams of officers selected from the districts' tactical squads could use their existing skill set with little additional training. Officer surveys showed they supported the notion of focusing on repeat violent offenders.

Beyond immediate crime reduction, OF policing has several potential ancillary benefits. First, a carefully implemented OF tactic can be less intrusive for law-abiding citizens. By focusing on specific suspected or known violent offenders, police can avoid wholesale increases in pedestrian and traffic stops, which disproportionately affect those living in

10. To save space, the calculations are shown in this footnote. The counts for the violent felonies outcome across the OF treatment (A), buffer (B), and control areas (C) during the 90-day pretreatment period (t_0) were 164 (At_0), 215 (Bt_0), and 55 (Ct_0), respectively. For the treatment period (t_1), the OF treatment (*A*), buffer (*B*), and control areas (C) recorded counts of 304 (At_1), 395 (Bt_1), and 164 (Ct_1) on the violent felonies outcome. The full WDQ equation is shown as follows: $\text{WDQ} = \frac{395/164-215/55}{304/164-164/55} = \frac{-1.501}{-1.128} = 1.33$

high-crime, often minority neighborhoods. A set of analyses not presented in this article found that no significant differences occurred in the numbers of pedestrian or car stops or narcotics incidents and supports the interpretation that OF enables police officers to be more judicious with their field investigations. Second, an add-on benefit of stopping the "right" people instead of a wide cross section of people may make it more likely that the community will perceive police actions as procedurally just (Tyler, 2003). In other words, if police simply go into a hot spot without a list of repeat offenders, then the likelihood they will use enforcement actions against law-abiding citizens is increased. When community members perceive that police actions are fair, they are more likely to be more satisfied with and have confidence in police services, follow the law, and help police fight crime (Reisig and Lloyd, 2009; Taylor and Lawton, 2014; Tyler, 1988, 2004). Surveys of community members from the experimental areas suggested they supported a police focus on repeat offenders. Therefore, focusing only on the most serious offenders in crime hot spots in theory reduces the likelihood that hot spots policing will produce "backfire effects" (Weisburd et al., 2011).

Durlauf and Nagin (2011) hypothesized that strategically shifting resources from prisons to police to conduct hot spots policing can simultaneously produce crime reductions while reducing prison populations. Philadelphia PD's experience with OF policing provides empirical support for their argument. With careful implementation, crime reductions can occur without the massive increases in pedestrian and car stops often found with saturation patrols; these outputs tend to produce many arrests that subsequently flood local jails, the court system, and the prison system (see Goldkamp and Vilcica, 2008). Not only could increasing resources to focus on prolific offenders in hot spots be effective, but also the tactic's focus on prolific offenders could benefit the whole criminal justice system.

OF policing efforts are frequently criticized for simply moving crime around, but we found crime reductions in adjacent areas and no evidence of immediate spatial displacement. The nature of the intervention offers one explanation for the lack of immediate spatial displacement of crime. A focus on the people who cause problems at a place naturally takes a more targeted view of the crime problem. If we assume the prolific offenders identified as targets by intelligence officers are operating in that hot spot area because it is particularly amenable to crime, then it is likely that places immediately adjacent to the hot spot may not have the same combination of attributes that make it criminogenic. Alternatively, if nearby areas have crime potential, then the targeted offenders may be unaware of the target area boundaries and reduce their offending generally. In addition, if arrests are made as a result of focused efforts, then those arrested will be incarcerated for a period of time and thus unable to contribute to an increased crime problem in adjacent areas.

PROBLEM SOLVING AND FOOT PATROL FAILED TO REDUCE VIOLENT CRIME

Neither FP nor POP resulted in statistically significant reductions in violent crime relative to controls. Two possible explanations are insufficient dosage and diversity in the hot spots (Sherman, 2007). As Sherman (2007: 311) observed, "there is no independent evidence of how much dosage is 'enough' for good results in problem-oriented policing or any evidence that dosage levels matter more than the content of the strategy." Thus, it impossible to determine how much dosage levels affected the results of this study. Citing

Weisburd's paradox, Sherman (2007) noted that much of the impact of an intervention may occur in the high-rate places and people, and thus, diversity in hot spots is an important factor. When more sites are included, the number of crimes at each site declines dramatically, and the overall diversity across observations increases. Thus, the police department's desire to deliver treatment to 60 places might have made it more difficult to find an effect because it required the inclusion of lower crime hot spots and lowered uniformity of hot spots within each tactic.

Hot spots of violent crime identified using violent crime rates for the previous year may not seem to be as hot when viewed through the lens of the preceding 90 days. Related to experimental design, hot spot analyses using the previous 1–3 years to identify reliably high violent crime places may need to be supplemented with analyses of more recent crime fluctuations for the previous 90 days to ensure the candidates for hot spots are still violent. Overall, more research is needed to understand more fully and quantify the short-term temporal stability of crime hot spots to choose areas accurately for hot spot policing.

POP and FP implementation also might explain the results. Turning to the POP sites first, POP was still relatively new in Philadelphia, and the officers assigned to work on the project were drawn from patrol. They were still responsible for answering radio calls during the initiative. Thus, unlike FP and OF, POP activity was conducted during downtime and at irregular and unpredictable periods between other duties. The result was that even with the additional support of a mentor at headquarters, POP teams undertook relatively "shallow" analyses. This result reflects the situation many agencies experience when shifting from a reactive to a proactive POP paradigm (Bullock, Erol, and Tilley, 2006). The instability of violent crime at small places over short time periods of weeks or months was also a factor. In almost half of the POP sites, after evaluating recent crime figures, police officers reported violence was not the biggest problem. Instead, it was property crime or quality-of-life offenses. Fieldwork and fidelity surveys confirmed that at least eight of the problem-solving areas switched to a focus on nonviolent crime and/or quality-of-life issues during the experiment. At least four sites focused on narcotics in addition to a specific violent or nonviolent crime problem. Another site was located in the territory of the "Kensington Strangler," a serial rapist and killer who was active during the study period (Rawlins, 2011). In the end, almost half of the POP sites did not implement POP targeting violent crime problems as the primary objective of the study site. It could be that those sites achieved crime reductions for the crime they focused on, but those reductions would not have appeared in the violent crime statistics that were the subject of this investigation. In sum, the implementation did not provide a strong test of POP, and consequently, the nonsignificant results produced as part of this experiment do not necessarily indicate that POP is an ineffective strategy.

In the case of FP, the officers actively patrolled the hot spots as assigned, but the design of FP in this experiment had two distinct differences compared with the previous experiment in Philadelphia. One difference was in the experience level of the officers. This experiment used veteran officers rather than officers with little experience. In supplementary analyses not presented in this article or in our online supporting information, we found no significant increases in police activity in the FP areas. That finding suggests the veterans were less aggressive in their enforcement than the officers with less experience from the Philadelphia Foot Patrol Experiment who increased pedestrian and vehicle stops by 64 percent and 7 percent, respectively (Ratcliffe et al., 2011). Veteran officers may have less interest in dealing with minor drug cases or simply perceive FP as a punishment and choose not to generate activity (Moskos, 2008).

The second distinct difference was in the dosage level. FP officers in this experiment spent only half the amount of time in hot spots than those who were part of the Philadelphia Foot Patrol Experiment (i.e., one shift instead of the two shifts per day) and the hot spots they patrolled were physically larger. It could be that there is a threshold of FP presence required to make a difference and that threshold was not met in this investigation. The timing of the dosage also was different. In eight sites from this experiment, officers worked exclusively during the day shift rather than during the busier late afternoon and early evening shift. In sum, the effectiveness of FP is contingent on the timing and duration of FP and on the activities undertaken by FP officers. Police management should not assume that deploying FP officers automatically reduces violent crime.

IMPLICATIONS FOR RESEARCH

Our research reveals several important implications for future evaluations of policing tactics in violent crime hot spots. First, the preceding discussion illustrates that it is not only *what* strategy is chosen but also *how* it is implemented. Investigating dosage requires a more systematic approach to measuring both time spent and activities undertaken by officers in hot spots. Many place-based experiments have focused exclusively on one aspect of dosage: the time spent in hot spots. One study used automatic vehicle location data to demonstrate that police presence can be precisely measured without the expense of field observation (Telep, Mitchell, and Weisburd, 2014). However, more research is needed to determine how much police presence is necessary to achieve a crime reduction in hot spots.

Measuring the component of dosage that captures police vigor or fidelity (i.e., consistency of treatment design with treatment implementation) is far more challenging and has received less attention (Sherman and Weisburd, 1995; Sorg et al., 2014) because of the difficulty and expense of collecting data describing the specific activities undertaken while assigned to FP, OF, and POP. In our study, the combination of official data, ad hoc field observations, and interviews provided only a basic measure of treatment fidelity. The shortcomings of the measures used in this investigation provide valuable information to researchers. As Eck (2003: 105) pointed out, "It is the unsuccessful cases that allow us to see the limits of interventions, reveal where we are ignorant, stimulate us to look further, and provoke our creativity." We could uncover only one study that applied a rigorous qualitative methodology to capture the types of actions undertaken during a POP evaluation (Braga and Bond, 2008). Developing and implementing systematic observations of police behavior as well as the use of technology to track both the "where" and the "what" of police activity should be considered whenever possible. Advances in technology have made it possible to build computer applications that can be used by officers to provide both detailed reports of activity and their location. Future research combining technology with qualitative research methods has promise for improving our understanding of police activity (see Esbensen et al., 2011, for a multimethod framework for program evaluation).

Second, the size of the hot spots seems to have bearing on the likelihood of a successful crime prevention outcome. In the current study, the foot beats were enlarged in response to officers' feedback that smaller foot beats did not provide enough activity over time and resulted in boredom (Sorg et al., 2014). But it might be that FP is only effective when concentrated in smaller areas because the average size of the hot spots in this experiment was two to four times larger than in experiments that experienced successful crime reduction efforts in POP (Braga and Bond; 2008; Taylor, Koper, and Woods, 2011) and FP (Ratcliffe et al., 2011). This may be the case for POP as well (see footnote 4).

Third, although violent crime is consistently concentrated at a relatively small number of places, it does not seem to be stable at the same small places over short time periods of weeks or months. In almost half of the POP sites, after evaluating recent crime figures, police officers reported violence was not the biggest problem, but it was property crime or quality-of-life disturbances. Thus, hot spots of violent crime identified using violent crime for the previous year may not appear as "hot" when viewed through the lens of the preceding 90 days. Hot spot analyses using the previous 1 to 3 years to identify reliably high violent crime places may need to be supplemented with analyses of more recent crime fluctuations over the previous 90 days to ensure the candidates for hot spots are still viable, at least from the perspective of the officers expected to implement the intervention. We must better understand and quantify the short-term temporal stability of crime hot spots in order to choose most accurately the areas for hot spot policing.

Fourth, conducting place-based field experiments is challenging and involves balancing the operational needs of law-enforcement agencies with the rigors of randomized experiments. Even with the support of the Police Commissioner and active participation in the design by other high-ranking personnel, the reality of personnel shortages put commanders in the difficult position of juggling immediate demands of citizen calls for service with the experiment's emphasis on proactive policing. Our results reflect the implementation of these policing tactics in a large, urban police department experiencing significant resource constraints. Replication is needed in different locations across different police departments.

CONCLUSION

This research was conducted to explore the question of which policing tactics reduce violent crime in small, violent crime hot spots. The offender-focused tactic implemented in violent crime hot spots achieved a 42 percent reduction in violent crime and a 50 percent reduction in violent felonies compared with their control areas. The effectiveness of a hot spot policing tactic that focuses on the people who are causing the problems in hot spots suggests the application of intelligence-led policing tactics might be fruitful at microlevel places. In addition, our results indicate that by focusing police efforts on the problem people associated with the problem places, police can achieve significant crime reductions while avoiding negative community perceptions of their actions (Haberman et al., 2014; Ratcliffe et al., 2012). Additional research is needed that more precisely measures what police officers do while in hot spots if we are to develop greater insight into why some crime reduction tactics are more successful than others.

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SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Appendix A. Hot Spots Identification and Assessment of Pretreatment Differences
  Table S.1. 2009 Violent Crimes Used to Delineate Deployment Areas
  Table S.2. Mann–Whitney U Tests Assessing Randomization Fidelity
Appendix B. Model Building
  Table S.3. Model Sets