

FOOT PATROL IN VIOLENT CRIME HOT SPOTS: THE LONGITUDINAL IMPACT OF DETERRENCE AND POSTTREATMENT EFFECTS OF DISPLACEMENT*

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This study revisited the Philadelphia Foot Patrol Experiment and explored the longitudinal deterrent effects of foot patrol in violent crime hot spots using Sherman's (1990) concepts of initial and residual deterrence decay as a theoretical framework. It also explored whether the displacement uncovered during the initial evaluation decayed after the experiment ended. Multilevel growth curve models revealed that beats staffed for 22 weeks had a decaying deterrent effect during the course

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of the experiment, whereas those staffed for 12 weeks did not. None of the beats had residual deterrence effects relative to the control areas. The displacement uncovered had decayed during the 3 months after the experiment, and it is theoretically plausible that previously displaced offenders returned to the original target areas causing inverse displacement. These results are discussed in the context of Durlauf and Nagin's (2011) recent proposal that prison sentences should be shortened, mandatory minimum statutes repealed, and the cost savings generated by these policy changes shifted into policing budgets to convey more effectively the certainty of detection. It is concluded that if Durlauf and Nagin's proposal is to succeed, then more holistic policing strategies would likely be necessary. Foot patrol as a specific policing tactic seems to fit nicely into a variety of policing paradigms, and suggestions for incorporating them to move beyond strictly enforcement-based responses are presented.

The police function envisaged by Sir Robert Peel was to provide an “unremitting watch” (Shearing, 1996: 74)—to deter offenders from committing crime through a uniformed patrol. However, the question of whether the police actually deterred crime did not come under academic scrutiny until the 1970s. The Kansas City preventative patrol experiment demonstrated that routine vehicle patrol across large geographic units was ineffective (Kelling et al., 1974), and it was followed by several evaluations concluding that foot patrol had no measurable impact on crime when similarly deployed (Bowers and Hirsch, 1987; Esbensen, 1987; Kelling, 1981; Pate, 1986). As America entered the 1990s, the prevailing sentiment was that, “The police do not prevent crime” (Bayley, 1994: 4). In response to evidence suggesting that the standard model of policing was ineffective, the decades that followed became an innovative time in American policing (Weisburd and Braga, 2006).

Of recent policing advances, hot spots policing is considered a promising strategic innovation (Braga, 2007). In light of this promise, Durlauf and Nagin (2011) argued that it is possible to manipulate the deterrence equation to emphasize the certainty of detection, in part through hot spots policing, as opposed to the severity of punishment through lengthy prison sentences and achieve reductions in both crime and prison populations. Durlauf and Nagin (2011: 39–40) did not make general recommendations about the tactics police should employ in hot spots if their policy proposal were to come to fruition, and they called for more research to determine the most appropriate responses. The debate over what police should do to reduce crime at hot spots is not new, and hot spots policing techniques often are met with concern. Much of this concern centers on the short-term crime reductions resulting from traditional police responses such as

crackdowns¹ and the potential for side effects such as spatial displacement (Rosenbaum, 2006).

Empirical investigations of the longitudinal impacts of crackdowns in hot spots are largely absent in the experimental literature but are, in light of this proposal, relevant to those allocating criminal justice funding. To make their case, Durlauf and Nagin (2011) highlighted the weaknesses of deterrence-based policies that rely on administering severe punishments, but understanding the limitations of hot spots policing tactics intended to convey certain detection is equally important. This research is undertaken to learn whether crackdowns in the form of foot patrols were susceptible to the criticisms that benefits are only short term, and shed light on whether responses such as crackdowns are likely to deliver the aggregate crime reductions predicted by Durlauf and Nagin (2011). The Philadelphia Foot Patrol Experiment is revisited to explore the deterrent effects of foot patrol in violent crime hot spots over time using Sherman's (1990) concepts of initial and residual deterrence decay as a theoretical framework. Although a previous evaluation found that foot patrols reduced crime, it is unclear whether this reduction was sustained once the "certainty communicating device" (Ratcliffe et al., 2011: 819) (i.e., foot patrol police) was withdrawn. Furthermore, a ratio measure is developed to test whether *inverse displacement*—previously displaced criminal activity flowing back into target areas after a policing initiative—might contribute to deterrence decay.

DURLAUF AND NAGIN PROPOSAL

A recent issue of *Criminology & Public Policy* (Volume 10, Issue 1) debated a policy proposal articulated by Durlauf and Nagin (2011). They posited that if policy makers rethink the ways in which the criminal justice system fosters deterrence, then it is possible to reduce crime, prison populations, and correctional spending. The point of departure from their proposals is that they advance a means by which crime *and* incarceration can be reduced simultaneously. Their appraisal of the deterrence literature led them to conclude that 1) increasing already excessive prison sentences will have, at best, a minimal marginal deterrent effect and 2) increasing police visibility by hiring more officers and organizing deployments in ways that increase the risk of apprehension seems to have "substantial marginal deterrent effects" (Durlauf and Nagin, 2011: 14). They advocated for reductions in prison sentence lengths, the repeal of mandatory minimum statutes,

1. In this article, we refer to crackdowns as Sherman (1990: 1) defined them: "Police crackdowns are sudden increases in officer presence, sanctions, and threats of apprehension either for specific offenses or for all offenses in specific places."

and a broad move away from policies that emphasize imposing severe punishments. They suggested the resulting monetary savings be used to supplement police budgets to increase perceptions of certain apprehension.

Beccaria (1963 [1764]) and Bentham (1948 [1789]) long ago theorized that to deter crime, the costs had to outweigh the benefits, the risk of apprehension had to be certain, and the severity of punishment had to be great and swiftly imposed. Thus, deterrence is theorized to be the result of interplay among certainty, severity, and celerity. Although punishment is a requisite to deter, Durlauf and Nagin (2011) concluded that relative to the impacts of certain detection, increasingly severe punishments have contributed comparatively less to the aggregate deterrent effect of the criminal justice system. This conclusion is reflected in their proposed reduction in sentence lengths from status quo levels, which they believed could be carried out with few, if any, negative consequences. Durlauf and Nagin (2011) pointed out the weakness in the imprisonment and crime literatures in making their case.

DETERRENT EFFECTS OF SEVERE PUNISHMENT

Some studies have found a negative association between aggregate levels of incarceration and crime rates, and this relationship has been interpreted as evidence of a deterrent effect (see Donohue, 2009). Durlauf and Nagin (2011) are skeptical of this literature for reasons including the following. Several of these studies treat imprisoned populations as a policy variable where the number of persons incarcerated is more realistically an *outcome* of an overall sanction policy. Therefore, these studies cannot control for other variables influencing crime and incarceration. This relationship also may be spurious and should not be interpreted as a *causal* relationship. Finally, the statistical modeling in many of these studies is flawed. Aggregate regression analyses have statistical assumptions whose validity is highly problematic (Durlauf, Navarro, and Rivers, 2008). As a result of model uncertainty, conflicting results using the same data set have been reported. As a whole, Durlauf and Nagin (2011) concluded that this literature provides little convincing evidence that housing greater numbers of inmates for lengthier periods has contributed much to the aggregate deterrent effect of the criminal justice system.

Durlauf and Nagin (2011) also reviewed studies that examined the deterrent effects of changes in policies intended to increase punishment severity. For example, California's "three strikes" law mandating 25 years to life for three-strikes-eligible convictions has received considerable attention. The results are heterogeneous. Stolzenberg and D'Alessio (1997) concluded that in 9 of 10 California jurisdictions, the implementation of three strikes laws did not reduce serious crime. In contrast, Zimring, Hawkings,

and Kamin (2001) concluded that the law reduced felonies by 2 percent. However, only those with two previous strikes-eligible convictions seem to have been deterred. Helland and Tabarrok (2007) also found that offending was lower for those convicted of two strikes-eligible offenses, and they estimated that the law deters 31,000 crimes per year. They also estimated that the cost of incarcerating three-strikes offenders is approximately \$4.6 billion or approximately \$150,000 per crime avoided. These estimates are relevant to Durlauf and Nagin's (2011) proposal, as it is estimated that the same spending on hiring additional police could prevent approximately 1 million crimes, a far greater cost/benefit (Donohue, 2005; Helland and Tabarrok, 2007). Durlauf and Nagin (2011) concluded that the costs of laws mandating broad increases in sentence lengths far outweigh the deterrent effects.

Durlauf and Nagin (2011) reviewed several other evaluations of policies intended to deter crime through increasing punishment severity, such as sentence enhancements for crimes involving firearms (Loftin and McDowall, 1984) or the precursor to California's "three strikes" laws, known as proposition 8, which mandated enhanced penalties for repeat offenders (Kessler and Levitt, 1999). After an exhaustive review, they deduced that the crime reduction benefits of increasing punishment severity are modest at best. As a result, Durlauf and Nagin (2011: 31) concluded that, "the marginal deterrent value of increased sentence length at current levels is small for contexts in which sentences are currently long."

DETERRENT EFFECTS OF HOT SPOTS POLICING

Empirical support for the deterrent effects of increasing the certainty of detection seems more promising (Durlauf and Nagin, 2011: 17). In particular, hot spots policing has a solid evidence base (Braga, 2007; Weisburd and Braga, 2006). Hot spots policing involves focusing police resources on high-crime locations—"addresses, buildings, block faces, street segments, or clusters of addresses" (Mastrofski, Weisburd, and Braga, 2010: 251) with an above-average concentration of crime (Eck and Weisburd, 1995; Sherman, Gartin, and Buerger, 1989; Sherman and Weisburd, 1995). In the first experimental evaluation of hot spots policing, Sherman and Weisburd (1995) found that doubling preventative patrols at hot spots reduced crime by 6 to 13 percent, and the prevalence of disorder was approximately 50 percent lower in the targeted areas compared with the control. A number of evaluations followed.

Sherman and Rogan (1995a) found that raids on crack houses reduced violent and property crime by 24 and 3 percent, respectively. Likewise, Sherman and Rogan (1995b) found that a 65 percent increase in gun seizures in a targeted beat coincided with a 49 percent decrease in gun crimes during a quasi-experimental evaluation in Kansas City, Missouri.

Another evaluation in Jersey City, New Jersey found that drug markets targeted with crackdowns had significantly fewer disorder calls for service during the 7 months after the intervention compared with controls (Weisburd and Mazerolle, 1995). In Philadelphia, foot patrols reduced violent crime by 23 percent relative to controls (Ratcliffe et al., 2011). These evaluations suggest that increasing the certainty of detection at hot spots with heightened enforcement can prevent crime.

Another method for reducing the opportunity for crime and increasing the certainty of detection at crime hot spots is problem-oriented policing (Goldstein, 1979, 1990). In Jersey City, New Jersey, a problem-solving strategy focused on addressing physical and social disorder resulted in decreases in reports of assault, robbery, and property crimes (Braga et al., 1999). In addition, a randomized experiment evaluating problem-oriented policing found reductions in assault (34.2 percent), robbery (41.8 percent), burglary (35.5 percent), and disorderly/nuisance behaviors (14.0 percent) (Braga and Bond, 2008). In sum, both problem-oriented policing and targeted enforcement seem effective when focused in hot spots. This conclusion is reflected in Durlauf and Nagin's (2011) suggestion that monetary savings resulting from reducing sentence lengths be used to supplement police budgets. However, critiques relevant to this proposal deserve consideration.

CRITIQUES OF HOT SPOT CRACKDOWNS

Rosenbaum (2006) outlined several critiques of hot spot policing, particularly relevant to deployments involving crackdowns (see also Kochel, 2011). Two of these critiques are addressed by the current work: 1) Crime reductions elicited by crackdowns are short term and decay rapidly, and 2) taking a geographic focus may result in spatial displacement or the movement of crime to other geographic locations (Reppetto, 1976).²

INITIAL AND RESIDUAL DETERRENCE DECAY

Empirical evidence does suggest that crackdowns do not have lasting effects. For example, Sherman and Rogan's (1995a) evaluation of raids on crack houses found a reduction in calls for service, but these effects decayed within two weeks, or resulted in "residual deterrence decay" (Sherman, 1990: 10). As Sherman noted, crackdowns work through changing an offender's perceived risk of being caught through heightened policing, but crackdowns are rarely indefinite. Therefore, because the underlying causes of crime are not typically addressed, once a crackdown ends, offenders

2. Only spatial displacement is examined in this study, but other types exist (see Eck, 1993).

will begin to reoffend. Theoretically, it makes sense that their benefits will largely be observed when they are in effect.

Evidence shows that the effectiveness of crackdowns declines while they are ongoing, or results in “initial deterrence decay” (Sherman, 1990: 10). In reviewing several case studies of crackdowns, Sherman (1990) noted that in some cases, crime reductions began to decay while crackdowns were still under way. According to Sherman, one explanation for initial deterrence decay is that offenders overestimate the certainty that criminal behavior will be detected when a crackdown begins. Over time, offenders may recognize that they overestimated this risk and begin reoffending. Therefore, an initial decline in crime may emerge at the onset of a crackdown, but according to his theory, the returns diminish as offenders recognize that apprehension is not certain.

Initial and residual deterrence decay is problematic when put in the context of the Durlauf and Nagin (2011) proposal. If crackdowns do not have lasting effects and these effects decline while crackdowns are under way, then it seems unlikely that these and other targeted enforcement techniques are capable of eliciting aggregate and lasting crime reductions. However, Sherman (1990) did provide some guidance on how to best harness the crime reduction benefits of crackdowns. Sherman (1990) suggested that crackdowns should be short term and randomly rotated across numerous locations to optimize their effectiveness. This strategy would improve the efficiency of crackdowns by avoiding initial deterrence decay and capitalizing on residual deterrence decay, as Sherman noted that decay following a crackdown is slow. In other words, a benefit, albeit a decaying one, is observed after a short-term crackdown. Although Sherman (1990) reviewed several case studies in which initial and residual deterrence decay was documented, the hot spots literature, particularly evaluations of randomized controlled trials, largely leaves these phenomena unaddressed.

SPATIAL CRIME DISPLACEMENT

Rosenbaum (2006) argued that geographically concentrating police resources could result in spatial displacement. The available evidence suggests that this is not a definite outcome of place-based initiatives, and displacement rarely overwhelms crime reduction benefits (Guerette and Bowers, 2009). For example, only one of the five studies reviewed by Braga (2007) that measured displacement found any evidence that it occurred. Likewise, Eck (1993) noted that more than half of the evaluations he reviewed found no evidence of displacement. Finally, in the only study designed specifically to measure displacement during a hot spots program, Weisburd et al. (2006) found no evidence that displacement resulted. In

sum, spatial displacement remains a possibility during place-based initiatives, but its effect is generally marginal.

The fact that any displacement occurs, or could occur, is problematic. In addition to raising questions about the novelty of practices that disperse crime, there should be concern over pushing crime into locations where it did not exist previously. Displacement that reaches a point where communities are cognizant of a crime increase could diminish police–community relations, negatively impact perceptions of police legitimacy, and raise concerns over inequitable policing practices. When the police are perceived as illegitimate, citizens are less likely to participate in neighborhood watch, attend community meetings, collaborate with police in problem-solving initiatives, report crimes, and participate in investigations (Kochel, 2011). Despite evidence suggesting that displacement is not common, its potential consequences have implications for long-term crime control.

Whether a community will notice a crime increase resulting from displacement is not clear. Because much of the discourse on displacement suggests that its effects are marginal, a short-term increase in crime, if noticed at all, may not result in the negative consequences discussed. This begs the question of whether spatial displacement is a long- or short-term outcome. When a policing initiative ends, does displaced crime remain elevated in these areas or does it decline and return to treatment locations? Although short-term crime fluctuations may not devastate communities and perceptions of police, one might predict otherwise if crime increases become lasting problems. No previous evaluations of which these authors are aware have assessed whether displacement was a long- or short-term outcome.

CURRENT RESEARCH

The current work makes the following contributions. First, crime changes in targeted areas are quantified after the Philadelphia Foot Patrol Experiment concluded. Specifically, residual benefits and the extent of residual deterrence decay are measured at conclusion of the experiment, and these effects are estimated in relation to control areas. Second, multilevel growth curve models were used to estimate initial deterrence decay during the experiment. Third, an adaptation of a ratio measure commonly used to estimate displacement is introduced as means to examine the role of *inverse displacement* as a possible cause for previously displaced crime flowing back into target areas postoperation.

PHILADELPHIA FOOT PATROL EXPERIMENT

Until the Philadelphia Foot Patrol Experiment, foot patrols were considered capable of improving community relations and of reducing fear of crime (Cordner, 1986) but incapable of reducing crime (Bowers and

Hirsch, 1987; Kelling, 1981; Pate, 1986). However, previous evaluations spread officers across large geographic areas, likely reducing their ability to deter. In Philadelphia, Commissioner Charles Ramey's support for foot patrols led to a collaboration between Temple University researchers and the Philadelphia Police Department to measure the deterrent effects of foot patrols in microlevel hot spots (Ratcliffe et al., 2011).

During the summer of 2009, 240 rookie police officers were assigned to 60 of Philadelphia's violent hot spots as part of a randomized experiment after their graduation from the police academy. They patrolled in pairs on a day (10 A.M. to 6 P.M.) and night (6 P.M. to 2 A.M.) shift, 5 days a week (Tuesday through Saturday). They were deployed in two phases coinciding with academy graduation dates. Phase 1 deployed March 31, 2009 and terminated August 31, 2009, and phase 2 deployed July 7, 2009 and terminated September 28, 2009, for 22 and 12 weeks, respectively. However, some police commanders chose to continue deploying these rookie officers on foot after the experiment ended. In discussing this with various district commanders, it seems that even if the foot patrols remained, they were not staffed with the same frequency, and in some cases, the officers were working different locations.

Temple University researchers assisted the Philadelphia Police Department in identifying the experimental areas. Violent crime event data, including homicide, certain categories of aggravated assault, and robbery, were extracted from the Philadelphia Police Department's incident database for the 3 years prior to the experiment (2006–2009).³ The records in this database are geocoded by the department's system at roughly a 98 percent hit rate. Violent crimes were weighted such that crimes occurring more recently were most influential in identifying the hot spots, but also so long-term trends contributed to their creation (2008 = 1.0; 2007 = .5; 2006 = .25). Incidents were aggregated to a set of Thiessen polygons centered on street intersections (see Chrisman, 2002). A local Moran *I* test was performed, and the resulting clusters of high-crime street corners were mapped and presented to the Philadelphia Police Department leadership. It should be noted that crime event data are influenced by police decision making (Black, 1980) and local structural characteristics (Varano et al., 2009). It is possible that the hot spot identification and the results would have been different if calls for police service data were used; however, these data were not available.

With the stipulation that each beat must contain at least one of the highest crime corners, the department's leadership drew 129 foot beats. Temple University researchers adjusted beats that were deemed too large or

3. Categories of assault and robbery that police patrols were unlikely to influence were excluded from the analysis.

overlapped with other beats; this process yielded 124 potential foot beats. Because the police department could staff a maximum of 60 beats during peak crime hours, the four lowest crime beats were dropped, resulting in 120 experimental areas (60 target foot beats and 60 control foot beats). The treatment and control beats averaged 1.3 miles of streets, 23.9 street segments, and 14.7 street intersections. The beats were assigned to treatment and control groups via a randomized block design.⁴ Vehicle patrol officers continued to patrol and respond to calls for service within treatment and control locations, which meant both types of areas received a “business as usual” dosage. Neither foot nor vehicle patrol officers were aware of where the control beats were located.

Buffer zones were drawn around the treatment locations to measure displacement. The buffer zones were slightly larger, on average, than the treatment locations. The buffer zones averaged 2.8 miles of street, 67.8 street segments, and 27.1 street intersections. Buffer zones were first drawn two blocks past the target areas based on precedent within the literature⁵ (Braga and Bond, 2008; Braga et al., 1999; Weisburd and Mazerolle, 1995; Weisburd et al., 2006). Contextual knowledge was then incorporated into their

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4. Ratcliffe et al. (2011: 806–7) reported that, “Independent samples *t* tests indicated no significant difference between treatment (mean = 5.98; standard deviation [SD] = 4.04) and control groups (mean = 4.93; SD = 3.34) on pretreatment violent crime counts [$t(118) = -1.55, p > .10$] (two tailed). An independent samples *t* test found no significant differences in the amount of area encompassed by treatment ([mean] $M = 891,953$; $SD = 305,506$) and control ($M = 833,038$; $SD = 332,537$) groups [$t(118) = -1.01, p > .10$], the length of road (ft) contained within treatment ($M = 6,957$; $SD = 2,212$) and control ($M = 6,631$; $SD = 2,084$) groups [$t(118) = -.83, p > .10$], or the number of intersections contained within treatment ($M = 15.42$; $SD = 5.21$) and control ($M = 14.02$; $SD = 5.38$) groups [$t(118) = -1.45, p > .10$].” The authors noted that when a paired samples *t* test was run on pretreatment violent crime counts, a minor yet statistically significant difference was found between the treatment and control locations [$t(118) = 2.03, p < .05$]. Shadish, Cook, and Campbell (2002) noted that even when matched designs are used, randomization may result in mean differences between groups; however, randomization negates the possibility that these differences are a result of systematic bias. We control for these minor differences by entering a pretreatment crime variable at level two.
 5. As Weisburd and Mazerolle (1995: 354) noted, “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 Bowers and Johnson (2003) and Ratcliffe and Breen (2011).

design. Four field researchers observed each pair of officers four times. As part of the observations, researchers noted adjacent locations particularly amenable to displacement (for example, it had similar land uses). Buffer zones were adjusted past two blocks if necessary. Buffer zones could not overlap the experimental areas and could not cross obvious physical barriers. Because some beats were in close proximity to one another, some buffer zones were combined. In total, 10 foot patrol areas shared a buffer zone with another beat, resulting in 55 total buffer zones.

The analysis found that violent crime was 23 percent lower in the target locations relative to controls during the experimental period (Ratcliffe et al., 2011). However, crime reductions were conditioned on levels of pretreatment violence; only beats within or above the 60th percentile for crime counts in the 90 days prior to implementation saw statistically significant reductions. The weighted displacement quotient was used to measure spatial displacement (Bowers and Johnson, 2003). An increase in crime in the buffer zones during the course of the experiment relative to the controls was uncovered, but the amount of displacement was less than the overall treatment effect (weighted displacement quotient [WDQ] = .41). Calculation of the total net effect (Clarke and Eck, 2005) indicated that there were 90 fewer crimes in the treatment locations relative to controls, and this number was offset by 37 crimes displaced, for a total net effect of 53 fewer crimes in treatment locations.

The two tactical elements theorized to have elicited the crime reductions were “presence” and “sanctions” (Sherman, 1990: 8). During the course of the experiment, 120 officers provided 57,000 hours of spatially focused police presence, which Ratcliffe et al. (2011: 819) suggested allowed them to act as a “certainty communicating device.” The spatially concentrated police presence likely reduced offending by communicating that the detection of crime was certain. The second tactical element involves meting out “sanctions.” During the experimental period, foot patrol officers contributed substantially to a 64 percent increase in pedestrian stops, a 7 percent increase in vehicle stops, and a 13 percent increase in arrests within the treatment locations compared with 3 months prior to the experiment.⁶ In comparison, pedestrian stops in the control locations increased by less than 1 percent, vehicle stops declined by 13 percent, and arrests declined by 2 percent from before the operation to during the operation. Ratcliffe et al. (2011) suggested that increasing field interviews and arrests might be especially effective in deterring wanted individuals or those carrying illegal weapons. These offenders may have avoided these public spaces

6. Qualitative observations conducted during the experiment revealed that the foot patrol officers were conducting vehicle stops. Typically, the officers would stand on a corner and wave people over if vehicle infractions were observed.

in an attempt to avoid police encounters (Goffman, 2009). Both of these predictions are derived from deterrence theory.

The fact that all of the officers involved in the experiment were rookies deserves discussion. Because the officers were new, it is possible that they were especially motivated to reduce crime and be proactive, which could have enhanced the net gains. Conversely, their inexperience may have impeded their ability to respond to crime in the most efficient manner or in ways that would make long-term differences. Therefore, replication in the future using more experienced officers could be useful in analyzing how the uniqueness of rookie officers may have impacted the findings.

Also, it is worth noting that the qualitative observations conducted during the experiment suggested that some officers also worked on building relationships with the community (see Wood et al., in press). They commented that these relationships resulted in useful intelligence that gave them a better sense of the problems afflicting the neighborhoods and who were prolific offenders. Whether these efforts contributed to the crime reductions achieved during the experiment is difficult to say, but it cannot be ruled out as a possibility.⁷ This topic is addressed more extensively in the policy implications discussion.

ANALYTIC APPROACH AND RESULTS

MULTILEVEL GROWTH CURVE MODELS

One focus of this study is to determine whether the deterrent effects of foot patrol in violent crime hot spots varied over time and whether it was retained after the experiment. Because these research questions and experimental design have a nested data structure—changes in violent crime over time (level 1) nested within treatment and control areas (level 2)—multilevel growth curve models (MGCMs) are employed. This technique can test hypotheses about the time-varying effects of experimental treatments at level 1 while controlling for temporally stable differences across experimental areas at level 2 (for a recent example, see Corsaro, Brunson, and McGarrell, 2009). More technically, MGCMs

7. If this did contribute to the crime reductions in a meaningful way, then one way to test this would be to run a growth model with lagged treatment variables; this relationship would theoretically be lagged because it would take time to build these relationships and for a treatment effect to materialize. We, therefore, ran a lagged model using the methods described, and yet with a 2-week lag where treatment period one was transformed into a nontreatment period, time two became time one, time three became time two, and so on. These models did not yield significant results (phase 1 lagged treatment, $t = -.936$, $p = .35$; phase 2 lagged treatment, $t = -1.599$, $p = .11$).

provide unbiased parameter estimates for nested data structures (Bryk and Raudenbush, 1987).

This technique is advantageous for several reasons. With MGCMs, the intercept represents the expected value of the outcome (here, violent crime) averaged across level 2 units (here, experimental beats), at the start of a time series (here, the first treatment time block). Differences across level 2 units that affect the outcome are accounted for by predicting the intercept with control variables at level 2. This step is important because it is necessary to control for pretreatment levels of violence across the beats to remove variability in the dependent variable (Shadish, Cook, and Campbell, 2002: 51). The models outlined in this study control for these differences by controlling for pretreatment counts of crime at level 2. In addition, the analysis of posttreatment effects requires controlling for levels of crime at the beginning of the posttreatment period to remove variability in the dependent variable after the treatment was administered. The models outlined control for these differences by entering the residual values of each experimental area during the final treatment time period from the “treatment effects” model as a level 2 variable (as discussed in detail in the following sections). Finally, this technique can model the conditional deterrent effect that Ratcliffe et al. (2011) found was predicated on pretreatment violence. In MGCM, time-varying covariates at level 1 may be treated as fixed or their slopes specified as random, allowing their effects to be predicted by level 2 variables to estimate between-unit differences in the outcome. The treatment variables discussed can be specified as random and then predicted by pretreatment crime counts to estimate this interaction.

To assess the effects of the Philadelphia Foot Patrol Experiment over time and at its conclusion, two-level growth curve models with biweekly time blocks nested within treatment and control hot spots are estimated (Raudenbush and Bryk, 2002). Four separate models are run: 1) a model estimating treatment effects, 2) a model estimating initial deterrence decay, 3) a model estimating posttreatment effects, and finally, 4) a model estimating residual deterrence decay. All models are specified as Poisson distributions with overdispersion (Raudenbush and Bryk, 2002). The differences in geographic size across the experimental areas are controlled by introducing an exposure variable of geographic area (sq. ft.). All variables are entered uncentered.

OUTCOME

The dependent variable is violent crime counts in each of the treatment and control hot spots aggregated to 2-week time periods. The same violent crime incident categories used during the foot patrol experiment

evaluation make up the outcome variable: 1) homicides, 2) robbery (excluding cargo thefts), and 3) pertinent classifications of aggravated assaults, excluding categories that foot patrols are unlikely to impact, such as assaults against police or assaults in schools. These data were extracted from the Philadelphia Police Department's incident database and aggregated to biweekly time blocks. For all models, the first biweekly time period begins approximately 1 year (April 1, 2008) prior to the deployment of the phase 1 foot patrols (deployed March 31, 2009). Because the four models answer distinct research questions, the end dates differ across the models (as discussed subsequently).

MODELS

Level 1 variables are time-varying covariates. Each model includes a linear and quadratic time variable accounting for the position of the biweekly data point in the time series at level 1. Because of the well-established link between season/temperature and violence (see Rotton and Cohn, 2002), which was recently demonstrated for robbery in Philadelphia (Sorg and Taylor, 2011), the average high temperature corresponding to each biweekly block is entered in all models at level 1. These data were collected from an online weather archive (Weather Underground, 2011) used in previous research (Ratcliffe, Taniguchi, and Taylor, 2009; Sorg and Taylor, 2011). As noted, the phase 1 foot patrols were implemented for 22 weeks (11 biweekly treatment periods) and the phase 2 foot patrols for 12 weeks (6 biweekly treatment periods), so the treatment and posttreatment variables discussed below are separated by phase. Descriptive statistics are reported in table 1.

Treatment Effects. The first model tests the impacts of foot patrol during the experimental period and the conditional deterrent effects reported by Ratcliffe et al. (2011). As noted, the first biweekly time block begins on April 1, 2008. The final biweekly block ended on September 28, 2009, the final day of the phase 2 foot patrols. At level 1, a dichotomous variable is entered to examine whether significant differences exist in expected violent crime counts in target areas relative to controls during the experiment. Phase 1 beats were coded "1" for all time blocks beginning March 31, 2009 and ending August 31, 2009. Phase 2 beats were coded "1" for all time blocks beginning July 7, 2009 and ending September 28, 2009. Both treatment and control areas were coded "0" for time blocks preceding and during the experiment. At level 2, the total number of violent crime incidents occurring during the 3 months before the experiment is entered as a control variable. To estimate the interaction between pretreatment violence and the effects of treatment, the phase 1 and phase 2 treatment variables'

Table 1. Descriptive Statistics of Violent Crime^a per Biweekly Time Block (N = 120)

Phase	Status/Time	Dates	Mean	Median	SD	Min	Max
1	Target						
	Pretreatment	4/1/08 to 3/30/09	1.39	4.0	1.39	0	8
	During treatment	3/31/09 to 8/31/09	1.17	3.0	1.22	0	6
	Posttreatment	9/1/09 to 11/23/10	1.25	3.0	1.33	0	6
1	Control						
	Pretreatment	4/1/08 to 3/30/09	1.36	3.5	1.34	0	7
	During treatment	3/31/09 to 8/31/09	1.23	3.5	1.36	0	7
	Posttreatment	9/1/09 to 11/23/10	1.17	2.5	1.20	0	5
2	Target						
	Pretreatment	4/1/08 to 7/6/09	.80	4.5	1.06	0	9
	During treatment	7/7/2009 to 9/28/09	.63	2.0	.84	0	4
	Posttreatment	9/29/09 to 12/22/10	.59	2.0	.87	0	4
2	Control						
	Pretreatment	4/1/08 to 7/6/09	.74	3.5	.96	0	7
	During treatment	7/7/2009 to 9/28/09	.61	3.5	.97	0	7
	Posttreatment	9/29/09 to 12/22/10	.64	2.0	.81	0	4

SOURCE: Philadelphia Police Department INCT database (2008–2010).

ABBREVIATIONS: Max = maximum; Min = minimum; SD = standard deviation.

^aViolent crime includes pertinent categories of homicide, robbery, and aggravated assault.

slopes are specified as random and predicted by the level of violence during the 3 months prior to treatment at level 2.

Initial Deterrence Decay. The second model employs a linear decay function to measure initial deterrence decay during the treatment period. Again, the first biweekly block starts on April 1, 2008 and the data sequence ends on September 28, 2009. For the phase 1 “initial deterrence decay” variable, the first biweekly data point during the treatment period is coded “11,” with each subsequent time block decreasing linearly to “1” for the last 2 weeks of the phase 1 treatment period. The phase 2 “initial deterrence decay” follows the same coding scheme, with the first treatment period being assigned as time “6” and the subsequent treatment period time blocks decrease linearly to time “1.” Operationalizing the variable in this manner allows for the estimation of the expected difference between the treatment and control beats from the beginning to the end of the treatment period.⁸

8. The results are interpreted as the average expected differences (either increase or decrease in crime) between the treatment and control beats per time period. With the linear decay function, one can calculate the expected difference at treatment time 1 (coded here as time 11 for the phase 1 beats) and discern (if statistically significant) the expected decline in the differences between the treatment and control beats at each time period by multiplying the percentage difference

All other time blocks for the target beats are coded as “0.” All control beats are assigned values of “0” for each time block.

Posttreatment Effects. This model estimates the impacts of foot patrol after the experimental period; therefore, posttreatment time blocks are added to the data sequence. The first biweekly block of these data also begins on April 1, 2008, and yet the final time block has an end date of December 22, 2010, approximately 3 months after the phase 2 foot patrols were terminated. Separate dummy variables for phase 1 and phase 2 are entered at level 1 to assess whether target locations had lasting effects during the 3 months after the experiment ended. The targeted areas are coded “1” during each biweekly period during the 3 months after the phase ended. For phase 1, the first posttreatment period begins on September 1, 2009 and ends on November 23, 2010. For phase 2, the first posttreatment period begins on September 29, 2009 and ends on December 22, 2010. The control areas are coded “0” during this posttreatment period. Both treatment and control areas are coded “0” for all other time blocks.

At level 2, the pretreatment crime counts are replaced with the residual values for each experimental area during the final treatment time block. Recall the model estimating treatment effects discussed previously, which measures the experimental impacts of foot patrol during the treatment period only. As part of model estimation, HLM 6.06 (Scientific Software International, Skokie, IL) software produces a residual file for both level 1 and level 2. After executing the model estimating treatment effects, the residual values during the final treatment period were retrieved from the level 1 residual file. This variable was included in the data file of this posttreatment model to control for across-beat differences at the start of the posttreatment period. These residual values are the discrepancies between 1) the fitted values, or the predicted score based on the specified model, and 2) the observed values, or the actual count of violent crime during that biweekly period (Raudenbush et al., 2004: 15). In other words, residual values represent the variation in the dependent variable that persists after controlling for all variables entered in a model. By accounting for

by the time block. For example, if treatment beats had levels of crime that were 2.5 percent lower than the controls at the first treatment period, with this coding we could multiply the percentage difference by time block [$2.5(11) = 27.5$] and discern that at time one, treatment beats had overall expected crime counts that were 27.5 percent lower than the controls. To find the differences at the end of the treatment period, one simply multiplies the event rate ratio by that time block, which as coded here would be time 1, meaning that by the end of the treatment period, there was only an expected 2.5 percent difference between the treatment and control beats [$2.5(1) = 2.5$]. This process allows for a direct test of Sherman's (1990) concept of initial deterrence decay.

these differences at level 2, this variable ensures that variability in the dependent variable related to any factor not included in the treatment effects model is controlled for at the start of the posttreatment period.

Residual Deterrence Decay. In this model, a linear decay variable is employed to test whether there was residual deterrence decay when the experimental period ended. This variable follows the same coding scheme as the initial deterrence decay variable discussed above. For both the phase 1 and phase 2 beats, the first posttreatment time block for the target areas is coded “6” and the last time block is coded “1” for the 3-month posttreatment period. All other biweekly data points are coded “0,” and control beats are assigned “0” values for all data points. The residual values remain at level 2.

RESULTS⁹

Results of the four models are displayed in table 2. The experimental effects model displays the differences in the expected violent crime counts between the treatment and control areas during the experimental period. Controlling for area (exposure variable), linear and quadratic time, and temperature at level 1 and pretreatment violence at level 2, foot patrol, during both phases 1 and 2, had significantly lower expected violent crime counts than controls—an average of approximately 16 and 20 percent, respectively. The outcome also is linked to temperature, where a 2°F increase in average temperature results in approximately a 1 percent increase in expected violent crime counts. Beats with higher levels of pretreatment violence had expected violent crime counts that were approximately 5.5 percent higher during the treatment period. When the phase 1 and phase 2 treatment slopes are specified as random, neither phase 1 nor phase 2 produced statistically significant random effects (phase 1 random slope, $p > .500$; phase 2 random slope, $p = .467$). In addition, the reliability estimates for both variables dropped to insufficient levels (phase 1 = .007; phase 2 = .072). It was, therefore, inappropriate to model the cross-level interaction.¹⁰

9. Two preliminary models were run. The unconditional model controlling only for exposure confirmed a significant variation exists in the outcome across the experimental sites ($p < .01$). A one-way analysis of covariance model confirmed that this between-site variation remained significant when controlling for linear and quadratic time and temperature at level 1 (see Raudenbush and Bryk, 2002: 23–9).

10. This result may seem contradictory to previous findings; however, the data and analyses conducted by Ratcliffe et al. (2011) were cross-sectional and aggregated to a pretreatment and during-treatment period. Our data encompass a longer pretreatment period, we control for seasonal fluctuation with the temperature

**Table 2. Multilevel Growth Curve Modeling Results
(N = 120)**

Fixed Effect	Experimental Effects		Initial Deterrence Decay		Posttreatment Effects		Residual Deterrence Decay	
	ERR	(SE)	ERR	(SE)	ERR	(SE)	ERR	(SE)
Level 1								
Time	.999	(.009)	.998	(.009)	.995	(.009)	.995	(.003)
Time ²	.999	(.001)	.999	(.001)	.999	(.001)	.999	(.001)
Temperature	1.005**	(.001)	1.005**	(.001)	1.004**	(.001)	1.005**	(.001)
Phase 1	.842**	(.056)	.980**	(.056)	1.062	(.079)	1.017	(.017)
Phase 2	.801**	(.086)	.955	(.086)	.887	(.073)	.968	(.017)
Intercept	13.896**	(.111)	14.436**	(.111)	29.718**	(.111)	29.696**	(.099)
Level 2								
Pre VC	1.005	(.009)	1.005	(.009)				
Residual					1.988	(.029)	.988	(.029)
Random Effect	χ^2	Variance	χ^2	Variance	χ^2	Variance	χ^2	Variance
Intercept	1009.785**	.149	1011.080**	.150	1769.015**	.248	1769.878**	.248
(d.f.)	(118)		(118)		(118)		(118)	
Overdispersion	1.220		1.221		1.213		1.213	
Parameter								

NOTES: All models specified as Poisson distributions with overdispersion. All models have an exposure variable of geographic area (sq. ft.). The outcome is violent crime counts per beat/per biweekly time block. Phase 1 and phase 2 are dummy variables in experimental effects and posttreatment effects models and linear decay variables in initial deterrence decay and residual deterrence decay models.

ABBREVIATIONS: ERR = event rate ratio; Pre VC = summed count of violent crime 3 months prior to the experiment; SE = standard error.

* $p < .05$; ** $p < .01$.

The initial deterrence decay column reports the effects of initial deterrence decay during the treatment period net of controls. The initial deterrence decay variable for phase 1 reaches statistical significance, whereas the phase 2 coefficient does not, suggesting a declining treatment effect during the experiment for phase 1 only. During the first treatment period of phase 1 (coded here as “11”), the target beats had crime counts that were approximately 22 percent lower than the control beats [$.02(11) = .22$], but during the final 2 weeks of treatment, the phase 1 beats had expected crime counts that were only 2 percent lower than the control areas. Again, a 2°F increase in temperature results in approximately a 1 percent increase in expected violent crime counts.

variable, and our analysis separates the treatment periods by phase, whereas the previous evaluation did not. Therefore, these data and analyses are patently different than those used by Ratcliffe et al. (2011). It may be that, as operationalized, our analysis is masking the interaction effects that were found previously. Table 2 reports the results only from the fixed slopes.

The posttreatment effects column reports the impacts of the foot patrols after the experimental period net of controls. For both the phase 1 and phase 2 beats, no significant differences were found between the treatment and control areas. The insignificant findings suggest that foot patrol did not have lasting impacts on crime once the officers were removed from the beats. The temperature effect retained statistical significance.

The final model estimates the effects of residual deterrence decay during the posttreatment period. As with the posttreatment variables, the residual deterrence decay variables did not reach statistical significance when all other variables were held constant. This finding suggests that no significant differences were found between the treatment and control areas on levels of violence from the beginning to the end of the posttreatment period. This model finds no evidence of a *slow* decay after the intervention ended as predicted by Sherman's (1990) concept of residual deterrence decay. Again, the temperature effect retained statistical significance.

SPATIAL DISPLACEMENT IN POSTTREATMENT PERIOD

The second focus of this study was to learn whether, if the impact of the experiment decayed after the officers were removed from their beats, previously displaced offenders could be returning to the targeted foot beats causing inverse displacement. To examine whether this is a possibility, an inverse displacement quotient (IDQ) was developed. The WDQ of Bowers and Johnson (2003) is borrowed; yet the algorithm is modified to examine the posttreatment impact of crime displacement. It should be noted that the IDQ is only useful if an intervention 1) reduced crime in targeted locations and 2) resulted in spatial displacement. Readers unfamiliar with the WDQ may consult Bowers and Johnson (2003). An abridged version of the mathematical logic behind the WDQ also is provided in appendix A.

INVERSE DISPLACEMENT QUOTIENT

The IDQ was developed to estimate whether *displacement decay*, *treatment decay*, or *inverse displacement* has occurred. Consider three separate areas as depicted in figure 1: a treatment location denoted (a), a buffer displacement location denoted (b), and a separate control area denoted (c). If treatment decay has occurred, then an *increase* in crime in target areas (a) relative to control locations (c) would be observed during a posttreatment period (t_2). If displacement decay has occurred, then a *decrease* in crime in the buffer zones (b) relative to the control areas (c) would be observed during a posttreatment period (t_2). If inverse displacement has occurred, then a *simultaneous* decrease in the buffer zones (b) relative to the control areas (c) and increase in the target areas (a) relative to the control areas (c) during this posttreatment period (t_2) would be observed. Table 3 depicts

Figure 1. Hypothetical Target Area (a), Buffer Zone (b), and Control Area (c)

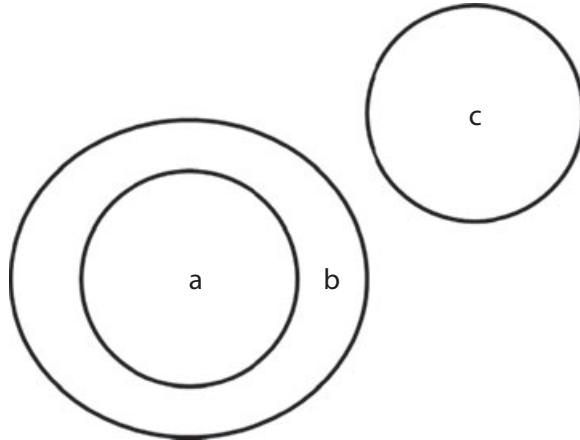


Table 3. Expected Crime Direction for Displacement, Treatment Decay, Displacement Decay, and Inverse Displacement

Outcome	Treatment Relative to Control	Buffer Relative to Control
Displacement	↓	↑
Treatment decay	↑	No significant change
Displacement decay	No significant change	↓
Inverse displacement	↑	↓

the expected outcome in buffer and treatment locations relative to controls in the event of spatial displacement, treatment decay, displacement decay, and inverse displacement.

The first step involves calculating a *displacement decay measure*. The proportion of crime that occurred in the buffer area (b) relative to the control area (c) during the treatment period (t_1) is subtracted from the proportion of crime in the buffer area (b) relative to the control area (c) that occurred during a posttreatment period (t_2). The displacement decay measure is expressed as

$$b_{t_2}/c_{t_2} - b_{t_1}/c_{t_1}$$

If the displacement decay measure is negative, then displacement is decaying in the buffer zones over t_2 . A positive displacement decay measure indicates that crime continued to increase in displacement locations

after an intervention relative to control locations. A positive displacement decay measure may represent a lagged displacement effect (Bowers and Johnson, 2003) or that spatial displacement is a longer term rather than a shorter term side effect of hot spots policing. Continuing to calculate the IDQ would be inappropriate if displacement had not decayed during t_2 . For inverse displacement to occur, crime would theoretically increase in the treatment locations and simultaneously decrease within displacement locations. It would be theoretically illogical to continue with the analysis if displacement did not decay over t_2 .

Next, a *treatment decay measure* is calculated. This measure uncovers the long-term impact of the treatment that was applied during an intervention. The amount of crime occurring within target area (a) during the treatment period (t_1) relative to within the control location (c) is subtracted from the amount of crime occurring in the targets area (a) during the posttreatment period (t_2) relative to the amount of crime occurring within control area (c). The treatment decay measure is expressed as

$$a_{t_2}/c_{t_2} - a_{t_1}/c_{t_1}$$

A positive treatment decay measure suggests that the effect of the treatment was decaying in the posttreatment period. A negative treatment decay measure suggests that the effect of the treatment applied was sustained (or increased) in the posttreatment period. Continuing to calculate the IDQ would be appropriate regardless of the measures direction.¹¹ Following the calculation of the treatment and displacement decay measures, one executes the IDQ by weighting the treatment decay measure by the displacement decay measure, expressed as

$$IDQ = \frac{a_{t_2}/c_{t_2} - a_{t_1}/c_{t_1}}{b_{t_2}/c_{t_2} - b_{t_1}/c_{t_1}} = \frac{\text{treatment decay}}{\text{displacement decay}}$$

Interpretations of the IDQ are presented in table 4.

INVERSE DISPLACEMENT QUOTIENT RESULTS

When the values for each of the locations for 3-month treatment periods and 3-month posttreatment periods are entered into the IDQ equation, a treatment decay measure of .052 is returned, reiterating the decaying

11. Because a negative treatment decay measure indicates the treatment effect was sustained, it would be more logical and useful to calculate a weighted displacement quotient.

Table 4. Interpretation of Inverse Displacement Quotient

IDQ Value		Interpretation
IDQ > 0	Postintervention crime reduction > displacement decay	No inverse displacement
IDQ = 0	No treatment decay	
0 > IDQ > -1	Treatment decay < displacement decay	Inverse displacement
IDQ near -1	Treatment decay = to displacement decay	
IDQ < -1	Treatment decay > than displacement decay	

posttreatment effect reported previously.

$$\text{IDQ} = \frac{307/308 - 306/324}{316/308 - 371/324} = \frac{.052}{-.119} = -.43$$

Calculation of the displacement decay measure returns a value of $-.119$. This negative value suggests that in the 3 months after the foot patrols, crime was decreasing in the buffer zones relative to controls. With a decline in crime in the buffer zones confirmed, continuing with the calculation of the IDQ is justified. An IDQ value of $-.437$ is returned, suggesting that inverse displacement may have occurred postexperiment, and yet the amount returning to the treatment zones was less than had decayed in the buffer zones. During the 3 months after treatment, crime in the target areas stayed relatively stable with an increase by less than 1 percent, whereas the control location crime counts decreased by approximately 5 percent. In the buffer zones, crime declined by nearly 15 percent.

DISCUSSION

TREATMENT EFFECTS

The MGCM uncovered statistically significantly less violent crime in the treatment areas, by approximately 16 percent for phase 1 and 20 percent for phase 2, relative to the controls during the treatment periods. Thus, the findings augment the evidence base supporting hot spots policing and reiterate that spatially focused foot patrol in hot spots can reduce violent crime. Consistent with Sherman's (1990) theory of initial deterrence decay, these deterrent effects were "slowing down" during the experimental period for the phase 1 beats. Following Sherman's theoretical logic, offenders may have determined that they overestimated the risk of apprehension at the onset of the experiment. As time went on, it seems that offending increased as a result. Staffing the phase 1 hot spots with foot patrols consistently for 22 weeks seems to have been somewhat inefficient. Although reductions in crime were achieved, the phase 1 beats, which were staffed

for 10 more weeks than the phase 2 beats, were less effective overall and exhibited evidence of initial deterrence decay, whereas the phase 2 beats did not. The findings support Sherman's (1990) suggestion that crackdowns should be short term.

POSTTREATMENT EFFECTS

These results point out that foot patrol in hot spots is susceptible to the critique of Rosenbaum (2006), who had concerns that the effects of crackdowns are short term and decay rapidly. Three months after the experiment, the statistically significant differences in violent crime between the experimental and control areas could no longer be detected. The results are in line with the design of the treatment and consistent with deterrence theory. Because a mechanism is required to promote the certainty of detection, these findings confirm that a shortcoming of crackdowns is that they are only beneficial while they are in effect. In Philadelphia, once the "certainty communicating device" was removed, no differences between the treatment and control locations were detectable.

DISPLACEMENT DECAY AND INVERSE DISPLACEMENT

The results suggest that spatial crime displacement was a short-term outcome in Philadelphia. During the 3 months after the experiment, crime remained relatively stable in the target beats, whereas controls declined by 5 percent. Violent crime declined by nearly 15 percent in the buffer zones. This pattern of changes is consistent with a scenario in which inverse displacement was occurring. It is possible that a portion of the treatment decay uncovered could be a result of displaced offenders realizing that the crackdown ended and that it was again safe to offend in the target areas. Also, it is possible that the crime decline during the experiment caused people to spend more time outdoors; once the officers left, it is conceivable that these individuals were victimized more often. Therefore, our conclusions are only speculative. To verify or discredit such speculation would require further analysis, as offender and victim behavior were not monitored.

In light of opportunity theory, this speculation seems plausible. Several studies have demonstrated the long-term stability of crime at place (Spelman, 1995; Taylor, 2001; Weisburd et al., 2004). Considering this stability, it makes sense that displaced offenders would return to their ideal offending locations after a crackdown. If offenders are "tightly coupled" (Weisburd and Telep, 2012) to their ideal offending locations and these locations are surrounded by blocks with fewer opportunities for crime (Groff, Weisburd, and Yang, 2010), this helps explain the marginal impact of displacement. It also supports the proposition that displaced offenders will

return to their original offending sites as predicted by the theory of inverse displacement.

POLICY IMPLICATIONS

CRACKDOWN DEPLOYMENTS

If the Durlauf and Nagin (2011) proposal was to come to fruition and police budgets were supplemented with additional funds, then should these resources be used to organize foot patrols and similar crackdown type deployments? Their shortcomings notwithstanding, even critics concede that crackdowns deserve a place in the “the arsenal of urban policing ... and [are] essential for providing short-term relief of distressed areas” (Rosenbaum, 2006: 258). These tactics will be beneficial at certain times and places, even if only in the short term. For example, the significant seasonal effects found in this study suggest that crackdowns might be useful in addressing seasonal spikes in crime.

Sherman (1990) argued that because of initial deterrence decay, crackdown initiatives may be most effective if they are limited in duration and randomly rotated across target areas to avoid offender adaptation. This study offers a degree of support for Sherman’s suggestion. If foot patrols are to be deployed solely as “certainty communicating devices,” then these findings suggest that longer is not necessarily better. Although it is premature to prescribe a specific length of deployment time, what can be gleaned is that staffing hot spots 5 days a week for 16 hours a day over a 3-month period did not result in initial deterrence decay, whereas the same dosage over 22 weeks did. Whether the effects uncovered would have been equivalent if officers were randomly rotated across hot spots as advocated by Sherman (1990) is unclear, but police may not need to be continually present to be effective. Koper (1995: 668), for example, cautiously suggested that the optimal time to spend at a hot spot to reduce disorder was 14 to 15 minutes; after this point, his data indicated that initial deterrence begins to decay. These “dosage” questions are important for future research to address.

POSSIBILITY OF BACKFIRE

Critics charge that crackdowns could result in several “backfire effects” (Weisburd et al., 2011) that may impede attaining the ends of the Durlauf and Nagin (2011) proposal. Although future research must explore whether backfire effects are inevitable, “it seems likely that overly aggressive and indiscriminant police crackdowns would produce some undesirable effects” (Braga and Weisburd, 2010: 188). These effects may include the following.

Deterrence Decay and Inverse Displacement. Although results suggest that displacement was short term, police officials must be cognizant of the possibility that displacement and inverse displacement may occur and must consider how these phenomenon may negatively impact police–community relations and perceptions of police legitimacy. The extent to which adjacent communities experience the effects of displacement could raise community concerns over the inequitable allocation of police resources. Any amount of displacement runs the risk of causing tension between the police and these communities, and the reductions achieved by foot patrols or other crackdown deployments are unlikely to impress communities experiencing displacement, however short term it may be. Likewise, if target locations suffer residual deterrence decay and inverse displacement, then perceptions of police legitimacy might decline if residents view police as providing only short-term relief to long-term crime problems.

Overreliance on Sanctions. Police are particularly adept at carrying out traditional responses such as making arrests or *Terry* stops. These tactics are fundamental components of crackdowns and likely contributed to the treatment effect. It is inevitable that the number of sanctions administered will increase during crackdowns, and yet an overreliance on these tactics could strain police–community relations, decrease police legitimacy, and increase resentment of police (Braga and Weisburd, 2010: 188). These outcomes have implications for the police to reduce crime in the long term, as the police need the support and cooperation of the public to combat crime effectively (Braga and Weisburd, 2010; Tyler, 2004; Weisburd et al., 2011). An overreliance on crackdowns, especially those involving increased use of sanctions, may backfire in the long run.

Impediments to Reducing Incarceration. The goal of the Durlauf and Nagin (2011) proposal is to reduce crime *and* incarceration, and yet an overreliance on crackdowns might impede attaining the latter. As noted, foot patrols were responsible for a 13 percent increase in arrests in the target areas. Heavily relying on these tactics has at least two implications. It is reasonable to assume that increases in arrests will result in increases in prosecutions and ultimately in increases in custodial sentences.¹² Even with policies to limit sentence lengths, Goldkamp (2011) suggested that this could result in a sustained use of imprisonment, yet the confined population would more rapidly turn over. Better funded crackdowns might increase the efficiency

12. However, Goldkamp (2011) also noted that an increase in the number of cases resulting in dismissals could result, which suggests that alternatively, custodial sentences would not increase. If this were the case, then this factor might dilute the effectiveness of certainty-communicating policies.

with which offenders are arrested, and yet they would do comparatively less to reduce the extent of incarceration.

System Side Effects. A sudden spike in arrests may temporarily overwhelm court systems and, as a consequence, increases fugitive populations (Goldkamp and Vilcica, 2008). Court systems become unable to process the increased workload, detention, and supervision resources fail and greater numbers of offenders live “on the run” (Goldkamp, 2011: 119). Increasing the efficiency for detecting criminal behavior yet not following through with punishment in a timely manner could dilute the power of certain detection. This problem is confounded if growing populations of fugitives demonstrate that it is possible simply to bypass the criminal justice process after arrest. Although the role of celerity in deterring is largely overlooked by Durlauf and Nagin (2011), it could become pertinent if crackdowns are overused.

MOVING BEYOND CRACKDOWNS

More holistic policing strategies are almost certainly necessary if Durlauf and Nagin’s (2011) proposal is to succeed, even if crackdowns can be deployed with a great degree of efficiency. Most police agencies allocate patrol resources disproportionately at high-crime places, so it is questionable whether better funded crackdowns will elicit the aggregate crime reductions predicted (Baumer, 2011). The failure to sustain the treatment effects in this analysis suggests that there is “the need for a more complete understanding of criminogenic forces at work in hot spots” (Rosenbaum, 2006: 246–7) and a need to move away from responses that are “narrow and predictable” (Rosenbaum, 2006: 249). Unfortunately, operational barriers impede the implementation of innovative policing tactics frequently. Problem-oriented policing evaluations, for example, often report the implementation of “shallow” responses (Braga and Bond, 2008: 578). As a result, real-world problem-oriented policing often falls short of its true rhetoric and more closely resembles traditional policing (Braga and Weisburd, 2010; Bullock, Erol, and Tilley, 2006).

However, crackdowns and other traditional responses ignore the role of many other factors that contribute to crime, such as social disorganization, offender reentry, or the physical environment (Rosenbaum, 2006). Whether foot patrols can incorporate other innovative strategies will likely dictate whether the short-term benefits crackdown-style foot patrols produce can be extended or translated into aggregate and long-term crime reductions. Foot patrols as a specific policing tactic seem to fit nicely into a variety of promising policing paradigms.

Problem-Oriented Policing. Problem-oriented policing involves the analysis of crime problems, understanding why they continue and

implementing responses tailored to the problem, with the ultimate goal of problem reduction (Eck, 2006: 118). Its “normative principle” is that police should reduce problems, not respond to incidents (p. 119). In analyzing the qualitative data collected during the Philadelphia Foot Patrol Experiment, Wood et al. (in press) report that being on foot provided officers the opportunity to deepen their understanding of the hot spots and to build relationships with community members, business owners, and local political officials. It contributed to their understanding of who did and did not belong in the beats, and it allowed them to identify and regulate the behavior of perceived offenders while collecting intelligence from cooperative residents. They reported becoming aware of problem locations and the role that the physical environment played in fostering environments conducive to crime. These are essential components of the *scanning* phase of problem-oriented policing, and may add nuance to the *analysis* of crime problems (Eck and Spelman, 1987). Foot patrol could be useful in understanding and responding to problems in hot spots, and it might be a valuable tool during such initiatives. If officers were deployed on foot in hot spots for a relatively short period, then they may not only exert a deterrent effect but also allow for gathering information pertinent to implementing true problem-oriented responses rather than shallow ones.

Community Policing. Foot patrol is already a popular community policing tactic (Skogan, 2006). Although community policing is an overall organizational strategy, concepts of community policing could prove beneficial if incorporated into hot spots policing programs. For example, rather than relying exclusively on crackdowns, Taylor (2006: 109) advocated for a community “co-production model.” Likewise, Braga and Weisburd (2010: 203) stressed the need for a “solid commitment to the community policing philosophy” before employing aggressive hot spot techniques. It may be wise to heed this advice.

If communities are properly engaged in hot spots programs, then it is likely that perceptions of police legitimacy can be enhanced (Braga and Weisburd, 2010). Community policing has been shown to reduce fear of crime and result in a more positive police–community relationship (Weisburd and Eck, 2004). When citizens perceive police as legitimate, they are more likely to cooperate with police and obey the law (Tyler, 1990). Engaging with the community could help to rebuild the “social and organization fabric of neighborhoods” and “enable residents to contribute to maintaining order in their communities” (Braga and Weisburd, 2010: 204). Foot patrol has long been considered a “proactive, non-threatening, community-oriented approach to local policing” (Wakefield, 2007: 343) and is particularly amenable to community outreach, as officers are visible, engaged, and accessible.

Intelligence-Led Policing. Intelligence-led policing is also a management philosophy. This model uses data analysis and criminal intelligence to direct police resources and focus enforcement activities on serious and prolific offenders (Ratcliffe, 2008: 87). The findings of possible inverse displacement in this study, combined with a general understanding that a minority of offenders are responsible for a majority of crime (Wolfgang, Figlio, and Sellin, 1987), suggests that a focus on prolific offenders that is refined by focusing at hot spots may be beneficial. This focus would require police to acquire knowledge of prolific offenders operating within hot spots through data analysis and intelligence gathering. Wood et al. (in press) report that the officers involved in the Philadelphia Foot Patrol Experiment reported developing knowledge of who the prolific offenders operating within their beats were, and likely did so using different avenues than their vehicle-bound colleagues (Groff et al., in press). This information was learned over time and was sometimes relayed to the officers by community members. Using foot patrols as a mechanism to gather intelligence and direct enforcement at prolific offenders could be a beneficial addition to an intelligence-led policing model.

CONCLUSION

Hot spot policing as a strategy for reducing crime and disorder has been growing in popularity among scholars and practitioners. A recent policy proposal called for the funneling of funds from corrections to policing budgets to reduce crime, incarceration, and correctional spending simultaneously. Hot spots policing techniques were included in this discussion. Although many evaluations report on the successes of hot spots patrols, there is concern about the overreliance of crackdowns and more traditional police responses during hot spots initiatives that rely solely on deterrence because they elicit crime reductions that are only short term and decay rapidly.

Although foot patrols and crackdowns more generally seem to be useful as a short-term deterrent to violent crime if deployed in hot spots, more holistic strategies are likely needed if the hypothesized ends of the Durlauf and Nagin (2011) proposal are to come to fruition. Foot patrol as a specific tactic seems to fit into several more holistic strategies, such as problem-oriented policing, community policing, and intelligence-led policing. Some suggestions for incorporating foot patrols into these policing paradigms were presented, although future research will have to evaluate whether these deployment suggestions can extend the benefits of crackdowns using foot patrols or elicit greater and more long-term crime reductions.

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APPENDIX A

The WDQ is a method for evaluating whether a spatially focused crime intervention was successful and whether displacement or a diffusion of benefits occurred. Consider again the three separate areas as depicted in figure 1: a treatment location denoted (a), a buffer displacement location denoted (b), and a separate control area denoted (c). If geographic displacement were to occur, then crime in (a) would decrease and crime in (b) would increase relative to (c) during the treatment period. The WDQ provides estimates of displacement or diffusion by calculating relative changes in crime from before to during an intervention by creating treatment success and buffer displacement measures. The treatment success measure is calculated by dividing crime occurring in a treatment location (a) by crime occurring in a control location (c) for a time period during an intervention (t_1). This quotient is then subtracted from the quotient returned by dividing the amount of crime that occurred in a treatment location (a) by crime that occurred in control (c) areas before the treatment period (t_0). The treatment success measure is expressed as

$$at_1/ct_1 - at_0/ct_0$$

The next step involves calculating a buffer displacement measure. Crime occurring in the buffer locations (b) during an intervention is divided by crime occurring in a control location (c) before a treatment (t_0); this quotient is then subtracted from the quotient returned by dividing crime in the buffer location by crime in the control location during the treatment period (t_1). The buffer displacement measure is expressed as

$$bt_1/ct_1 - bt_0/ct_0$$

The buffer displacement measure is then weighted by the treatment success measure expressed mathematically as

$$\text{WDQ} = \frac{bt_1/ct_1 - bt_0/ct_0}{at_1/ct_1 - at_0/ct_0} = \frac{\text{buffer displacement}}{\text{treatment success}}$$

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

