Foot Patrol in Violent Crime Hot Spots: The Longitudinal Impacts of Deterrence and Post-Treatment Effects of Displacement

Final draft of:


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 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. Multi-level growth curve models revealed that beats staffed for 22 weeks had a decaying deterrent effect during the course 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 three 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of Durlauf and Nagin’s (2011) recent proposal that prison sentences should be shortened, mandatory minimum statutes repealed, and the cost savings generated by these policy changes shifted into policing budgets in order to more effectively convey the certainty of detection. It is concluded that if the Durlauf and Nagin proposal is to succeed, more holistic policing strategies would likely be necessary. Foot patrol as a specific policing tactic appears to fit nicely into a variety of policing paradigms, and suggestions for incorporating them to move beyond strictly enforcement-based responses are presented.

Introduction

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) argue 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) do not make general recommendations about the tactics police should employ in hot spots if their policy proposal were to come to fruition, and they call for further 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 are
oftentimes met with concern. Much of this concern centers on the short-term crime reductions resulting from traditional police responses such as crackdowns\(^2\), 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) highlight the weaknesses of deterrence-based policies which 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 that Durlauf and Nagin (2011) predict. 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, p. 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.

### The Durlauf and Nagin Proposal

A recent issue of Criminology and Public Policy (Volume 10, Issue 1) debated a policy proposal articulated by Durlauf and Nagin (2011). They posit that if policymakers rethink the ways in which the criminal justice system fosters deterrence 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 simultaneously be reduced. Their appraisal of the deterrence literature led them to conclude that: (1) increasing already excessive prison sentence will have, at best, a marginal deterrent effect and (2) increasing police visibility by hiring more officers and organizing deployments in ways that increase the risk of apprehension appears to have “substantial marginal deterrent effects” (Durlauf and Nagin, 2011: 14). They advocate for reductions in prison sentence lengths, the repeal of mandatory minimum statutes and a broad move away from policies which emphasize imposing severe punishments. They suggest 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 between certainty, severity and celerity. Although punishment is a requisite to deter, Durlauf and Nagin (2011) conclude 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 believe could be carried out with few, if any, negative consequences. Durlauf and Nagin (2011) point out the weakness in the imprisonment and crime literature in making their case.

### The 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. A number 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 are unable to control for other variable influencing crime and incarceration. This relationship may also 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 dataset have been reported. As a whole, Durlauf and Nagin (2011) conclude that this literature provides little convincing evidence that housing greater numbers of inmates for lengthier periods has done much to deter crime.

Durlauf and Nagin (2011) also review 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 strike-eligible convictions has received considerable attention. Results are heterogeneous. Stolzenberg and D’Alessio (1997) conclude that in 9 of 10 California jurisdictions, the implementation of three strikes laws did not reduce serious crime. In contrast, Zimring, Hawkins and Kamins (2001) conclude that the law reduced felonies by 2 percent. However, only those with two previous strike-eligible convictions appear to have been deterred. Helland and Tabarrok (2007) also found that offending was lower for those convicted of two strike-eligible offenses, and estimate that the law deters 31,000 crimes per year. They also estimate that the cost of incarcerating third strike offenders is about 4.6 billion dollars, or about 150,000 dollars 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 about 1 million crimes, a far greater cost-benefit (Donohue, 2005; Helland and Tabarrok, 2007). Durlauf and Nagin (2011) conclude that the costs of laws mandating broad increases in sentence lengths far outweigh the deterrent effects.

Durlauf and Nagin (2011) review several other evaluations of policies intended to deter crime through increasing punishment severity, such as sentence enhancements for crimes involving firearms (Loftin and McDowell, 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 deduce that the crime reduction benefits of increasing punishment severity are modest at best. As a result, Durlauf and Nagin (2011: 31) conclude that, “the marginal deterrent value of increased sentence length at current levels is small for contexts in which sentences are currently long.”

**The Deterrent Effects of Hot Spots Policing**

Empirical support for the deterrent effects of increasing the certainty of detection appears 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 about 50 percent lower in the targeted areas compared to the control. A number of evaluations followed.

Sherman and colleagues (1995) found that raids on crack-houses reduced violent and property crime by 24 and 3 percent, respectively. Likewise, Sherman and Rogan (1995) 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 (MO). Another evaluation in Kansas City found that drug markets
targeted with crackdowns had significantly fewer disorder calls for service during the 7 months following the intervention compared to controls (Weisburd and Green, 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 (NJ), 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%), robbery (41.8%), burglary (35.5%), and disorderly/nuisance behaviors (14%) (Braga and Bond, 2008). In sum, both problem-oriented policing and targeted enforcement appear 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 Spots Crackdowns

Rosenbaum (2006) outlines a number of critiques of hot spot policing, particularly relevant to deployments involving crackdowns (see also Kochel, 2011). Two of these critiques are addressed by the present 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 (Repetto, 1976).

INITIAL AND RESIDUAL DETERRENCE DECAY

Empirical evidence does suggest that crackdowns do not have lasting effects. For example, Sherman et al. (1995) 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 notes, crackdowns work through changing offender’s perceived risk of being caught through heightened policing, but crackdowns are rarely indefinite. Therefore, since the underlying causes of crime are not typically addressed, once a crackdown ends offenders will begin to reoffend. Theoretically, it makes sense that their benefits will largely be seen when they are in effect.

There is also evidence that the effectiveness of crackdowns decline while they are ongoing, or result in “initial deterrence decay” (Sherman, 1990:10). In reviewing several case studies of crackdowns, Sherman (1990) notes that in some cases crime reductions began to decay while crackdowns were still underway. 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 proposal. If crackdowns do not have lasting effects, and these effects decline while crackdowns are underway, it seems unlikely that these and other targeted enforcement techniques are capable of eliciting aggregate and lasting crime reductions. However, Sherman (1990) does 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 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, there is a benefit, albeit a decaying one, after a short-term crackdown. Although Sherman (1990) reviews a number of case studies where 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) argues that geographically concentrating police resource 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) notes 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 which disperse crime, there should be concern over pushing crime into locations where it did not previously exist. 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 or not a community will notice a crime increase resulting from displacement is not clear. Since 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 above. 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 evaluated whether displacement was a long- or short-term outcome.

**The 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 upon 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 post-operation.
THE PHILADELPHIA FOOT PATROL EXPERIMENT

Until the Philadelphia Foot Patrol Experiment, foot patrols were considered capable of improving community relations and 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 collaboration between Temple University researchers and the Philadelphia Police Department to measure the deterrent effects of foot patrols in micro-level 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 upon their graduation from the police academy. They patrolled in pairs on a day (10am-6pm) and night (6pm-2am) shift, five days a week (Tuesday-Saturday). They were deployed in two phases coinciding with academy graduation dates. Phase one deployed 31 March 2009 and terminated 31 August 2009 and phase two deployed 7 July 2009 and terminated 28 September 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 appears 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 three years prior to the experiment (2006-2009). 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 = 0.5; 2006 = 0.25). Incidents were aggregated to a set of Thiessen polygons centered on street intersections (see Chrisman, 2002). A local Moran’s 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 decisionmaking (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 utilized; 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 overlapped with other beats; this process yielded 124 potential foot beats. Since 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. 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 literature (Braga et al., 1999; Braga and Bond, 2008; Weisburd and Green, 1995; Weisburd et al., 2006). Contextual knowledge was then incorporated
into their 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. Since some of the beats were in close proximity to one another, some buffer zones were combined. In total, ten 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 pre-treatment 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 utilized to measure spatial displacement (Bower 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 (WDQ= 0.41). Calculation of the total net effect (Clarke and Eck, 2005) indicated that there were 90 fewer crimes in the treatment locations relative to controls, this being offset by 37 crimes displaced, for a total net effect of 53 fewer crimes in treatment locations.

Two tactical elements were theorized to have elicited the crime reductions: “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, p. 819) suggest 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 to three months prior to the experiment. 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 pre- to during-operation. Ratcliffe et al. (2011) suggest 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 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. Since the officers were new, it is possible that they were especially motivated to reduce crime and be proactive, and this 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 which 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.

It is also worth noting that the qualitative observations conducted during the experiment suggested that some of the officers also worked on building relationships with the community (see Wood et al., in press). They commented that these relationships resulted in useful intelligence which gave them a better sense of the problems afflicting the neighborhoods and who prolific offenders were. Whether or not these efforts contributed to the crime reductions achieved during the experiment is difficult to say, but it cannot be ruled out as a possibility. This topic is addressed more extensively in the policy implications discussion below.
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. Since these research questions and experimental design have a nested data structure—changes in violent crime over time (level-one) nested within treatment and control areas (level-two)—multilevel growth curve models (MGCM) are employed. This technique can test hypotheses about the time varying effects of experimental treatments on growth curves at level-one while controlling for temporally stable differences across experimental areas at level-two (for a recent example, see Corsaro, Brunson, and McGarrell, 2009). More technically, MGCM provides unbiased parameter estimates for nested data structures (Bryk and Raudenbush, 1986).

This technique is advantageous here for a number of reasons. With MGCM the intercept represents the expected value of the outcome (here, violent crime) averaged across level-two units (here, experimental beats), at the start of a time series (here, the first treatment time block). Differences across level-two units which affect the outcome are accounted for by predicting the intercept with control variables at level-two. This is important because it is necessary to control for pre-treatment levels of violence across the beats in order to remove variability in the dependent variable due to blocking (Shadish, Cook and Campbell, 2002: 51). The models outlined below control for these differences by controlling for pre-treatment counts of crime at level-two. In addition, the analysis of post-treatment effects requires controlling for levels of crime at the beginning of the post-treatment period to remove variability in the dependent variable after the treatment was administered. The models outlined below 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-two variable (discussed further below). Finally, this technique can model the conditional deterrent effect that Ratcliffe et al. (2011) found was predicated on pre-treatment violence. In multilevel growth curve modeling, time-varying covariates at level-one may be treated as fixed or their slopes specified as random, allowing their effects to be predicted by level-two variables to estimate between-unit differences in the outcome. The treatment variables discussed below can be specified as random and then predicted by pre-treatment crime counts to estimate this interaction.

To assess the effects of the Philadelphia Foot Patrol Experiment over time and upon its conclusion, two-level growth curve models with bi-weekly 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 post-treatment effects and finally, (4) a model estimating residual deterrence decay. All models are specified as Poisson distributions with over dispersion (Raudenbush and Bryk, 2002). Differences in geographic size across the experimental areas are controlled by introducing an exposure variable of geographic area (sq. ft.). All variables are entered un-centered.

OUTCOME

The dependent variable is violent crime counts in each of the treatment and control hot spots aggregated to two week time periods. The same violent crime incident categories utilized during the foot patrol experiment makeup 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 bi-weekly time blocks. For all models the first bi-weekly time period begins approximately one year (1 April 2008) prior to the deployment of the phase one foot patrols (deployed 31 March 2009). Since the four models answer distinct research questions, the end dates differ across the models (discussed below).

**MODELS**

Level-one variables are time-varying covariates. Each model includes a linear and quadratic time variable accounting for the position of the bi-weekly data point in the time series at level-one. Due to 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 bi-weekly block is entered in all models at level-one. 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 above, the phase one foot patrols were implemented for 22 weeks (11 bi-weekly treatment periods) and the phase two foot patrols for 12 weeks (6 bi-weekly treatment periods), so the treatment and post-treatment variables discussed below are separated by phase. Descriptive statistics are reported in table 1.

**Table 1. Descriptive Statistics**

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</tbody>
</table>

**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 above, the first bi-weekly time block begins on 1 April 2008. The final bi-weekly block ends on 28 September 2009, the final day of the phase two foot patrols. At level-one, a dichotomous variable is entered to examine whether there are significant differences in expected violent crime counts in target areas relative to controls during the experiment. Phase one beats were coded “1” for all time blocks beginning 31 March 2009 and ending 31 August 2009. Phase two beats were coded “1” for all time blocks beginning 7 July 2009 and ending 28 September 2009. Both treatment and control areas were coded “0” for time blocks preceding and
during the experiment. At level-two, the total number of violent crime incidents occurring during the three months before the experiment is entered as a control variable. In order to estimate the interaction between pre-treatment violence and the effects of treatment, the phase one and phase two treatment variables’ slopes are specified as random and predicted by the level of violence during the three months prior to treatment at level-two.

Initial deterrence decay. The second model employs a linear decay function to measure initial deterrence decay during the treatment period. Again, the first bi-weekly block starts on 1 April 2008 and the data sequence ends on 28 September 2009. For the phase one “initial deterrence decay” variable, the first bi-weekly data point during the treatment period is coded “11”, with each subsequent time block decreasing linearly to “1” for the last two weeks of the phase one treatment period. The phase two “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. All other time blocks for the target beats are coded as “0”. All control beats are assigned values of “0” for each of the time blocks.

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

At level-two the pre-treatment 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 above, which measures the experimental impacts of foot patrol during the treatment period only. As part of model estimation, HLM 6.06 software produces a residual file for both level-one and level-two. After executing the model estimating treatment effects the residual values during the final treatment period were retrieved from the level-one residual file. This variable was included in the data file of this post-treatment model to control for across-beat differences at the start of the post-treatment period. These residual values are the discrepancies between the fitted values, or the predicted score based on the specified model, and the observed values, or the actual count of violent crime during that bi-weekly period (Raudenbush, Bryk, Cheong, Congdon and du Toit, 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 these differences at level-two 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 post-treatment 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 one and phase two beats, the first post-treatment time block for the target areas is coded “6” and the last time block is
coded “1” for the three months post treatment period. All other bi-monthly data points are coded “0” and control beats are assigned “0” values for all data points. The residual values remain at level-two.

RESULTS

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-one and pre-treatment violence at level-two, foot patrol, during both phases one and two, had significantly lower expected violent crime counts than controls; an average of about 16 and 20 percent respectively. The outcome is also linked to temperature, where a two degree increase in average temperature results in about a one percent increase in expected violent crime counts. Beats with higher levels of pre-treatment violence had expected violent crime counts that were about 5.5 percent higher during the treatment period. When the phase one and phase two treatment slopes are specified as random neither phase one nor phase two 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 interaction11.

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 one reaches statistical significance whereas the phase-two coefficient does not, suggesting a declining treatment effect during the experiment for phase one only. During the first treatment period of phase one (coded here as “11”), the target beats had crime counts that were about 22 percent lower than the control beats [0.02(11) = 22], but during the final two weeks of treatment, the phase one beats had expected crime counts which were only 2 percent lower than the control areas. Again, a two degree increase in temperature results in about a one percent increase in expected violent crime counts.

The post-treatment effects column reports the impacts of the foot patrols after the experimental period net of controls. For both the phase one and phase two 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 post-treatment period. As with the post-treatment variables, the residual deterrence decay variables did not reach statistical significance when all other variables were held constant. This suggests that there were no significant difference between the treatment and control areas on levels of violence from the beginning to the end of the post-treatment period. This model finds no evidence of a slow decay after the intervention ended as predicted by Sherman’s concept of residual deterrence decay. Again, the temperature effect retained statistical significance.
Table 2. Multilevel Growth Curve Modeling Results

<table>
<thead>
<tr>
<th>Fixed-Effect</th>
<th>Experimental Effects</th>
<th>Initial Deterrence Decay</th>
<th>Post-Treatment Effects</th>
<th>Residual Deterrence Decay</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ERR (SE)</td>
<td>ERR (SE)</td>
<td>ERR (SE)</td>
<td>ERR (SE)</td>
</tr>
<tr>
<td>Level-One</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Time</td>
<td>.999 (.009)</td>
<td>.998 (.009)</td>
<td>.995 (.009)</td>
<td>.995 (.003)</td>
</tr>
<tr>
<td>Time²</td>
<td>.999 (.001)</td>
<td>.999 (.001)</td>
<td>.999 (.001)</td>
<td>.999 (.001)</td>
</tr>
<tr>
<td>Temperature</td>
<td>1.005** (.001)</td>
<td>1.005** (.001)</td>
<td>1.004** (.001)</td>
<td>1.005** (.001)</td>
</tr>
<tr>
<td>Phase 1</td>
<td>.842** (.056)</td>
<td>.980** (.056)</td>
<td>1.062 (.079)</td>
<td>1.017 (.017)</td>
</tr>
<tr>
<td>Phase 2</td>
<td>.801** (.086)</td>
<td>.955 (.086)</td>
<td>.887 (.073)</td>
<td>.968 (.017)</td>
</tr>
<tr>
<td>Intercept</td>
<td>13.896** (.111)</td>
<td>14.436** (.111)</td>
<td>29.718** (.111)</td>
<td>29.696** (.099)</td>
</tr>
<tr>
<td>Level-Two</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre VC</td>
<td>1.005 (.009)</td>
<td>1.005 (.009)</td>
<td>1.988 (.029)</td>
<td>.988 (.029)</td>
</tr>
<tr>
<td>Residual</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Random Effect</td>
<td>χ²</td>
<td>Variance</td>
<td>χ²</td>
<td>Variance</td>
</tr>
<tr>
<td>Intercept</td>
<td>1009.785** .149</td>
<td>1011.080** .150</td>
<td>1769.015** .248</td>
<td>1769.878** .248</td>
</tr>
</tbody>
</table>
**SPATIAL DISPLACEMENT IN POST-TREATMENT 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. In order to examine whether this is a possibility, an inverse displacement quotient (IDQ) was developed. The weighted displacement quotient (WDQ) of Bowers and Johnson (2003) is borrowed, yet the algorithm is modified in order to examine the post-treatment impact of crime displacement. It should be noted that the IDQ is only useful if an intervention (a) reduced crime in targeted locations, and (b) 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 is also provided in Appendix I.

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

---

**THE 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 an *increase* in crime in target areas (a) relative to control locations (c) would be observed during a post-treatment period ($t_2$). If displacement decay has occurred, a *decrease* in crime in the buffer zones (b) relative to the control areas (c) would be observed during a post-treatment period ($t_2$). If inverse displacement has occurred, 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 post-treatment period ($t_2$) would be observed. Table 3 depicts the expected outcome in buffer and treatment locations relative to controls in the event of spatial displacement, treatment decay, displacement decay and inverse displacement.
Table 3. Expected Crime Direction for Displacement, Treatment Decay, Displacement Decay and Inverse Displacement.

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Treatment Relative to Control</th>
<th>Buffer Relative to Control</th>
</tr>
</thead>
<tbody>
<tr>
<td>Displacement</td>
<td>↓</td>
<td>↑</td>
</tr>
<tr>
<td>Treatment Decay</td>
<td>↑</td>
<td>No Significant Change</td>
</tr>
<tr>
<td>Displacement Decay</td>
<td>No Significant Change</td>
<td>↓</td>
</tr>
<tr>
<td>Inverse Displacement</td>
<td>↑</td>
<td>↓</td>
</tr>
</tbody>
</table>

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 post-treatment period \( t_2 \). The displacement decay measure is expressed as:

\[
\frac{b_{t_2}}{c_{t_2}} - \frac{b_{t_1}}{c_{t_1}}
\]

If the displacement decay measure is negative, displacement is decaying in the buffer zones over \( t_2 \). A positive displacement decay measure indicates that crime continued to increase in displacement locations following 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 shorter-term side effect of hot spots policing. Continuing to calculate the IDQ would be inappropriate if displacement had not decayed during \( t_2 \). In order 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 post-treatment period \( t_2 \) relative to the amount of crime occurring within control area (c). The treatment decay measure is expressed as:

\[
\frac{a_{t_2}}{c_{t_2}} - \frac{a_{t_1}}{c_{t_1}}
\]

A positive treatment decay measure suggests that the effect of the treatment was decaying in the post-treatment period. A negative treatment decay measure suggests that the effect of the treatment applied was sustained (or increased) in the post-treatment period. Continuing to calculate the IDQ would be appropriate regardless of the measures direction\(^6\). 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:
Interpretations of the IDQ are presented in table 4.

<table>
<thead>
<tr>
<th>IDQ Value</th>
<th>Interpretation</th>
<th>Interpretation</th>
</tr>
</thead>
<tbody>
<tr>
<td>IDQ &gt; 0</td>
<td>Post-Intervention crime reduction &gt; displacement decay</td>
<td>No inverse displacement</td>
</tr>
<tr>
<td>IDQ = 0</td>
<td>No treatment decay</td>
<td></td>
</tr>
<tr>
<td>0 &gt; IDQ &gt; -1</td>
<td>Treatment decay &lt; displacement decay</td>
<td>Inverse displacement</td>
</tr>
<tr>
<td>IDQ near -1</td>
<td>Treatment decay = to displacement decay</td>
<td></td>
</tr>
<tr>
<td>IDQ &lt; -1</td>
<td>Treatment decay &gt; than displacement decay</td>
<td></td>
</tr>
</tbody>
</table>

**Inverse Displacement Quotient Results**

When values for each of the locations for three month treatment periods and three month post treatment periods are entered into the IDQ equation a treatment decay measure of 0.052 is returned, reiterating the decaying post-treatment effect reported above.

\[
IDQ = \frac{at_2/c_t^2-at_1/c_t^1}{bt_2/c_t^2-bt_1/c_t^1} = \frac{Treatment \ Decay}{Displacement \ Decay}
\]

Calculation of the displacement decay measure returns a value of -0.119. This negative value suggests that in the three months following 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 -0.437 is returned suggesting that inverse displacement may have occurred post-experiment, yet the amount returning to the treatment zones was less than had decayed in the buffer zones. During the three months after treatment, crime in the target areas stayed relatively stable with an increase by less than one percent while the control location crime counts decreased by about 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 about 16 percent for phase one and 20 percent for phase two, relative to the controls during the treatment periods. Thus, the findings further 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 theory of initial deterrence decay, these deterrent effects were “slowing down” during the
experimental period for the phase one 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 appears that offending increased as a result. Staffing the phase one hot spots with foot patrols consistently for 22 weeks appears to have been somewhat inefficient. Although reductions in crime were achieved, the phase one beats, which were staffed for 10 more weeks than the phase two beats, were less effective overall and exhibited evidence of initial deterrence decay, whereas the phase two beats did not. The findings support Sherman’s (1990) suggestion that crackdowns should be short-term.

**POST-TREATMENT EFFECTS**

These results point out that foot patrol in hot spots is susceptible to the critique of Rosenbaum (2006), who has 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. Since 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**

Results suggest that spatial crime displacement was a short-term outcome in Philadelphia. During the three months after the experiment, crime remained relatively stable in the target beats while 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 due to displaced offenders realizing that the crackdown ended and that it was again safe to offend in the target areas. It is also 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. A number of studies have demonstrated the long-term stability of crime at place (Weisburd et al., 2004; Taylor, 2001; Spelman, 1995). 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, 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). There are times and places where these tactics will be beneficial, even if only in the short-term. For example, the significant seasonal effects found here suggest that crackdowns might be useful in addressing seasonal spikes in crime.

Sherman (1990) argued that, due to 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” 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 five days a week for 16 hours a day over a three month period did not result in initial deterrence decay whereas the same dosage over 22 weeks did. Whether or not 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 suggests 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.

THE POSSIBILITY OF BACKFIRE

Critics charge that crackdowns could result in a number of “backfire effects” (Weisburd, Hinkle, Famega and Ready, 2011) which 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 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 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, perceptions of police legitimacy might decline if residents see 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, 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 effectively combat crime (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 Durlauf and Nagin proposal’s goal is to reduce crime and incarceration, 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 increases in custodial sentences. Even with policies to limit
sentence lengths, Goldkamp (2011) suggests that this could result in a sustained use of imprisonment, yet a confinement population that more rapidly turns over. Better funded crackdowns might increase the efficiency with which offenders are arrested, 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 to simply bypass the criminal justice process after arrest. Although celerity’s role in deterring is largely overlooked by Durlauf and Nagin, it could become pertinent if crackdowns are overused.

**MOVING BEYOND CRACKDOWNS**

More holistic policing strategies are almost certainly necessary if Durlauf and Nagin’s proposal is to succeed, even if crackdowns can be deployed with a great degree of efficiency. Most police agencies disproportionately allocate patrol resources 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: 246-47), and also a need to move away from responses that are “narrow and predictable” (Rosenbaum, 2006: 249). Unfortunately, there are frequently operational barriers which impede the implementation of innovative policing tactics. Problem-oriented policing evaluations, for example, often report the implementations 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 a number of other factors which contribute to crime such as social disorganization, offender re-entry, or the physical environment (Rosenbaum, 2006). Whether or not 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 appear 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 (ibid.: 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 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 might be a valuable tool during such initiatives. If officers were deployed on foot in hot spots for a relatively short period they
may not only exert a deterrent effect, but may 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) advocates for a community “co-production model.” Likewise, Braga and Weisburd (2010:203) stress 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, 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 here, 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 further refined by focusing at hot spots may be beneficial. This would require police to acquire knowledge of prolific offenders operating within hot spots through data analysis and intelligence gathering. Wood et al. (et al.) 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 in order to simultaneously reduce crime, incarceration and correctional spending. Hot spots policing techniques were included in this discussion. Although a number of 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 which rely solely on deterrence because they elicit crime reductions that are only short-term and decay rapidly.

Although foot patrols and crackdowns more generally appear to be a 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 proposal are to come to fruition. Foot patrol as a specific tactic appears to fit into a number of more holistic strategies, such as problem-oriented policing, community policing and intelligence-led policing. A number of suggestions for incorporating foot patrols into these policing
paradigms were presented, though 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.

Notes

1. In this paper 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 offences in specific places.”

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

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

4. Ratcliffe et al. (2011, p. 806-807) report 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 (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 note that when a paired samples t test was run on pretreatment violent crime counts a minor yet statistically significant difference between the treatment and control locations [t(118) = 2.03, p < .05]. Shadish, Cook and Campbell (2002) note that even when matched designs are used, randomization may result in mean differences between groups, however, randomization negates the possibility that these differences are due to systematic bias. We control for these minor differences by entering a pre-treatment crime variable at level two.

5. As Weisburd and Green (1995: 354) note, “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).

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 a vehicle infraction was observed.

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 since 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 below, yet with a two week lag where treatment period one was transformed into a non-treatment 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, \( t = -0.936, p = 0.35 \); Phase 2 lagged treatment, \( t = -1.599, \ p = 0.11 \)).

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 one beats), and discern (if statistically significant) the expected decline in the differences between treatment and control beats at each time period by multiplying the percentage difference by the time block. For example if treatment beats had levels of crime that were 2.5 percent lower that 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 treatment and control beats \([2.5(1) = 2.5]\). This allows for a direct test of Sherman’s (1990) concept of initial deterrence decay.

9. 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.

10. Two preliminary models were run. The unconditional model controlling only for exposure confirmed there was significant variation 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-one (see Raudenbush and Bryk, 2002: 23-29).

11. This may seem contradictory to previous findings; however, the data and analyses conducted by Ratcliffe et al. (2011) were cross-sectional and aggregated to a pre-treatment and during-treatment period. Our data encompass a longer pre-treatment period, we control for seasonal fluctuation with the temperature 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 two only reports the results from the fixed slopes.

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

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 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 which occurred in a treatment location (a) by crime which occurred in control (c) areas before the treatment period ($t_0$). The treatment success measure is expressed as:

$$\frac{a_{t_1}}{c_{t_1}} - \frac{a_{t_0}}{c_{t_0}}$$

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:

$$\frac{b_{t_1}}{c_{t_1}} - \frac{b_{t_0}}{c_{t_0}}$$

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

$$WDQ = \frac{\frac{b_{t_1}}{c_{t_1}} - \frac{b_{t_0}}{c_{t_0}}}{\frac{a_{t_1}}{c_{t_1}} - \frac{a_{t_0}}{c_{t_0}}} = \frac{Buffer \ Displacement}{Treatment \ Success}$$
References


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