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The possible "backfire" effects of hot spots policing: an experimental assessment of impacts on legitimacy, fear and collective efficacy

David Weisburd · Joshua C. Hinkle · Christine Famega · Justin Ready

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Abstract
Objectives To examine the impacts of broken windows policing at crime hot spots on fear of crime, ratings of police legitimacy and reports of collective efficacy among residents of targeted hot spots.
Methods A block randomized experimental design with a police intervention targeting disorder delivered to 55 treatment street segments with an equal number of segments serving as controls. Main outcomes were measured using a panel survey of 371 persons living or working in these sites.
Results The broken windows police intervention delivered to crime hot spots in this study had no significant impacts on fear of crime, police legitimacy, collective efficacy, or perceptions of crime or social disorder. Perceptions of physical disorder appear to have been modestly increased in the target areas.
Conclusions The findings suggest that recent criticisms of hot spots policing approaches which focus on possible negative "backfire" effects for residents of the targeted areas may be overstated. The study shows that residents are not aware of, or much affected by, a three hour per week dosage of aggressive order maintenance policing on their blocks (in addition to routine police responses in these areas). Future research needs to replicate these findings focusing on varied target populations and types of crime hot spots, and examining different styles of hot spots policing.

D. Weisburd (✉)
The Hebrew University, Jerusalem, Israel
e-mail: msefrat@mscc.huji.ac.il

D. Weisburd
George Mason University, Fairfax, VA, USA

J. C. Hinkle
Georgia State University, Atlanta, USA

C. Famega
California State University–San Bernardino, San Bernardino, USA

J. Ready
Arizona State Unversity, Phoenix, USA

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In recent years, there has been a growing consensus that police can be effective in combating crime and disorder problems when they focus resources on very small areas with a high level of crime problems (Braga and Weisburd 2010; National Research Council 2004; Weisburd and Eck 2004). These areas are commonly termed crime hot spots, and a number of experimental and quasi-experimental studies from the mid-1990s on have shown that police can reduce crime and disorder by focusing on high-crime places as opposed to spreading police resources broadly across beats, precincts or cities (e.g., Braga and Bond 2008; Braga et al. 1999; Sherman and Weisburd 1995; Weisburd and Green 1995). Despite this growing consensus over the effectiveness of hot spots policing approaches in combating crime and disorder, the tactic is still not without its critics. Much of the criticism is focused on the potential for increased police presence and activity in small crime hot spots to have collateral consequences for residents living in these areas. For instance, some have expressed concern that hot spots tactics risk increasing fear of crime and eroding police-community relations (Kochel 2011; Rosenbaum 2006; for such concerns raised by supporters of hot spots policing, see Braga and Weisburd 2010; Weisburd 2004; Weisburd and Braga 2003).

Possible "backfire" effects of hot spots policing are particularly important to assess given theories that have argued that the police must reduce fear and increase collective efficacy and legitimacy in affected areas if they are to have long-term effects on crime. In their seminal "Broken Windows" article in the *Atlantic Monthly*, Wilson and Kelling (1982), for example, argued that the key to neighborhood decline was gradual withdrawal of citizens from the community and their inability to exercise community social control. Fear of crime is a critical variable in this thesis since heightened fear is a key factor in the withdrawal of citizens from the community (see also Skogan 1990). Sampson and Raudenbush (1999) also emphasize the importance of community social controls in their examination of the importance of "collective efficacy" in crime control (see also Sampson et al. 1997). In this case, the willingness of community members to intervene in community affairs is seen as critical to the long-term ability of the community to discourage crime. Finally, scholars have recently begun to emphasize the importance of the "legitimacy" of the police in the eyes of the public. In this case, it has been hypothesized that a key component of the ability of the police to successfully do something about crime is the acceptance of the legitimacy of police actions by the community (Mastrofski et al. 1996, 2000; McCluskey et al. 1999; Tyler 1990, 2004).

However, if the critics of hot spots policing are correct in their claims, the application of hot spots policing tactics may increase fear of crime, and decrease collective efficacy and police legitimacy. This would appear to be particularly true in cases where hot spots policing includes aggressive order maintenance strategies often associated with "broken windows" or "zero-tolerance" policing (e.g., see Greene 1999; Harcourt 2001). This would mean that the short-term crime and disorder benefits that studies have documented for hot spots policing could be undermined by possible long-term reductions in the abilities of residents of affected

places to exercise informal social controls. If residents become more fearful, then the broken windows hypothesis would predict increased crime in the long run. If collective efficacy is reduced, then community social controls would also be expected to be negatively affected. And finally, if legitimacy of the police is threatened, then the ability of the police to enlist citizens in crime control efforts will also be curtailed in the long run.

In this paper, we present findings from a randomized field experiment designed directly to address these issues. We focus on a "broken windows" policing style because we thought it represented a strategy that was both visible to the public and has been associated with aggressive police enforcement strategies (e.g., see Greene 1999). Also, as noted above, outcomes such as fear and collective efficacy are particularly relevant to disorder policing strategies at hot spots given the logic of the broken windows thesis. Specifically, the current study involved a randomized experimental test of the impacts of a broken windows style crackdown on disorder on residents of 55 targeted street blocks (as compared with 55 control street blocks) in three cities in California. Our central research questions are focused on the impacts the police intervention had on residents' fear of crime, collective efficacy and police legitimacy.

Hot spots policing: effectiveness and possible backfire effects

The notion that police can have a larger impact on crime by focusing on small locations that have serious crime problems is a relatively recent idea. While police have always known that certain areas had more problems than others, it was not until the late 1980s that technological advances allowed scholars to illustrate just how extremely concentrated crime was in very small geographic areas. The most prominent example of this was the finding that only 3 percent of the addresses in Minneapolis accounted for 50 percent of the crime calls for service (Sherman et al. 1989; see also Pierce et al. 1986; Weisburd and Green 1995; Weisburd et al. 2004). This finding had significant practical applications as it as it offered an explanation for why random preventive patrol had not been found to prevent crime (Kelling et al. 1974)—random patrol across beats or precincts makes little sense if crime is highly concentrated in a very small number of locations in a city (Sherman and Weisburd 1995).

The 1990s saw a series of studies of hot spots policing tactics, beginning with a randomized field trial in Minneapolis (Braga 2005). The Minneapolis Hot Spots experiment found that crime and disorder were significantly reduced in 55 target hot spots randomly assigned to receive extra patrols, relative to 55 control areas which received their normal level of police presence (Sherman and Weisburd 1995). Such a finding was good news for police, after a series of studies had shown that traditional police tactics such as random patrol (Kelling et al. 1974), expanding the size of the police force (Levine 1975), rapid response to calls for service (Spelman and Brown 1984) and police investigations (Greenwood and Petersilia 1975) had little or no impact on preventing or solving crimes. Later studies of hot spots policing tested not only increasing police presence through preventive patrol but also a variety of problem-oriented approaches at crime hot spots (see, for example, Braga et al. 1999; Braga and Bond 2008; Weisburd and Green 1995).

As a whole, the body of research on hot spots policing suggests it provides strong crime and disorder benefits. A Campbell systematic review of hot spots policing by Anthony Braga (2001, 2005) indentified five randomized experimental and four quasi-experimental studies testing the strategy. Braga found noteworthy reductions of crime and/or disorder in seven of these nine studies. Importantly, five of these studies also reviewed possible displacement of crime into nearby areas. Braga reports that none of the studies found displacement effects, and in four of five there is evidence of what Clarke and Weisburd (1994) define as a "diffusion of crime control benefits." The areas nearby the targeted hot spots actually improved relative to areas nearby the control areas. A recent randomized experiment examining the effectiveness of targeting disorder within a problem-oriented policing framework in Lowell Massachusetts also found strong crime prevention benefits without evidence of significant displacement (Braga and Bond 2008). The National Research Council committee on police practices and policies concluded in its review that "a strong body of evidence suggests that taking a focused geographic approach to crime problems can increase the effectiveness of policing." (2004: 35; see also Weisburd and Eck 2004). In turn, there is evidence that hot spots approaches have diffused rapidly in policing (see Kochel 2011; Weisburd and Lum 2005).

Potential backfire effects of hot spots policing

While there is a strong body of evidence that hot spots policing can reduce crime and disorder at crime hot spots, recent critiques of hot spots policing tactics have focused on potential negative impacts on citizen fear, collective efficacy and police legitimacy. These critiques argue that focusing intense police presence on small crime hot spots may have unintended negative consequences for the community or the police themselves (e.g., see Hinkle and Weisburd 2008; Kochel 2011; Rosenbaum 2006).

Hinkle and Weisburd (2008) in a reanalysis of data from the Police Foundation Displacement and Diffusion Study (Weisburd et al. 2006) found, for example, that citizens on blocks with intensified police crackdowns focused on disorder were more fearful as compared with citizens who lived on blocks which did not experience such crackdowns. They hypothesized that a sudden increase in police presence led citizens to believe that crime had also increased on their blocks.

Rosenbaum (2006) argues that hot spots policing may have strong impacts on the collective efficacy of residents of targeted places. Sampson and colleagues (1997) first coined the concept of collective efficacy to emphasize the capacity of a community to realize common values and regulate behavior within it through cohesive relationships and mutual trust among residents (see also Sampson 2004). A community with strong collective efficacy is characterized by "high capacities for collective action for the public good" (St. Jean 2007: 3). Rosenbaum (2006) argues that crime hot spots may already be places where collective efficacy is weak, and the application of "get tough" policies that have typified many hot spots policing strategies, "run the risk of further undermining social control and a community's capacity for regulation" (p. 257; see also Kochel 2011).

Rosenbaum (2006) also argues that hot spots policing will lead to adverse impacts on police community relations, and thus the legitimacy of the police in the community.

Legitimacy refers to the "judgments that ordinary citizens make about the rightfulness of police conduct" (National Research Council 2004: 291). Recent research suggests that when citizens perceive police actions as legitimate they are not only more likely to view the police more positively, but are also more likely to comply with the law in the future (National Research Council 2004: chapter 8). While residents may initially be pleased to have the police addressing problems on their street, Rosenbaum suggests that over time there is a risk that residents may begin to feel like targets of the police rather than partners in crime prevention. Kochel (2011: 17) argues, in turn, that the "police must recognize that in hot spots, they are working with populations who are more skeptical and less trusting of the police. Therefore, aggressive or intrusive policing tactics, while effective as short-term crime fighting strategies, may have long-term implications for police legitimacy."

Implications for long-term crime control

Increases in fear, and declines in collective efficacy and police legitimacy, are in themselves reasons for concern. However, if the critics of hot spots policing are correct, then these possible backfire effects challenge the long-term crime prevention benefits of these strategies. Fear is seen by some scholars not only as an important outcome for policing but also as an important factor in producing long-term crime trends. For example, Hunter (1978) formulated a model through which disorder and incivilities affected both crime and fear of crime. Hunter hypothesized that disorder and incivilities produce fear in residents because they feel that the external agencies of control have failed to preserve order. As residents perceive that matters are out of the control of local agencies, they begin to feel personally at risk of victimization. Hunter also suggested that this will increase crime, which in turn will further increase fear. Hunter's work can be viewed as the groundwork for the influential broken windows thesis (Wilson and Kelling 1982).

Wilson and Kelling suggested that untended disorder makes residents fearful as they conclude that social control has broken down in the neighborhood. Residents eventually withdraw from the community, lowering the level of informal social control. This in turn causes more disorder to occur, and may even cause local criminals to step up their offending as they conclude that social control is low and that their chances of being caught are slim. As the cycle worsens, criminals from outside areas may move their activities into the neighborhood as they too may perceive their risk of capture to be low. In this context, if hot spots policing is causing increases in fear, then the short-term benefits observed in most studies would be offset by the impacts of increased fear of crime on the long-term crime problems of places.

Similarly, if hot spots policing is decreasing collective efficacy at places, whatever the short-term crime prevention benefits of such strategies, it might be expected to increase long-term crime risks. In a recent study of crime trends at street segments in Seattle, Washington, Weisburd et al. (forthcoming) found that higher rates of collective efficacy (as measured by voting behavior) are strongly negatively associated with a street segment being a chronic crime hot spot. If hot spots policing is decreasing collective efficacy at such places, as is hypothesized by

Rosenbaum (2006) and Kochel (2011), then the long-term benefits of hot spots policing in reducing crime are again challenged.

The long-term effectiveness of policing on crime is also dependent on public perceptions of the legitimacy of police actions (National Research Council 2004; Tyler 1990, 2004). The police need the support and cooperation of the public to effectively combat crime and maintain social order in public spaces. Legitimacy here means more than simply popular support. Rather, it is a deeper and more complex notion that takes into account not only public support but also public willingness to recognize and defer to official authority. Legitimacy is the public belief that there is a responsibility and obligation to voluntarily accept and defer to the decisions made by authorities (Tyler 1990, 2004). If hot spots policing is negatively affecting perceptions of legitimacy of residents of affected streets, we have another reason to question whether the crime control benefits of this approach will be long lasting.

The lack of strong empirical evidence

Despite the importance of identifying such possible backfire effects of hot spots policing, there is little empirical evidence to date on the impacts of hot spots policing approaches on citizens who live in targeted areas in terms of either fear of crime, collective efficacy or attitudes toward the police more generally. A study by Hinkle and Weisburd (2008), noted earlier, examined the effects of a hot spots policing effort that targeted disorder and crime on citizen fear of crime as part of a more general study of displacement and diffusion of crime control benefits at crime hot spots (see Weisburd et al. 2006). Hinkle and Weisburd found that police crackdowns on crime and disorder hot spots led people living in the areas targeted to become more fearful of crime. However, that study was based on a correlational design, in which the affected hot spot areas had higher overall levels of crime than the comparison areas used in the study. Also, there is some evidence from other studies that residents in crime hot spots that are subject to focused police attention welcome the concentration of police efforts in problem places (e.g., Chermak et al. 2001; McGarrell et al. 1999). For example, a study linked to the Kansas City Gun Project (Sherman and Rogan 1995) found that the community strongly supported the intensive patrols and perceived an improvement in the quality of life in the treatment neighborhood (Shaw 1995).

Finally, one recent study by Braga and Bond (2009) examined community reaction to a problem-oriented policing initiative in Lowell, Massachusetts (see also Braga and Bond 2008). Data from interviews showed that the community perceived improvements in social and physical disorder and an increased number of contacts with the police. However, no statistically significant differences were found in fear of crime or perceptions of police tactics or demeanor. An important difference from the current study is that Braga and Bond interviewed 52 systematically selected "key community members" (pp. 99–101) rather than a random sample of residents of the hot spots. As their sample was designed to survey those who they defined as likely to be more knowledgeable about the area and to have more contact with the police, the findings were not generalizeable to the general population of residents of targeted hot spots.

Study design

The current study employed a randomized experimental design to test the impacts of a broken windows based hot spots policing approach on residents of targeted areas in three cities in California. We chose a disorder reduction tactic based on the broken windows thesis both because it had been shown to be effective in other hot spots studies (e.g., see Braga and Bond 2008; Braga et al. 1999; Green 1995, 1996; Weisburd and Green 1995) and because the strategy has generally been associated with an aggressive street-level presence of the police (Greene 1999; Hinkle and Weisburd 2008; Weisburd et al. 2006). The study sites are three mid-sized cities located approximately 90 minutes east of Los Angeles—Redlands, Colton and Ontario. Based on population estimates from the 2009 Uniform Crime Report, Colton is the smallest of the three cities with a total of 50,803 people covering 16 square miles, while Ontario is the largest with an estimated population of 173,212 spread over 50 square miles. Redlands lies in between these two with a population of 70,360 spread over 36 square miles. Based on the 2000 Census (U.S. Census Bureau (2000), both Colton and Ontario are majority Hispanic populations with both cities being over 60 percent Hispanic, while Redlands was 74 percent white.

The experimental intervention was applied at the street-segment level. However, our key outcome measures were assessed at the level of residents of the street segments studied. Accordingly, our study involved the random allocation of 110 street segments in the three cities to treatment and control conditions. Measurement of the effects of the intervention was carried out by assessing how a sample of individuals on each of the street segments responded either to the disorder policing hot spots approach delivered in the target areas, or to ordinary police service delivered in the control areas. We begin by describing the selection of the hot spot street segments for the study. We then turn to a description of the interventions at the study sites and the methodology we used for conducting the survey of residents used to measure outcomes.

Selection of street segments for the study

The street segment, which forms the main unit of analysis for our study, was defined as the two block faces on both sides of a street (including both of the intersections that are connected to the street block). As Weisburd et al. (2004) note, the street segment has long been seen as a key organizing unit within cities (Appleyard 1981; Jacobs 1961; Smith et al. 2000; Taylor 1997). It is viewed as the most appropriate unit for measuring perceptions of crime, disorder and fear of crime in the current study as we think that residents are much more likely to be accurately aware of such issues on the blocks on which they reside, rather than larger and more ambiguous geographic units such as neighborhoods.

Across the three cities, street segments were selected for the study on the basis of three criteria. First, the number of blocks targeted in each city was limited to a number that police leaders pledged to be able to deliver the police intervention to in sufficient dosage (see later). Prior studies suggest that it is not helpful to increase sample size at the expense of dosage. Indeed, in a review of experimental studies, Weisburd and colleagues (1993) found that effect sizes tended to decline as the

number of study cases increased. Weisburd and colleagues argued that a smaller treatment "dosage" in larger studies, as well as the increased heterogeneity in larger samples, was often a cause of these smaller effect sizes.

Second, study street segments were selected on the basis of having a sufficient degree of crime and disorder problems to require police attention. These thresholds were identified in consultation with the participating police agencies. As we will note in our discussion, the level of crime and disorder problems that are seen as serious in the police agencies we studied are relatively lower than those that are commonly found in hot spots policing studies in larger cities. In order to be included in the study a street segment and associated intersections must have reported 10 or more disorder calls for service[1] and three or more UCR Part 1 crime calls for service[2] the year before the experiment began.

We also set a threshold for the number of telephone numbers we could identify on a street segment. As the key outcomes in our study related to citizen attitudes, there had to be a sufficient number of respondents available for interviewing on each street segment. We used PowerFinder (from InfoUSA) reverse directory database software to identify respondents.[3] A street segment had to have at least seven phone entries in PowerFinder to be included in the database. Finally, each study street segment had to be isolated from every other study segment by a buffer of at least one full street segment in every direction. When choosing among segments clustered together, the segments with the highest amount of crime and disorder calls were chosen. These criteria left us with 110 street segments across the three cities—60 in Ontario, 30 in Redlands and 20 in Colton. As we describe later, this sample size was sufficient for measuring the main outcome measures in our study, but did not allow for a powerful statistical test of the impacts of the intervention on crime and disorder because of the relatively low base rate of crimes in the six-month study period.

We used a block randomized design approach in the random allocation of street segments to the control and treatment conditions (see Bloom 2005; Sherman and Weisburd 1995). The blocking factor in this study was the city and, accordingly, in each jurisdiction, we randomly allocated the street segments in equal numbers to control and treatment conditions within each city. For example, Ontario had 60 street segments included in the study (Redlands had 30 and Colton 20). Thirty of Ontario's segments were randomly assigned to receive the police treatment, with the other 30 remaining as controls. Randomization was conducted by generating a variable of

[1] For selection of study street segments, disorder was defined to include all calls for service for prostitution, drug possession, disturbing the peace, vandalism, public drinking, misdemeanor DUI, noise complaints, fights, and thefts from automobiles. We use a different measure of disorder in our analyses in this study, as we later decided that DUI, fights and automobile burglaries did not fit with Wilson and Kelling's (1982) concept of disorder. However, for selecting the study sites, they were considered as good proxies as places with such problems were deemed likely to also have issues with other types of social disorder (loitering, panhandling, vagrancy, etc.) which do not tend to generate many police calls for service.

[2] Part 1 crime included the FBI defined part 1 offenses, excluding thefts from autos which were included as a disorder for site selection purposes as outlined in footnote 1.

[3] For Redlands and Colton, we were able to bolster the PowerFinder database with a list of phone numbers on the study blocks provided by each city's water department. We were unable to obtain such data for Ontario, but this was less of a concern as the number of phones per street segment in the PowerFinder data were higher in Ontario than the other two cities.

random numbers in a dataset listing the street segments and then sorting the file by this variable. The first 50 percent of the segments in this sorted file for each city were designated as the treatment group, with the bottom 50 percent being assigned to the control group.

The police intervention

The treatment condition involved the use of a "broken windows" style approach to disorder. We established a one-day training program for participating officers. Following Wilson and Kelling (1982) and Kelling and Coles (1996), the training encouraged project officers to never ignore any incidents of disorder they encountered in the target areas. At the same time, they were told to use discretion in handling incidents, with the preference of using warnings and explanations of why a behavior such as public drinking or loitering was not permissible for all first offenses. Arrest and citation were to be used for subsequent problems with the same person, or cases with aggravating circumstances. For dealing with physical disorder, a rapid repair approach was employed. Officers were trained to report every instance of physical problems such as graffiti or litter to the appropriate city agencies for clean up, and to follow up with them if the problem was not dealt with in a timely manner.

In terms of police presence, after discussions with the three police chiefs, it was agreed that the designed dosage would be an extra three hours of police presence on each target block per week over the study period, with project officers focusing their time in these areas on combating social and physical disorder. This meant that the treatment sites received the normal levels of police presence that would come from citizen calls to the police, but would also receive three additional hours of attention per week as part of the special activities of officers assigned to the experiment. The control blocks received only their normal levels of police presence and activity that would come from police response to citizen calls to the police.

The police intervention began in mid-June 2008 and was carried out through mid-January 2009. After some initial implementation difficulties early in the project, this dosage was met or exceeded through most of the intervention period (see Figure 1). This meant that in Ontario a minimum of 385 hours of policing was applied to 30 hot spot street segments each month, in Redlands, an additional 190 hours to 15 hot spot street segments, and in Colton, an additional 130 hours to 10 hot spot street segments.

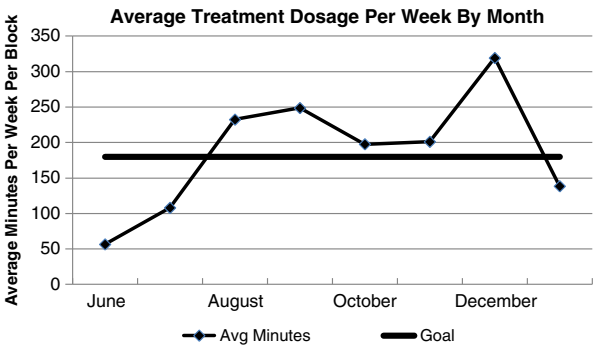

Fig. 1 Implementation of police intervention

Additionally, police working in the target areas were very active in dealing with disorder. From log sheets project officers filled out after each visit to a target area, data show that police dealt with 2,025 social incidents and 1,293 physical disorder problems across the target sites over the study period. This was in addition to normal police response to such problems in the context of citizen calls to the police.

The survey of residents

The primary data analyzed in this study come from a telephone survey of residents and business owners/managers residing on the study street segments. The surveys were administered in two waves, one before the police intervention and one afterwards. Surveys were collected by a team of trained research assistants at California State University, San Bernardino. The first wave was collected from early March through early June 2008.[4] The second wave began immediately after the end of the police intervention in mid-January 2009 and ran through late April. Businesses and residences on the study blocks were surveyed, and those respondents who completed the pre-intervention survey formed the sample for the post-intervention survey. Subjects for the survey were identified by phone numbers in our database, and eligible respondents were defined as the first person over 18 in a residence that was willing to complete the survey. For businesses, the eligible respondents were the owners or managers—we wanted stakeholders in the business and people who were likely to be present at the location often enough to have a sense of the social environment on the street segment.

Overall, a total of 836 responses were obtained on the final 110 street segments during the pre-intervention survey. Of these 836 completed surveys, 489 (58.5 percent) were residential surveys and 347 (41.5 percent) were business surveys. The cooperation rate, which represented the ratio of completed surveys in sampled households where a member of the interview team spoke directly to a person and were refused or unable to complete the survey, was 46.1 percent.[5] This cooperation rate falls in the middle of the range for response/cooperation rates for recent telephone surveys in the research on fear of crime (a key outcome in the current study).[6]

As noted above, this study employed a panel design for the survey as our main interests were within-individual changes in outcomes such as fear of crime after the hot spots police intervention. As such, the sample for the post-intervention survey was comprised of the 836 individuals who completed the pre-intervention survey. A second wave of the survey was collected immediately following the end of the police

[4] The survey was first piloted in a city removed from the study. All interviewers were required to satisfactorily complete pilot shifts before calling respondents in the study sample.

[5] The cooperation rate excludes cases that were coded as chronic no answer/busy/answering machine (n=307) and cases where there was a language (not an English or Spanish speaker) or cognitive barrier (n=59) from the denominator.

[6] Specifically, a study using random digit dialing in the state of Kentucky had a response rate of 27.5% (Rader et al. 2007), a study of fear in Dallas neighborhoods had a response rate of 33.4% (Ferguson and Midel 2007), and a study by Xu et al. (2005) achieved a response rate of 60%, and a study in Philadelphia had a response rate of 77% (Wyant 2008). It is worth noting that the Xu et al. data was collected by a police department (and respondents may be less likely to refuse a survey collected directly by the police) and the Wyant study involved a $10 monetary reward for respondents to encourage participation.

Table 1 Demographics of sample

Variable	Treatment mean	Treatment SD	Control mean	Control SD
Age (years)	43.4	14.59	45.5	15.75
Female	55.0%	.50	58.0%	.50
White	34.0%	.48	40.2%	.49
Black	4.3%	.20	6.3%	.24
Hispanic	49.5%	.50	43.7%	.50
Asian	5.3%	.23	4.6%	.21
Other race	6.9%	.25	5.2%	.22

Target $n=192$

Control $n=179$

intervention in January 2009 and ran through late April. Of the original 836 respondents, a total of 389 also completed the post-intervention survey, for a retention rate of 46.5%. Missing values for key variables of interest were imputed using EM imputation methods. During this process, 18 cases were dropped for having too many missing values. Thus, our final sample size is 371 individuals who completed the survey at both time points—192 from the 55 target blocks, and 179 from the 55 control blocks. Of these, 205 surveys were completed with residential respondents and 166 with respondents from businesses located on the study street segments. Table 1 shows the descriptive statistics on the demographics of the final sample. Comparing the sample of residents in the treatment and control conditions, we find that the randomization produced groups very similar in social and demographic characteristics. None of the background characteristics we examined showed significant differences between the two samples.

Measuring study outcomes

The main outcomes in our study include fear of crime/perceived risk of victimization, collective efficacy, and police legitimacy. We also examine perceptions of disorder and crime among residents.

Fear of crime/perceived risk The first fear/perceived risk measure used in this study is a scale created from a series of questions asking respondents how likely they felt they would become victims of various crimes[7] in the next six months. Response options were "very unlikely," "unlikely," "likely," and "very likely." These were coded as ordinal variables ranging from 1–4. The scale showed good reliability with Cronbach's Alphas of .901 and .875 for the pre- and post-intervention surveys, respectively. The second fear measure used a variation of a the standard NCVS fear of crime question which asked the respondents how safe they felt walking alone at night on their blocks with response options of "very safe," "somewhat safe," "somewhat unsafe," and "very unsafe." This was simply measured as an ordinal variable with a range of 1–4 with "very unsafe" as the high point on the scale.

[7] The crimes asked about included: robbery, assault (attacked by stranger), murder, sexual assault, burglary, car stolen, vandalism

Police legitimacy To gauge the impact of the police interventions, residents' opinions of the police and the job they were doing, respondents were asked whether they agreed or disagreed with the following statements[8]: "I have a lot of respect for the [city] police;" "I feel proud of the [city] police;" "I am very supportive of the [city] police;" "The [city] police treat people fairly." Response options were "strongly agree," "agree", "disagree," or "strongly disagree." The scale produced good reliability with Alphas of .925 and .904 for the pre- and post-intervention surveys, respectively.

Collective efficacy The collective efficacy measures were modeled directly after those used by Sampson and Raudenbush's (1999) with the wording altered to refer to the respondent's block rather than their neighborhood given the unit of analysis of the current study. The questions used represent adequate measures of the two main components of collective efficacy as outlined by Sampson and colleagues (1997)—social cohesion and willingness to intervene for the common good.

The items used to make up the collective efficacy factor were represented in two series of questions in our survey. The first measured social cohesion/trust, while the second was a measure of shared expectations for informal social control which asked respondents how likely it was that their neighbors would intervene in various situations. The scale showed an acceptable level of reliability with Alphas of .779 and .741 for the pre-and post-intervention survey data, respectively.

Perceived disorder and crime The perceived social disorder variable contains a series of items on the survey which asked residents to report how often the various types of social disorder[9] occurred on their block. The question was collected as an ordinal variable with response options of: "once month or less," "a few times a month," "a few times a week," "everyday," or "not at all." The perceived physical disorder variable was created in a similar manner by including responses to the items in the survey which asked respondents to indicate the prevalence of various physical conditions[10] on their block. They were given response options of "none," "a few" or "many" to indicate the prevalence of these physical disorder problems on their block. Both scales showed good reliability. For perceptions of social disorder, the Cronbach's Alpha were .857 in the pre-survey and .872 in the post-survey.

In addition to perceptions of disorder, the survey also asked respondents how often they thought a series of crimes[11] occurred on their blocks. Response options and measurement were identical to the social disorder items described above. The perceived crime scale demonstrated acceptable levels of reliability with Alphas of .701 and .815 in the pre-and post-intervention survey data, respectively.

Table 2 reports the baseline outcomes for these measures for the treatment and control samples.

[8] These questions were drawn from work on police legitimacy by Tom Tyler (1990; 2004).

[9] The perceived social disorder measure includes: fist fights, people loitering or being disorderly, public drinking, drunk or high in public, panhandlers, vandalism, people making too much noise late at night/ early morning, gambling in the street, drug sales, and prostitution.

[10] The perceived physical disorder measure includes: broken windows, graffiti, abandoned or boarded-up buildings, vacant lots, abandoned cars, litter, street or sidewalks in need of repair, and areas in need of better lighting.

[11] The perceived crime measure included: cars being broken into, burglary, robbery, shooting guns in public, sexual assaults.

Table 2 Pre-Intervention means for outcome variables

Variable	Scale min	Scale max	Treatment mean	Treatment SD	Control mean	Control SD
Perceived risk	7	28	17.15	4.08	17.05	4.36
Fear of crime	1	4	1.96	.96	2.02	.91
Police legitimacy	4	16	12.42	2.28	12.92	2.20
Collective efficacy	7	28	20.27	3.33	20.47	3.14
Perceived social disorder	0	40	7.00	7.49	7.56	7.72
Perceived physical disorder	0	16	2.46	2.56	2.63	2.43
Perceived crime	0	24	1.21	1.78	1.54	2.35

Target n=192

Control n=179

Statistical power

Our final sample included an average of 3.5 subjects per street segment and provided a statistically powerful test of our main hypotheses at the individual level. The power of our study, assuming a two-tailed significance test and a .05 significance level, was very strong for detecting what Cohen (1988) describes as moderate study effects (d=.50).[12] Indeed, the power level of our study for such effects is over .90. A small to moderate effect size of approximately .35 was needed to achieve a power level of .80.

While the power levels of our study are relatively high for identifying our main program effects, it is important to note that the statistical power of our study is relatively low for identifying the overall effect of the treatment as represented by official police data. This is due in good part to the relatively low base rate of crime and disorder at the sites during the intervention period, an issue we will return to again in our discussion. To give a sense of the magnitude of change that would have been required to have a high likelihood of observing a significant outcome during the six-month intervention period, we conducted a power analysis estimating the actual changes required for a power level of .80. Assuming no change in the control area, a decline of four crime calls per site on average would have been required—this would be a decline of 47.8 percent from the pre-intervention mean of 8.36 crime calls in the target areas.[13] For disorder, the changes required are not as great but still substantial. An average decline of five disorder calls per site would have been required to reach a power level of .80—more than a one-third decline from the baseline mean of 13.67 disorder calls in the target areas. Not surprisingly, given the power analysis we have described, our study does not yield statistically significant

[12] These power estimates were calculated using the Optimal Design software.

[13] This power analysis was conducted in the Power and Precisions Software, and based on t tests assuming no change from the pre-intervention mean in the control areas, and hypothesized changes from the pre-intervention mean in the target areas.

crime or disorder outcomes at the street segment level—though the overall direction of change is in favor of the treatment conditions (Weisburd et al. 2011).[14]

Results

Our analyses follow the block randomized design of the study, and are based on ANOVA models that include terms for the police intervention, city (the blocking term) and the interaction between the police intervention and city. [15] The outcome variables in all models are the pre- to post-intervention changes in each dependent variable at the individual level.[16]

Fear of Crime Hinkle and Weisburd (2008) have suggested, based on a non-experimental set of analyses, that hot spots crackdowns on crime and disorder will increase fear of crime because citizens may infer from heightened police presence that crime is getting worse on their block. Additionally, Rosenbaum (2006) had noted that even simply being labeled a hot spot by police may generate fear among residents. Conversely, Wilson and Kelling (1982) and later broken windows policing advocates have suggested that order maintenance policing will reduce fear by cleaning up disorder and eventually empowering communities faced with serious crime problems.

Our results do not support either position, and suggest that hot spots policing at the intensity we observed in this study does not strongly influence levels of fear among people who live on targeted blocks. Our first measure, perceived risk, shows a modest decline in fear in the target areas, but this result is not statistically significant (see Table 3). The second measure, adapted from the National Crime Victimization Survey, assesses residents' fear of walking alone at night on their block (see Table 4). There is very little change in both groups in this case.

[14] These results are based on regression models that include treatment, city, pre-crime counts, and an interaction of treatment and pre-crime counts. In the simple analyses of all cases only for crime outcomes, we do find a significant effect for treatment and the interaction of treatment and crime baseline outcomes. However, a sensitivity analysis shows that this effect is highly unstable and impacted strongly by outliers.

[15] As such there is some nesting of the data. However, given the small number of subjects per street segment, there are not enough cases in the level-one clusters to justify hierarchical data modeling. The mean number of subjects per segment is 3.5 with a range of 1 to 10. Sixteen segments have only one respondent, which means there is no within-cluster variation for nearly 15 percent of our segments. See Silver (2000) for a similar justification for not using multilevel modeling with a low number of subjects per level-two unit.

[16] We think that this approach is reasonable given our assumption that the treatment is delivered relatively similarly across sites and the balance of treatment and control cases in the survey. Nonetheless, we recognize that treatment was randomly allocated at the street-segment level and not the individual level. In order to examine the outcomes of the experiment strictly at that level, we also aggregated outcome variables to the segment level and ran the analyses on those variables as a sensitivity check. All results were substantively identical to the individual-level findings presented below in terms of direction of changes, and significance levels were also very similar. As an additional sensitivity test, we also analyzed the data using repeated measures split plot ANOVA tests with the pre- and post-intervention outcome variables as the within subjects repeated factor and the treatment and city as between subjects factors. These analyses were also substantively identical to both the analyses below and the segment-level analyses.

Table 3 Analysis of fear of crime: perceived risk measure—mean change in perceived risk, pre- to post-intervention by area

Area	Mean change	SD			
Target blocks (n=192)	−1.01	4.51			
Control blocks (n=179)	−0.79	4.25			
ANOVA results					
Source	Type 3 sums of squares	*df*	*F*	Partial eta sq.	Sig.
Corrected model	106.760	5	1.114	.015	.352
Intercept	251.159	1	13.106	.035	.000
Treatment	18.257	1	.953	.003	.330
City	35.779	2	.934	.005	.394
Treatment × City	57.650	2	1.504	.008	.224
Error	6994.752	365			
Total	7405.385	371			
Corrected total	7101.512	370			

R-squared=.015
(adjusted *R*-squared=.002)

Collective efficacy Those who have raised concerns about the impacts of heightened policing at crime hot spots have argued that it may reduce collective efficacy at affected places (Kochel 2011; Rosenbaum 2006). Our data again suggest that scholars may be overestimating the impacts of policing on attitudes of citizens who live on blocks targeted by hot spots policing. The differences between the two groups are small, and do not approach conventional levels of statistical significance (see Table 5).

Police legitimacy As we described earlier, there is very strong concern that aggressive police activities at hot spots will lead to citizens becoming alienated from the police. Our data, assessing the effects of aggressive order maintenance

Table 4 Analysis of fear of crime: safe walking at night question—mean change in safe walking at night question, pre- to post-intervention by area

Area	Mean change	SD			
Target blocks (n=192)	0.04	0.86			
Control blocks (n=179)	−0.03	0.39			
ANOVA results					
Source	Type 3 sums of squares	*df*	*F*	Partial eta sq.	Sig.
Corrected model	7.427	5	1.873	.025	.098
Intercept	.552	1	.696	.002	.405
Treatment	.148	1	.187	.001	.666
City	1.757	2	1.108	.006	.331
Treatment × City	6.255	2	3.944	.021	.020
Error	289.470	365			
Total	296.902	371			
Corrected total	296.897	370			

R-squared=.025
(adjusted *R*-squared=.012)

Table 5 Analysis of collective efficacy—mean change in collective efficacy, pre- to post-intervention by area

Area	Mean change	SD			
Target blocks (n=192)	−0.23	3.39			
Control blocks (n=179)	−0.45	3.25			
ANOVA results					
Source	Type 3 sums of squares	*df*	*F*	Partial eta sq.	Sig.
Corrected model	34.898	5	.629	.009	.678
Intercept	19.936	1	1.796	.005	.181
Treatment	1.155	1	.104	.000	.747
City	24.905	2	1.122	.006	.327
Treatment × City	6.429	2	.290	.002	.749
Error	4,051.884	365			
Total	4,127.901	371			
Corrected total	4,086.782	370			

R-squared= .009
(adjusted *R*-squared= −.005)

activities at street segments do not provide support for this concern. While the treatment subjects had slightly higher average evaluations of legitimacy than the control subjects, this result also did not approach conventional levels of statistical significance (see Table 6).

Perceptions of crime and disorder While our study is focused on the impacts of aggressive policing tactics on citizen perceptions of the police, we thought it important to also examine whether they believe that crime and disorder had changed on their block. When we ask citizens about crime on their block, we do find people on treatment blocks reported less crime relatively than subjects on the control blocks, though the differences are not large and not statistically significant (see Table 7). In

Table 6 Analysis of police legitimacy—mean change in police legitimacy, pre- to post-intervention by area

Area	Mean change	SD			
Target blocks (n=192)	−0.13	2.15			
Control blocks (n=179)	−0.35	2.17			
ANOVA results					
Source	Type 3 sums of squares	*df*	*F*	Partial eta sq.	Sig.
Corrected model	18.300	5	.783	.011	.562
Intercept	24.642	1	5.273	.014	.022
Treatment	3.957	1	.847	.002	.358
City	3.666	2	.392	.002	.676
Treatment × City	10.223	2	1.094	.006	.336
Error	1,705.719	365			
Total	1,745.068	371			
Corrected total	1,724.020	370			

R-squared= .011
(adjusted *R*-squared=−.003)

Table 7 Analysis of perceived crime—mean change in perceived crime, pre- to post-intervention by area

Area	Mean change	SD			
Target blocks (n=192)	0.17	2.60			
Control blocks (n=179)	0.45	2.85			
ANOVA results					
Source	Type 3 sums of squares	*df*	*F*	Partial eta sq.	Sig.
Corrected model	34.371	5	.926	.013	.464
Intercept	34.437	1	4.640	.013	.032
Treatment	8.761	1	1.180	.003	.278
City	10.567	2	.712	.004	.491
Treatment × City	16.666	2	1.123	.006	.327
Error	2,708.970	365			
Total	2,777.798	371			
Corrected total	2,743.341	370			

R-squared=.013
(adjusted *R*-squared= −.001)

the case of social disorder, the change in the two groups are very similar and again not statistically significant (see Table 8).

However, in the case of physical disorder there is a marginally significant (p=.082) change in the treatment areas (see Table 9). The treatment area subjects report higher relative levels of physical disorder than the control subjects. This may reflect the "backfire" effects hypothesized by Hinkle and Weisburd (2008). Many of the police activities in our study were directed at physical disorder, and many of the interventions demanded some activity of citizens on the blocks. For example, police might ask them to clean up trash from the yard, or ask property managers to repair broken locks, windows, etc. In this case, we might speculate that these "cues" made residents more aware of physical disorder on the experimental blocks.

Table 8 Analysis of perceived social disorder—mean change in perceived social disorder, pre- to post-intervention by area

Area	Mean change	SD			
Target blocks (n=192)	0.92	6.33			
Control blocks (n=179)	0.80	6.12			
ANOVA results					
Source	Type 3 sums of squares	*df*	*F*	Partial eta sq.	Sig.
Corrected model	161.232	5	.831	.011	.528
Intercept	255.906	1	6.597	.018	.011
Treatment	.225	1	.006	.000	.939
City	107.352	2	1.384	.008	.252
Treatment × City	52.351	2	.675	.004	.510
Error	14159.151	365			
Total	14596.016	371			
Corrected total	14320.383	370			

R-squared=.011
(adjusted *R*-squared=.−.002)

Table 9 Analysis of perceived physical disorder—mean change in perceived physical disorder, pre- to post-intervention by area

Area	Mean change	SD			
Target blocks (n=192)	0.57	2.60			
Control blocks (n=179)	0.23	2.13			
ANOVA Results					
Source	Type 3 sums of squares	*df*	*F*	Partial eta sq.	Sig.
Corrected model	51.453	5	1.827	.024	.107
Intercept	54.801	1	9.732	.026	.002
Treatment	17.071	1	3.032	.008	.082
City	23.500	2	2.087	.011	.126
Treatment × City	16.544	2	1.469	.008	.232
Error	2055.317	365			
Total	2168.480	371			
Corrected total	2106.770	370			

R-squared=.024
(adjusted *R*-squared=.011)

Discussion

Our findings provide the first experimental evidence on the effects of broken windows policing on residents who live at crime hot spots. Our results do not support the concerns of the critics of hot spots policing (Kochel 2011; Rosenbaum 2006). We do not find that the levels of aggressive order maintenance delivered in our study increase citizen fear or reduce perceptions of collective efficacy or police legitimacy as critics of hot spots policing had hypothesized. At the same time, our study does not provide evidence of short-term effects either in reducing fear or increasing collective efficacy and police legitimacy as suggested by proponents of broken windows-based policing tactics.

One way of interpreting our results is to acknowledge that ordinary citizens are not very aware of police activities unless they are directly impacted by them through interactions with the police. This would explain, for example, the fact that only in the case of perceived physical disorder did we observe study impacts. As noted above, police often enlisted citizens in doing something about physical disorder on the street segments. While we often assume that citizens will be very aware of police presence, our findings suggest that even adding three hours of police activities on a street segment each week does not necessarily mean that citizens come into regular contact with the police. People are ordinarily going through their daily routines which may include spending large parts of their day at work or shopping or carrying out other daily routines. This may mean that the likelihood of observing the police on a daily basis is not very high, even when police presence is intensified.

Our study did not have a direct measure of whether respondents observed the police during the study period because the survey was conducted in the pre- and post-intervention periods. We did, however, ask respondents more generally whether they observed increased police activity over the "last six months" which, for the post-intervention survey, would have included part of the intervention period (the average time

to survey of respondents was approximately one month after the end of the program). The responses did not differ significantly between the treatment and control conditions. It might be argued that this is a result of the nature of the police activities carried out at these hot spots. For example, though "stops and frisks" were commonly carried out on the experimental blocks, there were few large-scale police crackdowns which would have had much higher visibility. Nonetheless, even in the Police Foundation Displacement and Diffusion Study (Weisburd et al. 2004), where much more visible and intensive police interventions at hot spots were brought, similar findings were gained. Respondents in the treatment blocks did not report an increase in police presence during the study period. In this case, as in our study, it may be the result of the surveys being collected after the intervention period. But the consistency of these findings, and of our results more generally, suggest that ordinary people are not as strongly affected by hot spots policing as has been presumed by critics of this approach.

This is perhaps not surprising as past research on citizen perceptions of crime and fear of crime often show a disjuncture between citizen evaluations and actual conditions of their neighborhoods. For instance, since the 1990s, studies have consistently shown that fear of crime was not significantly related to actual crime rates, or people's actual risk of victimization (see Ferraro 1995 for a review). Further, in a study comparing researcher-observed street-segment level social disorder with perceptions of social disorder among residents of the same blocks, it was found that there was very little correspondence between the two measures (Hinkle and Yang 2008). In short, people simply may not have accurate perceptions about police activities, levels of social problems and other issues on their block, as their daily routines either take them away from their block or keep them indoors.

Of course, if we had surveyed offenders in these areas or others who spend much more time on the street, we expect that our results might have been different. In the Police Foundation study noted above, for example, offenders interviewed in the treatment areas reported being very much aware of heightened police attention at the hot spots, though they were not necessarily aware of the precise geographic areas affected by the interventions (Weisburd et al. 2004; 2006). Similarly, one may find different results if research is focused on community activists or place managers who are more likely to have more regular interaction with the police than ordinary community members. As reviewed earlier, Braga and Bond (2009) interviewed 52 "key community members" of hot spots during an experimental test of disorder focused problem-oriented policing and found that they reported more contacts with police after the intervention. However, despite reporting more contacts, even these key community members were unaware of any changes in police tactics, willingness of the police to work with citizens or officer demeanor.

An assumption that residents are not very much aware of police activities on an everyday basis provides an explanation for why the "negative externalities" of hot spots policing are not observed in our study. Legitimacy evaluations do not decline in this context, and fear does not increase because ordinary people do not have a good deal of interaction with the police. But an observation that ordinary people are not necessarily aware of increases in police activities on their block does not explain why fear of crime does not decline or collective efficacy does not increase on blocks where the police have worked hard to ameliorate disorder problems. Following the broken windows thesis, we would expect that police work directed at problem

blocks would lead in the long run to improvements in disorder and then reductions in fear of crime. Our study did not have a powerful design to detect impacts on crime and disorder, but irrespective of those impacts, the reduction in fear in the broken windows model was seen to result from the presence of the police in the community and not in any specific reductions in crime. Such reductions were expected to come later in a developmental cycle. We do have measures of the activities of police, and those show that there was concentrated and consistent order maintenance policing carried out through the experimental period.

One explanation for our results may simply be that we do not observe these blocks long enough. Broken windows theorists argue that there is a developmental cycle (Kelling and Coles 1996; Skogan 1990; Wilson and Kelling 1982), and that cycle may take a long period to reach the stage where citizen attitudes are affected. Accordingly, our study does not show that the broken windows approach "doesn't work," but only that the developmental cycle does not appear in the short follow-up period of our study.

While our study provides the first experimental evidence about the effects of hot spots policing on perceptions of the police, fear of crime and collective efficacy, we think it important before concluding to note some very specific limitations of our study. The first relates to the study sites. As we noted earlier, the hot spots of crime in this study have much lower levels of criminal activity than crime hot spots in many other studies conducted in larger more densely populated urban areas. While the level of crime and disorder at hot spots in the three cities we studied is worrying to police administrators, it may be that if our study examined more serious crime hot spots in larger cities the results would have been different.

In this context, our findings suggest that, in smaller cities, where social and physical decay are at more modest levels, hot spots policing neither leads to the negative outcomes of its critics or the positive outcomes (at least in the short run) of broken windows policing advocates. In the case of broken windows policing, we think it important to note that in their original formulation, Wilson and Kelling (1982) suggested that places at a tipping point where crime and disorder were not necessarily "out of control" provided particularly promising targets for order maintenance policing. The sites in our study would seem to fit this model. In this context, one might have expected the places we examined to be particularly appropriate for these interventions.

It is also the case that the nature of the police interventions and their intensity may influence the outcomes observed. We did not ask police to "harass" citizens on the treatment blocks, nor did our study adopt a zero tolerance approach. Police were instructed to develop step-by-step strategies that would give citizens a chance to improve their behavior. While it might be argued that a much more aggressive zero tolerance approach could have impacted citizens more directly, we think it important to note that Kelling and colleagues (e.g., Kelling and Coles 1996) have emphasized that zero tolerance is not an appropriate broken windows approach, in good part because it would be expected to increase fear and public concern about the police. We recognize that if the police used more aggressive tactics as in the Police Foundation Displacement and Diffusion Study, our results might have been very different.

Our study does not say that hot spots policing will not affect people on the targeted streets. People who are home more often and observe street behavior may

be encouraged or become fearful from seeing police on their street. Young people who are more likely to have contact with the police may also be strongly affected. Moreover, our study assessed only one specific hot spots policing strategy, and other strategies might have a different impact. This suggests the importance of replicating our study for specific populations who live at crime hot spots (e.g., community activists; see Braga and Bond 2009), and for specific types of hot spots policing strategies such as preventive patrol, problem-oriented policing, or legitimacy policing at crime hot spots (see Braga and Weisburd 2010).

Conclusions

The critics of hot spots policing have argued that intense police activities directed at small geographic areas are likely to lead to increased fear of crime, and decreases in collective efficacy and perceptions of police legitimacy. These challenges are particularly important because of the links that have been made between these outcomes and long-term crime trends. Increased fear has been seen as an important part of community decline leading to crime and violence (Skogan 1990; Wilson and Kelling 1982). Decreased collective efficacy has also been linked to increasing crime at the street-segment level (Weisburd et al. 2011). In turn, police legitimacy has been identified as a key element of the ability of the police to succeed in their crime control efforts (National Research Council 2004). Irrespective of the short-term gains found for hot spots policing programs, if the strategy leads to increased fear, and decreases in collective efficacy and police legitimacy, it is likely in the long run that the crime and disorder control benefits of hot spots policing will be undermined.

Our study provides an experimental field test of these concerns. We find that hot spots policing crackdowns on disorder do not lead to the backfire effects that critics have hypothesized (e.g., see Kochel 2011; Rosenbaum 2006). At least at the level and intensity employed in the current study, residents of crime hot spots appear to be relatively unaffected by intensive police interventions at hot spots. Our findings thus suggest that the crime and disorder benefits of hot spots policing are not likely to be undone by long-term impacts on residents of these areas. However, our results are of course based on a single study, using a specific set of strategies, in jurisdictions with only moderate levels of crime. A great deal of further research is needed to examine whether these findings hold up in other studies that vary strategies and crime levels of jurisdictions examined.

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David Weisburd is the Walter E. Meyer Professor of Law and Criminal Justice and Director of the Institute of Criminology of the Hebrew University Faculty of Law in Jerusalem, and a Distinguished Professor of Criminology, Law and Society at George Mason University and Director of its Center for Evidence Based Crime Policy. Professor Weisburd has been studying crime hot spots for two decades, and received the 2010 Stockholm Prize in Criminology for work in this area.

Joshua C. Hinkle is an Assistant Professor in the Department of Criminal Justice at Georgia State University. He received his doctoral degree in criminology and criminal justice from the University of Maryland. His research interests include evidence-based policing and the impacts of geographically-focused policing interventions on residents of targeted areas.

Christine Famega is an Associate Professor in the Department of Criminal Justice at California State University - San Bernardino. Professor Famega's research interests include police technology and the organizational context of proactive and reactive policing.

Justin Ready is an Assistant Professor in the Department of Criminology and Criminal Justice at Arizona State University. He received his Ph.D. at Rutgers University, where he served as a Project Manager in the Center for Crime Prevention Studies. He later worked as a Senior Research Associate at the Police Foundation. His research focuses on urban policing strategies, crime displacement, and environmental criminology.