Assets Coming Together (ACT) at Crime Hot Spots:

An Experimental Evaluation in Brooklyn Park, Minnesota

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September 2018

Abstract

There is strong evidence that focusing police resources at crime hot spots (small geographic places at which crime is spatially concentrated) is an effective crime control strategy (Braga, Papachristos, & Hureau, 2012; 2014; Weisburd & Majmundar, 2017). However, hot spots policing and related strategies rarely take into account the social context of crime at place, despite growing emphasis on police-community collaboration to improve public safety and reinforce informal social controls (Weisburd, Davis, & Gill, 2015) and emerging empirical evidence that social disorganization and collective efficacy may influence crime patterns at the micro-geographic level (Weisburd, Groff, & Yang, 2012; Weisburd, White, & Wooditch, in progress). In this report, we describe an innovative policing approach—*Assets Coming Together to Take Action (ACT)*—that attempts to reduce crime at hot spots by garnering greater cooperation and collaboration with the public in addressing crime problems. The intervention was also designed to increase collective efficacy among citizens residing in the hot spots, and improve their evaluations of police legitimacy. A key innovation of the program was the use of patrol resources rather than special units to implement the program.

Our evaluation shows that in a sufficiently staffed[1] police department, unallocated patrol time can be used to develop an innovative community-based hot spots policing program. We find that patrol officers followed the program model with fidelity. The program also increased collective efficacy actions directly related to the program, such as participating with neighbors in problem-solving and speaking to the police about problems. A key question, of course, is whether the program led to crime prevention gains. As we show below, the answer to this question is complex because the program's emphasis on community collaboration with the police

[1] The International Association of Chiefs of Police (2005) generally recommends that officers should be able to devote one-third of their shift to proactive patrol time.

led to increased reporting of crime (which in turn confounds estimates of crime outcomes). While the program does not impact upon uncorrected crime outcomes, a correction factor applied to the data to account for this increased reporting suggests promising prevention outcomes. We do not find significant impacts on traditional general measures of collective efficacy or evaluations of police legitimacy. We argue that the time period examined may not be long enough to adequately assess such changes, and that broadly defined measures of collective efficacy may not capture the specific contributions of police led initiatives.

Background

Over the last three decades criminologists have emphasized the opportunities presented by crime hot spots for successful crime prevention (Eck & Weisburd, 1995; Sherman, Gartin, & Buerger, 1989; Sherman & Weisburd, 1995; Weisburd, 2008; Weisburd, Groff, & Yang, 2014; Weisburd, Telep, & Braga, 2010; Wilcox & Eck, 2011). In part, the impetus for this proposal has come from evidence of the very strong clustering of crime at a micro-geographic level. Weisburd (2015; see also Weisburd & Amram, 2014; Weisburd et al., 2012) has argued that this concentration is so general and consistent across cities that it leads to a *law of crime concentration* whereby there are similar levels of crime concentration across time and across cities. There is now a large number of studies that show that a substantial proportion of crime in cities is concentrated at crime hot spots (Pierce, Spaar, & Briggs, 1988; Sherman et al., 1989; Weisburd, 2015; Weisburd & Amram, 2014; Weisburd, Bushway, Lum, & Yang, 2004; Weisburd et al., 2012; Weisburd, Telep, & Lawton, 2014). Indeed, suburban cities like Brooklyn Park, the setting of the present study, have been found to have especially high levels of crime concentration (Gill, Wooditch, & Weisburd, 2017; Weisburd, 2015). Importantly for crime prevention, the locations of these hot spots are relatively stable over time and vary across communities, suggesting that micro-place effects do not simply reflect neighborhood-level crime problems (Groff, Weisburd, & Morris, 2009; Groff, Weisburd, & Yang, 2010; Weisburd & Amram, 2014; Weisburd, Groff, et al., 2014; Weisburd, Morris, & Groff, 2009).

In line with the implications of the law of crime concentration at places for crime prevention, studies (including a number of experimental field trials) provide strong evidence that hot spots policing strategies reduce crime (Braga & Bond, 2008; Braga et al., 2012; 2014; Lum, Koper, & Telep, 2011; Sherman & Weisburd, 1995; Telep, Mitchell, & Weisburd, 2014; Weisburd & Eck, 2004; Weisburd & Green, 1995). The Committee to Review Police Practices and Policies of the National Research Council (Skogan & Frydl, 2004) concluded that "[…] studies that focused police resources on crime hot spots provided the strongest collective evidence of police effectiveness that is now available" (p. 250). This conclusion has been reinforced more recently both by a subsequent National Academy of Sciences committee on proactive policing (Weisburd & Majmundar, 2017) and a Campbell Collaboration systematic review (Braga et al., 2012; see also Braga et al., 2014). These interventions are effective because they increase the efficiency of police, allowing them to deal with a large proportion of crime by focusing on a small number of places (Weisburd & Telep, 2010). The evidence base also suggests that displacement of crime to surrounding places is not an inevitable consequence of such focused police efforts; conversely, diffusion of crime control benefits from "treated" locations is more likely (Braga et al., 2012; 2014; Telep & Weisburd, 2016; Weisburd & Eck, 2004; Weisburd & Majmundar, 2017).

Police typically rely on one of two approaches to control opportunities for crime in hot spots. The first involves deterring potential offenders through formal guardianship. High levels of police visibility and activity at places, whether in a car or on foot, are intended to increase offenders' perceived risk of detection and apprehension (Durlauf & Nagin, 2011; Koper, 1995; Sherman & Weisburd, 1995; Telep et al., 2014). The second involves modifying the situational characteristics of places so that they are less amenable to crime (Braga et al., 1999; Braga & Bond, 2008; Braga & Schnell, 2013; Weisburd & Mazerolle, 2000).

However, these strategies rarely take into account the social context of variations in crime at places and the role of informal community processes in social control (Weisburd, 2012;

Weisburd, Groff, et al., 2014). Social characteristics of places, such as the socioeconomic levels of residents and population heterogeneity, have traditionally been the focus of social disorganization theories that are applied to the study of crime and crime prevention at meso-geographic places, such as neighborhoods within cities (Bursik, 1988; Bursik & Grasmick, 1993; Bursik & Webb, 1982; Kubrin & Weitzer, 2003; Sampson & Groves, 1989; Sampson & Morenoff, 1997; Shaw & McKay, 1942; Shaw, Zorbaugh, McKay, & Cottrell, 1929). Social disorganization theories suggest responses to crime that are based on informal social controls, or "the effectiveness of informal mechanisms by which residents themselves achieve public order" (Sampson, Raudenbush, & Earls, 1997, p. 917). Sources of the differential ability of communities to regulate their residents are reflected in structural characteristics such as poverty and residential mobility, or the ability of neighborhoods to restrain unruly juveniles. Sampson et al. (1997) coined the concept of collective efficacy of communities, which is based on the "willingness [of residents] to intervene for the common good" (p. 919) and social ties or cohesion among community members, to emphasize the mechanisms by which a community can prevent crime through enhanced informal rather than formal social controls.

Over the last three decades, American police have placed increasing emphasis on working closely with communities to increase public safety, reduce fear of crime, and solve crime problems through community-oriented policing programs, thus reinforcing these informal social controls (Gill, Weisburd, Telep, Vitter, & Bennett, 2014; Greene, 1993; Reisig, 2010; Weisburd & Braga, 2006). However, this broad set of ideas and potential interventions has not been applied to hot spots policing, where the key mechanisms are typically deterrence and reducing crime opportunities. This is not to say that community collaboration has not been a component of some hot spots policing programs. In the Lowell Hot Spots Policing Experiment

(Braga and Bond, 2008), for example, community collaboration was an element of the problem-oriented policing approach tested. Nonetheless, community collaboration was an approach used to enhance problem-solving efforts and not a key goal of the program itself.

Bringing community-oriented interventions into the equation

It is striking that police scholars have virtually ignored social disorganization in thinking about crime prevention at hot spots. Sherman et al. (1989), for example, argued that "[t]raditional collectivity [social disorganization] theories may be appropriate for explaining community-level variation, but they seem inappropriate for small, publicly visible places with highly transient populations" (p. 30). Are the concepts of social disorganization and informal social control inappropriate for understanding the variability of crime at specific places? Indeed, if the only units of analysis relevant to social disorganization are large geographic units like neighborhoods, this is a reasonable position.

However, we argue for another approach that is relevant to hot spots policing. Micro-geographic units such as individual street segments (the two block faces between two intersections) or specific facilities are not only physical settings but also "behavior settings," or "small-scale social systems" (Wicker, 1987, p. 614; see also Taylor, 1997; Weisburd et al., 2012). They have standing patterns of behavior where residents take on certain roles and behavioral norms. People who frequent a street segment get to know one another and become familiar with each other's' routines. They can be seen as the first building block of communities, or even "micro-communities" within themselves, where face-to-face contacts between residents, visitors, business owners and so on are structured in clearly demarcated settings. These micro-communities share many of the traits that are crucial to social disorganization theory at the meso-level, in that these physical units also function as social units with specific routines.

If small geographic units can be considered a type of micro-community, then social disorganization and informal social controls have direct relevance to our understanding of crime at micro-places (Rice & Smith, 2002; Smith, Frazee, & Davison, 2000; Taylor, 2012; Taylor, Gottfredson, & Brower, 1984). If characteristics relevant to the community's ability to self-regulate, such as residential mobility, disadvantage, and collective efficacy, also vary across micro-places, are they also related to micro-place variability in crime? Weisburd et al. (2012) collected data on related structural factors at the street segment level in Seattle, Washington. Their data indicated that there are micro-geographic hot spots of social disorganization and low social control, which vary considerably from street to street. For example, 50 percent of public housing assistance is consistently found on about 0.4 percent of street segments in Seattle. Within 800 feet of these housing assistance hot spots, 84 percent of street segments have no housing assistance recipients.

A key indicator of collective efficacy is residents' willingness to participate in public affairs (Morenoff, Sampson, & Raudenbush, 2001; Sampson et al., 1997). Weisburd et al. (2012) identified concentrations of collective efficacy as represented by the percentage of active voters on each street segment (see also Coleman, 2002; Putnam, 2001). They found that 50 percent of active voters lived on just 12-13 percent of Seattle's street segments. Again, there was considerable street-by-street variability in these high collective efficacy segments—only 25 percent of segments within 800 feet of active voter hot spots also evidenced such high levels of active voting. These findings of significantly lower levels of collective efficacy at crime hot spots has been replicated in a large NIH survey study comparing hundreds of crime hot spots with streets with little crime. There is a strong relationship between reported levels of trust and willingness to intervene and living on hot spot streets (Weisburd, 2018; Weisburd et al., in progress).

Police often view communities as a problem to be solved, rather than coalescing neighborhood assets to challenge criminogenic conditions. Fixing the broken window does not change the conditions that led to the window being broken or address the community's (un)willingness to fix it. We think the same is true for police approaches to hot spots. Hot spots enforcement strategies address the immediate need for police intervention, but the benefits slow as the police-induced order maintenance decreases. Can the police play a more substantive role in dealing with these underlying conditions?

In this study, we sought to examine whether social context has salience not only at the community level but also at the micro-geographic, hot spot level. With the Brooklyn Park (MN) Police Department (BPPD), we developed a police patrol intervention that was focused on increasing collective efficacy and especially cooperation between the police and the public in crime prevention efforts. The ACT program sought to identify *Assets* in the community—key individuals at the hot spot who could assist with crime prevention and encourage others to get involved—have those assets *Come together* to collaborate with the police, and finally to *Take action* to confront crime and related problems. The intervention is based on the assumption that social interventions at hot spots provide potential for the police to marshal a new and important strategy to do something about crime. It also assumed that such an intervention could be brought as part of the ordinary patrol operations of a police agency. Indeed, this is the first program of its type that we know of that seeks to rely on general patrol resources rather than specialized units to provide a wide scale application of a collaborative community problem-solving approach to crime hot spots. The logic of this approach is that patrol officers have much unallocated time at their disposal. ACT seeks to capitalize on that unallocated time for collaborative community-based crime control.

In this report, we describe the ACT program and our evaluation of its impacts. We used a partially blocked randomized design (see Weisburd & Gill, 2014) to evaluate the intervention in terms of three key outcomes: whether the idea of a large-scale program that sought to focus on social interventions could be implemented successfully in the context of patrol operations in the city; whether the intervention had impacts on crime; and whether the intervention had impacts on collective efficacy, cooperation with the police, and police legitimacy.

Methodology

Study setting

Brooklyn Park is a suburban city located immediately north-west of Minneapolis, Minnesota. The city is home to almost 79,000 residents and covers an area of 26 square miles. Brooklyn Park is the second-largest suburb in the Minneapolis-St. Paul metropolitan area and the sixth-largest in the state. Almost 30 percent of residents are under the age of 18 and around 11 percent are below the poverty level. The city is highly diverse: in 2016, nearly 22 percent of residents were foreign-born, just over half were non-White, and 28 percent spoke a language other than English at home.[2] In particular, Brooklyn Park has the highest concentration of Liberians outside Liberia; approximately 5 percent of residents are of Liberian descent.[3]

The BPPD has 108 sworn officers at full strength, including 48 officers, 9 sergeants, and 2 lieutenants in its patrol division. The city's crime rate is the highest among Minneapolis-St. Paul suburbs that have 50,000 or more residents. Crime is highly concentrated at streets where

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[2] All population and demographic information from the American Community Survey 2012-2016 5-year estimates, "Community Facts," http://factfinder.census.gov (accessed September 7, 2018).
[3] http://www.brooklynpark.org/our-sister-city-kakata-liberia/ (accessed September 7, 2018).

land use is mixed and population density and turnover are high. In particular, Brooklyn Park has a number of large, lower-income public housing and apartment communities, as well as single-family home neighborhoods that were hit hard by the recent recession and foreclosure crisis. Overall, just 2.1 percent of streets in the city produce 50 percent of the crime (Gill et al., 2017; Weisburd, 2015).

The idea of the police leading community-based interventions at hot spots is a new and somewhat radical idea for policing. Accordingly, there is no strong body of evidence or established model for practice to draw upon in advancing the approach. However, the collaboration between the BPPD and the Center for Evidence-Based Crime Policy at George Mason University (CEBCP), supported by the Bureau of Justice Assistance's Strategies for Policing Innovation (SPI) program (formerly the Smart Policing Initiative), provides the context and practical application for testing these ideas. This project sought to identify, develop, and test a department-wide program to increase collaboration and cooperation with the police, and collective efficacy more generally, at hot spots using a block-randomized experiment.

Description of the intervention

ACT is an innovative police-led collective efficacy-building and problem-solving program at hot spots. A key innovation of ACT was the involvement of the entire patrol force in delivering the program: all patrol officers were expected to undertake the activities described below during their discretionary time when they were not responding to 911 calls, taking breaks, or report writing. Approximately one-third of BPPD officers' time, or 3 hours and 51 minutes during a 12-hour shift, is discretionary or uncommitted, according to our analysis of dispatch data during the planning phase of the project (see also Famega, 2009).

ACT was intended to work through officers encouraging three key mechanisms at the hot spots: 1) the establishment of *proximal relationships* with and between residents; 2) increased *working trust* between the police and community members; and 3) the development of *shared expectations* that empower residents to take action against problems (Sampson et al., 1997; Uchida, Swatt, Solomon, & Varano, 2013); and then leveraging these mechanisms to develop successful problem-solving strategies. While ACT shares many similarities with established community- and problem-oriented policing strategies, the overall goal of the approach was to create a "culture of responsibility" within the community by connecting police intervention with the development of informal social control. The intervention comprised three stages:

Asset identification. At this initial stage officers were expected to "get to know" their hot spot, similar to the Scanning and Analysis phases of the scanning, analysis, response, and evaluation (SARA) model of problem-oriented policing. However, rather than identifying problems at the location at the outset, their main focus was on identifying key stakeholders and resources in each hot spot, known as *assets,* which needed to be brought together for problem-solving to occur. Assets included residents, business owners, and community groups, depending on the specific nature of the hot spot, as well as *anchor points* (Uchida, Swatt, Solomon, & Varano, 2014), such as social service agencies, schools, and places of worship that may not be located directly in the hot spot but are meaningful to residents there. In many cases, officers also had to identify *liabilities* at the location—individuals or organizations that may be contributing to the crime problem at the place. In these cases, part of the officers' approach to the hot spot involved stabilizing crime problems and addressing these liabilities to build positive relationships and trust with assets. Officers identified assets and liabilities by engaging community members in conversation during additional patrols in the hot spots during their discretionary time.

Coalescence/Coming together. The next stage of the intervention built on traditional community- and problem-oriented policing through the development of police-community partnerships that aims to increase residents' willingness to take ownership of the problem-solving process. In addition to continuing their intelligence gathering and patrol activities, officers planned creative approaches to bring identified assets together and build relationships between them. These included meet-and-greets, block parties and potlucks, community meetings, and setting up online communities, such as Facebook groups. Once officers made the initial introductions, assets began taking the initiative to collaborate in planning activities.

Taking action. In the final stage of the intervention, officers and assets at each hot spot moved from getting to know each other to identifying and addressing key community concerns. In many cases, the problems identified by community members were not directly crime-related, despite the focus on crime hot spots. Instead, many activities involved dealing with lower-level quality-of-life issues that residents felt affected the image of the community. This involved activities to promote the view that the area was a safe place where crime would not be tolerated, and helping to provide fun activities for children to keep them out of trouble. For example, in one hot spot, BPPD and residents set up a traffic study to address cars speeding through the street, which made it unsafe for children to play outside. In another site, a resident donated basketball hoops and equipment after hearing about the community's efforts on the news, and officers regularly joined games with local children while visiting the spot. Officers logged their activities and information about the assets and actions in a database stored on a Microsoft SharePoint server, which was custom-designed to meet the needs of the project.

As discussed above, the entire patrol force was involved in carrying out the intervention. To ensure all officers understood the relationship between collective efficacy and crime prevention and how to directly apply this concept to police work, we conducted two identical one-day training sessions covering each of BPPD's two patrol teams in April 2015. The training was designed and delivered by the full project team, including senior BPPD personnel and the project coordinator, the CEBCP research team, and subject matter experts in collective efficacy. The program included a brief introduction to the research project and theory, but focused primarily on tools developed by Uchida et al. (2014) to help officers build collective efficacy at the street-level.[4] An important part of developing the training was feedback from a small team of officers who spent a month testing the asset identification phase at a pilot hot spot.[5] This peer learning opportunity was crucial for obtaining buy-in from the patrol officers and giving them a real-world sense of what the intervention would look like in practice.

Implementation of the ACT program within the treatment condition

The implementation of ACT in the treatment hot spots was led by Inspector Bill Barritt of the BPPD, assisted by the project coordinator, Win Moua. The CEBCP team was led by Professor David Weisburd and Dr. Charlotte Gill, with the assistance of Alese Wooditch. Jody Murphy, BPPD's crime analyst, provided ongoing access to crime and geographic data. The project team met on a regular basis, generally twice a month, to discuss project development and implementation. These discussions often involved James "Chip" Coldren, Jr. from the Center for Naval Analysis (CNA), the SPI technical assistance provider, and subject matter experts Craig Uchida and Shellie Solomon from Justice & Security Strategies, Inc. These collaborative

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[4] Craig Uchida and Shellie Solomon participated directly in the design and delivery of the training program, as well as James "Chip" Coldren of CNA.

[5] This site was randomly selected from potential sites in the sampling frame. The assessment of the intervention did not include these data.

discussions and data reported by officers via SharePoint considerably benefited the project as they provided evidence to suggest that the intervention was administered as intended for the entirety of the project. For instance, when there was evidence to suggest that patrol officers had trouble identifying a sufficient number of assets or administering a sufficient number of actions, Inspector Barritt would meet with the officers assigned to those areas to provide guidance and ensure that the intervention was appropriately administered.

Two or three officers were assigned to each hot spot within existing patrol teams. Each sergeant was responsible for a small group of hot spots, with oversight from the day and night shift lieutenants. The officer teams also drew on support from other resources within the police department, such as the crime analysis unit and civilian crime prevention specialists, as needed. As this was a full patrol force intervention, each hot spot team included night shift as well as day shift officers. In practice, given the focus on regular community contact, and the fact that night shift officers typically had less discretionary time available, day shift officers did the majority of the outreach work, but night shift officers contributed in other ways, such as providing extra patrols and creating flyers and other materials for community events.

At the outset of the project, the police spent, as was directed, a large duration of time at the experimental sites. In the first three months of the project, officers spent approximately eight hours each month working on tasks related to their assigned study site. Across all 16 months of the project, the average amount of time spent on tasks at each hot spot per month was just under 6 hours. Table 1 shows the number of tasks performed, broken out by stage, type of task, and month. Extra patrol was the task most frequently recorded by officers during Stage 1 (asset and liability identification), accounting for almost half of the project activities. However, while the amount of extra patrol remained high throughout the project, officers were much more likely to spend time contacting residents or assets in Stages 2 (coming together) and 3 (taking action). These contacts accounted for over one-third of recorded tasks in those stages. Police identified a total of 405 assets across the treatment sites during the project. On average, 19 assets were identified per treatment site (SD = 8.5) and the median number was 18.

[INSERT TABLE 1]

The intervention was carried out for 16 months, from July 2015 through October 2016.[6] Our original implementation plan allowed 2 months for the asset identification phase, 5 months for coalescence, and the remaining time for taking action. Officers largely stayed within these timelines (see Appendix A, Figure A1), but there was between-site variation in the duration of intervention phases as some sites took longer than others to move to the next stage. In particular, the asset identification and coming together phases took longer than expected. Officers were still working on these activities through February of 2016, at which point most sites transitioned into the taking action stage. Sites generally moved from the identifying assets and liabilities stage to the coming together stage in September 2015.

The fact that officers were able to identify and interact with a large number of assets and implement a large number of actions provides support for the conclusion of strong program implementation. That combined with their ability to move through the three stages of the program provides evidence that the patrol force of a police agency can be used to carry out crime prevention programs at the same time that they continue their patrol activities. Later, in reviewing the survey results, we provide further evidence that BPPD successfully implemented the project.

[6] The intervention was initially intended to last for a year, but BPPD extended the implementation using their own funds to provide additional time for problem-solving and data for analysis.

The control condition

The control condition in our study received police response as usual. It is important to recognize that the police cannot withdraw service from places that are designated as hot spots of crime. This has also been the approach in a series of earlier experimental field trials in hot spots policing (see Braga et al., 2012; 2014). Accordingly, the control condition hot spots received normal police service, which included response to emergency calls, as well as special police response to community requests. The control hot spots did not receive interventions through the ACT program.

While the locations of the control sites were not disclosed to the officers, it is important to note that in specific cases control sites received special attention from the BPPD during the course of the experiment. Of our 21 control areas, three had special operations that included community elements overlapping with the ACT program. In each case, special requests were made to the police either by community groups or the city council for meeting and working with residents. In these sites, BPPD set up community meetings (monthly in some cases) with business owners, and apartment managers and owners. There were also efforts by owners to improve their properties, such as fencing around the apartment complex, and surveillance cameras at two locations. One of these sites was paired with a treatment area that had a major change in management during the experimental period. Section 8 housing was removed, and aggressive place management at the site occurred. Fortunately, this site was in a statistical block (block 2, see below) of only two hot spots so we are able to include and exclude both sites from our analyses below as a sensitivity analysis for our findings.[7]

[7] In a block randomized experiment (see below), blocks can be removed from the study without affecting the overall assumptions of an experimental design (see Weisburd & Gill, 2014).

Evaluation design

We evaluated ACT using a partially block-randomized controlled trial (see Weisburd & Gill, 2014) in which 42 identified hot spots were matched into groups according to their crime rates and overall land use of the site (e.g., commercial, residential). The hot spots were then randomly allocated into treatment (ACT) or control (policing as usual) conditions within their matched groups. The block randomized design improves statistical power (see below) and increases equivalence between the treatment and control conditions (Gill & Weisburd, 2013; Weisburd & Gill, 2014; Weisburd & Green, 1995).

Identification and selection of the hot spots. CEBCP researchers and BPPD's crime analyst collaborated on the initial assessment of Brooklyn Park's hot spots. We obtained three years of calls-for-service (CFS) and incident report data from BPPD for 2011 through 2013. These calls and incidents were geocoded by BPPD and tied to the adjoining street segment (defined as both block faces of a street from intersection to intersection). We excluded non-Part I and Part II crimes, traffic offenses, and other crime events that were not amenable to a community-oriented intervention, such as crimes against government officials and fleeing an officer. However, we did include some civil complaints that reflected social disorganization and informal social control, including disorder, neighbor disputes, fighting, animal complaints, and noise violations. Events occurring at intersections were tied to their adjoining street segments. We included residential, mixed-use residential and commercial street segments in the sampling frame, and some entirely commercial streets.[8] Based on the data, we decided that any street

[8] Commercial streets were included as long as there was a business community to work with (i.e., a block that contained a large national chain retailer where staff would not be able to work with police without permission from corporate headquarters was not included).

segment with more than 20 incidents per year for all three years examined would be considered for the final sample.

While many of the hot spots in Brooklyn Park could be seen as single street segments, others represented small clusters of segments where data and police experience suggested that the crime problem was linked across these places. In addition, there are a number of large apartment buildings in Brooklyn Park with internal "streets" that do not appear on maps. Crime events recorded in these buildings typically coded to the neighboring street segment, but did not necessarily occur on that segment (see Gill et al., 2017). To account for this, we used "polygons" in ArcGIS as well as street segments to identify hot spots in the city. We only included proximal hot spots when the BPPD believed they had their own behavioral setting and would not spatially confound one another (e.g., a gated complex).

Finally, we accounted for police operational constraints in our final selection. BPPD did not have the resources to implement a new program with a very large number of hot spots, so including all 85 eligible street segments in the study would not have been feasible. In addition, BPPD requested that treatment hot spots were selected in all four patrol districts. The reason for this request was two-fold. First, hot spots were more concentrated in the southern part of the city and the department did not have the resources to concentrate more patrol officers in that district. Second, the department was conscious of the political implications of focusing ACT only in certain parts of the city and felt that community members in other areas would want the program as well, even if those patrol areas were perceived as safer by comparison. There were multiple hot spots in the study sample within each patrol district, so we were able to address this issue by ensuring that sites in each district were distributed as evenly as possible across the blocks created for random assignment (see below).

We narrowed our final sample by reviewing each potential site that met the criteria above with BPPD's crime analyst and commanders. BPPD personnel provided important local context to help us decide whether the segment should be included in the sample. For example, street segments with only a large corporate business (e.g., Walmart) were excluded from consideration since it would not be amenable to intervention, and magnet phone locations from which 911 calls might be made about problems occurring elsewhere (such as the fire department or police station) were excluded. As noted above, BPPD personnel also advised whether adjacent hot spot segments should be combined into one site for the sample (i.e. a large apartment complex situated between, and accessible by, two separate street segments). Taking all these factors into account, we ended up with a final sample of 42 hot spots for random assignment (see Table 2; shown in Figure 1).

Matching and random assignment. As discussed above, we developed a block-randomized design for two reasons. First, block randomization can increase statistical power in small N experiments like ours (Gill & Weisburd, 2013; Weisburd & Gill, 2014). Second, block randomization can improve equivalence when cases are heterogeneous by matching cases with similar characteristics into groups and randomizing to treatment and control conditions within these groups. In our case, the final sample of hot spots differed substantially from each other in terms of land use (residential, commercial, or mixed) and crime rates. As Table 2 shows, the total number of recorded crime incidents in the three-year period from 2011 to 2013 ranged from as low as 68 in one hot spot to over 2,500 in another. We first organized the hot spots by land use type, which created three groups (commercial, mixed commercial/residential, and residential). The seven commercial and three mixed locations were grouped together to form Block 1. Following the methodology of the Jersey City Drug Market Analysis experiment (Weisburd &

Green, 1995), we then sorted the larger residential group by the number of Part I and Part II crimes in 2011-2013 and looked for natural breaks in the level of activity. This method identified an additional four statistical blocks: very high activity (Block 2, includes 2 out of the 42 locations), high activity (Block 3, 8 locations), medium activity (Block 4, 14 locations), and low activity (Block 5, 8 locations). We chose not to use police district as a blocking factor as it did not reduce the heterogeneity in the other factors and was therefore unnecessary (each blocking factor reduces the degrees of freedom and can limit statistical power; see Weisburd & Gill, 2014).

[INSERT TABLE 2]

Hot spots were organized in an Excel spreadsheet in their respective blocks. A member of the research team (Gill) assigned each hot spot a random number using Excel's inbuilt formula and then sorted hot spots by assigned random number within blocks, from smallest to largest. The first half of the sorted block was then allocated to the treatment group and the second half was allocated to control. These random assignment procedures were determined in advance of assigning the random numbers, and the researcher hid the columns showing the name or description of the hot spot to avoid seeing which sites fell into which group. To address the operational constraints discussed above, BPPD commanders provided the researcher with a target number of treatment hot spots for each patrol district (5 in the Central district, 4-5 in the East, 6-7 in the North West, and 5 in the South West). The researcher ran the random assignment sequence 10 times (concealing the prior results each time) and selected the result that most closely matched the resource needs of the department.[9] The selected result provided 5 treatment sites each in the Central and Eastern districts, 7 in the North West, and 4 in the South West.

[9] Treatment and control conditions are reasonably matched on crime levels in the three months before the intervention began (April-June 2015). The mean number of citizen-initiated CFS each month for treatment sites is

Statistical power

A key problem in a randomized study with a small number of units is statistical power. Such small studies are prone to bias in the direction of the null hypothesis, meaning that they are unlikely to show a treatment impact even if one existed in the population of interest. We were well-aware of this in the design of our study, and we want to examine this directly in our report to clarify the power levels for different outcomes in our experiment, and also to justify choices regarding the statistical tests employed.

Our study has the weakest power design in the case of crime outcomes. With 42 sites, a simple randomized design would produce a statistical power level of .35, assuming a standardized moderate effect (Cohen's *d*) of .50 and a two-tailed test of significance. Even if we establish a less strict significance level of .10, the power level is only .48. However, it is important to note that effect sizes for crime outcomes in hot spots policing studies have been closer to .20 than .50 (Braga et al., 2012; 2014). Using this effect size level, the statistical power of a simple randomized design is only .17.

Using block randomization, the expected power estimates are much higher because the blocking factor in this case—characteristics related to crime in the pre-experimental period—would be expected to be strongly related to the crime outcomes examined after the experiment. We used an intra-class correlation of .40 to estimate the power of the block randomized study (based on Gill & Weisburd, 2013) using *CRT Power* software (Borenstein, Hedges, & Rothstein, 2012). In this case, with a .10 significance level and a standardized effect of .50 (and assuming equal block size because of computation limitations), the power level of the study is about .69. However, assuming a .20 effect size, the power level of the study is only .22. This means that

12.2 (SD = 18.5) and the mean for the control sites is 11.4 (SD = 10.1). The mean number of crime incidents each month for the treatment sites is 4.3 (SD = 6.8) and the mean for the control sites is 4.1 (SD = 4.0).

even taking into account the block randomized design, the statistical power of the study was marginal for detecting moderate size effects on crime events, and very low for detecting small program impacts on crime. Because of the low statistical power of the study for detecting crime outcomes, we use a .10 significance level and a one tailed significance test. Our research question, accordingly, is whether the program reduced crime.

While the study could only include 42 sites, we were able to gain much higher levels of statistical power in assessing changes in citizen attitudes. We collected about 150 survey responses in each group during each wave. Our design includes three levels: block groups, sites within block groups, and subjects within sites within block groups. Again we assume an ICC of .40 for the block level. Importantly, we did not think that the subjects in the different clusters would differ tremendously one from another. These are all hot spots of crime, and we expect to have relatively disadvantaged residents living across the sites. Accordingly, we set the intraclass correlation for power estimates to a value of only .02. Assuming that value, our measures of collective efficacy, and other citizen outcomes meet a power level of .80 with a moderate size effect ($d = .50$). Even with a small to moderate effect size ($d = .35$) our power level is .53. Our use of repeated measures in the design should further increase this power level. Importantly, the statistical power of our study is much greater for measuring collective efficacy, cooperation with the police, and police legitimacy than for measuring changes in crime. Because of this, we use the standard .05 level of statistical significance and a two-tailed test for assessing impacts on citizen perceptions.

Data and Analytical Plan

Crime and disorder

Description of the data. Crime was measured via police calls-for-service (CFS) and crime incident data provided by the BPPD. The CFS only include citizen-generated calls related to crime and disorder. The crime incident data only include founded Part 1 and II crimes that are citizen-generated (we removed officer-initiated incidents and events that BPPD investigated and ultimately determined that there was insufficient evidence to conclude that the crime occurred). Geocoding hit rates of 97.2% and 99.1% were obtained for CFS and crime incidents respectively. These hit rates are well above the 85% suggested threshold for a minimal reliable geocoding rate (Ratcliffe, 2004).

Analysis of crime data. Our analytic approach follows that of our research design. We use an ANOVA (with Type III Sum of Squares) in which the CFS/incidents occurring during the experiment (July 2015-October 2016) are predicted by study condition, block, and crime during the pre-measurement period (July 2014-June 2015). The following model is estimated:

$$y_i = b_0 + b_1 Treatment_i + b_2 Block\ 1_i + \cdots + b_6 Block\ 5_i + b_7 Prior\ Crime_i + e_i,$$

where y_i represents crime CFS/incidents for the ith study site, b_0 is the intercept, b_{2-6} denote the estimated coefficients for the blocking variables, b_7 represents crime CFS/incidents during the pre-measurement period, and e_i is the error term. Table 3 provides the basic crime statistics for both our full sample of 42 sites, and the sample excluding block 2 (see above).[10] It is particularly

[10] Independent samples *t*-tests indicate no significant differences at baseline between study conditions for both CFS and crime incidents. $t(40) = -.442$, $p = .661$ and $t(40) = -.639$, $p = .527$, respectively.

important to examine the crime outcomes with and without block 2 because the two sites in this block included by far the largest number of crime events in the study.

[INSERT TABLE 3]

One concern in assessing crime outcomes in our study, as in other studies that seek to increase collaboration between the police and the public, is that the crime data would be influenced by increased reporting behavior of citizens in the treatment sites. Evidence of this relationship is provided in Table 4, which describes the number of assets identified in each treatment location, the percentage of assets who called the police during the experimental period, the number of times the police were called, and the percent of assets who had not called before this period. It is clear that assets were very active in calling the police during the experimental period, and many of them had not called the police in the past. More than 700 calls were made by assets during the implementation period, and on average more than a third of those assets had never called the police before. Such reporting most directly impacts citizen-initiated calls, which in turn impacts crime incidents indirectly as officers are dispatched to investigate the calls.

To examine this possible bias in our data we compared the relationship between citizen calls to the police and crime incidents before and during the treatment period. We call this "crime inflation" to recognize the possible inflationary impacts of the treatment itself on the crime outcome. We calculate this inflation factor by taking the ratio of calls to incidents in the pre-treatment as contrasted with treatment periods:

$$\textit{Crime Inflation} = (CFS_{during} - Incident_{during}) / (CFS_{pre} - Incident_{pre})$$

[INSERT TABLE 4]

It is important to note that we measure the pre-intervention period as the year before the intervention (July 2014-June 2015) but, as noted earlier, the intervention period was 16 months in total. This means that we would expect "call inflation" to be above 1 in both groups, since the intervention period is longer. When we compare the call inflation factor for the treatment and control groups, both groups have values above one as expected, but call inflation in the treatment condition was significantly higher than in the control condition (see Table 5; one-tailed $p = .018$ with the full sample, $p = .020$ with Block 2 excluded). The inflation factor was 1.67 for the treatment condition, and only 1.27 in the control group.[11] The control group inflation factor is about what you would expect given the larger number of months in the intervention period, as the ratio of 16 to 12 months is 1.33.

We think this finding is particularly important, though we recognize that it does not give us the "true" value of crime inflation that results from programs that encourage greater collaboration with the police. That value is clearly unknown. Our experimental data simply tell us that the treatment condition evidenced significantly higher numbers of crime calls than crime incidents compared to the control group in the experimental period. These results closely follow the logic model of the experiment, and especially the role of assets in increasing citizen reporting. The fact that we use experimental data to illustrate crime inflation provides in turn strong experimental support for the idea that the intervention increased the reporting of crime by citizens. Moreover, these findings raise important questions regarding the crime outcomes reported in prior studies of community policing, an issue that we discuss below. Later in the report, in assessing crime outcomes, we report outcomes using unadjusted and adjusted estimates based on the crime inflation measure.

[INSERT TABLE 5]

[11] The inflation factors were almost identical when we calculated them excluding Block 2 (1.67 in the treatment group and 1.26 in the control group), so we use the above inflation factor in all our analyses, whether or not Block 2 is included.

Survey of citizen perceptions

Description of the survey. We developed and administered a door-to-door questionnaire (at both treatment and control sites) to assess citizen perceptions of the police, collective efficacy, and feelings of safety. We conducted the baseline survey between March and June of 2015 (before the intervention began), and the follow-up survey was conducted between December 2016 and June 2017 (after the project ended).[12] The process for conducting the two waves of surveys closely followed the methods used by Weisburd, Cave, et al. (2018) in their study of health and crime at hot spots in Baltimore, MD. The local researchers, under direction from the local project coordinator, began by conducting a residential census of each hot spot to confirm that the lists of addresses (supplied by BPPD crime analysts from their geographic databases) were accurate at each location and to identify vacant properties. Properties that appeared to be vacant (for example, realtor signs or lockboxes were visible, the house was boarded up, major construction work was apparent) were excluded from the sampling frame. When needed, the project coordinator worked with BPPD's Crime Prevention Unit to reach out to apartment building management for permission to enter buildings; in most cases, the management also provided de-identified lists of occupied and vacant units so that vacant units could be excluded from the sample.

We determined that a goal of 7-10 surveys per site would provide adequate statistical power to detect reasonable estimates of attitudes at each site (see earlier). We sampled 2.5 times that number for initial contact (i.e., 25 addresses per hot spot). A member of the research team (Gill) drew the sample by assigning each address a random number in Excel using the random

[12] Local student researchers administered the survey. Several factors contributed to the much longer data collection period for the follow-up survey, including weather conditions during the winter, personnel challenges on the research team, and more difficulty in contacting residents on the first attempt compared to the baseline survey.

number formula and sorting from smallest to largest. The first 25 addresses in this sort were released. In streets with fewer than 25 addresses/households, all addresses were released. Some business locations, such as small strip malls, had fewer addresses than our target number of surveys (for example, one of our hot spots consisted of a street segment with just three businesses). In these cases, the researchers attempted to interview multiple employees within the same business. The response, contact, and cooperation rates reported below were calculated according to the number of households rather than the number of people approached/interviewed so that our overall rates are not skewed by individual sites in which rates exceeded 100% (for example, where 7 people were interviewed at 3 addresses). Since the unit of analysis was the hot spot rather than the individual, we decided not to resample the same households in the follow-up wave. A new sample of 25 households was drawn at each site. In sites with 25 or fewer households, all households were again released but we did not necessarily get surveys at the same houses or with the same people as the first wave.

Teams of at least two researchers, including a team leader, visited sites seven days a week between 11AM and 8PM. The researchers were hired from local colleges and universities and trained by project staff. Interviews were conducted face-to-face—the researchers knocked on the doors of sampled addresses (or entered businesses), identified whether an adult (at least 18 years-of-age) was available, and engaged potential respondents in a brief recruitment conversation. Interested participants reviewed a consent form with the researcher and the interview began if they gave consent. The consent form, recruitment process, and survey instrument were reviewed and approved by the George Mason University Institutional Review Board (IRB). All contacts (or lack thereof) completed surveys, refusals, break-offs, requests to come back later, and no response were recorded by the researchers on contact sheets. If there was no answer, the researchers would move on to the next released address and return to the households they could not contact later in the shift or on another day. Where possible, they also made appointments with residents who were interested but could not talk in the moment, and followed up at households where an adult was not present. Once at least seven surveys were completed at a given hot spot, or researchers had exhausted all options for obtaining further surveys (i.e. they had returned at least 3 times to the same address with no answer or a hard refusal), the researchers moved on to the next hot spot.

At baseline, we identified a total of 6,095 addresses (including vacant units) across the 42 hot spots. We released 949 addresses and conducted 316 surveys at 301 addresses. This gives an overall response rate (by address) of 33%. The contact rate was 52% and the cooperation rate was 61%. The maximum number of individual surveys conducted at a site was 11 and the minimum was 7. In the follow-up survey there was a slight increase in addresses to 6,105. We released 965 addresses and conducted 298 surveys at 292 addresses. This resulted in a slightly lower response rate of 30%. The contact rate was also lower at 45%; however, we obtained a higher cooperation rate than the baseline survey (67% compared to 61%), which can be attributed to an exceptionally persistent survey team leader. The maximum number of surveys conducted at any given site in the follow-up survey was 9, while the minimum was 7. Sample characteristics of survey respondents by wave and group are provided in Table 6. We found no significant differences between the treatment and control condition participants at baseline.

[INSERT TABLE 6]

Description of perception measures. While our survey included a large number of measures relating to collective efficacy, feelings of safety, and perceptions of the police, we report here the key outcomes that relate to the logic model of the program (see Table 7).

Increased cooperation and collaboration with the police and with other citizens were primary goals of the program. We assessed changes in these two measures with two yes/no questions, asked at baseline and in the follow-up survey. The first question asked whether the respondent (or members of the respondent's household) had gotten together with neighbors to do something about a problem or organize efforts to improve their block in the past year. The second asked whether the respondent had spoken to a police officer or crime prevention officer about a specific problem on their block or in their apartment building during that period. We want to stress that these measures reflect what might be termed "collective efficacy actions," since they represent social cohesion and a willingness to intervene in community problems. In the first case, citizens' participation in prevention efforts demonstrate their willingness to work together with neighbors, which is in turn a reflection of trust. The second assesses a willingness to enlist formal control agents. We also think it likely that efforts to improve the block would have been encouraged and in many cases organized through the ACT intervention in the treatment condition.

We also report findings from more general measures of collective efficacy, feelings of safety, concerns about crime and disorder, and police legitimacy. These measures are based on theoretically-constructed scales that follow prior work (Gill, Vitter, & Weisburd, 2016; Sampson et al., 1997; Uchida et al., 2013; Weisburd, White, et al., in progress). Individual items in each scale were measured on 4-point Likert scales assessing likelihood (for collective efficacy; 1 = very unlikely; 4 = very likely) or agreement (for the remaining scales; 1 = strongly disagree; 4 = strongly agree). Scale reliability was good to excellent overall (Cronbach's $\alpha > .80$). Descriptive statistics for each of these scales are presented in Table 7, and full details about the items included in each scale are included in Appendix A, Tables A1-A4.

Collective efficacy is a central measure for our program model, as officers were expected to work to increase collective efficacy at the treatment hot spots. We measured collective efficacy on a 6-item scale assessing the perceived likelihood that people on the respondent's block or apartment complex would intervene in various situations involving juvenile disorder or the closure of community resources (α = .893; Table A1). This measure differs from the direct measures of collaboration and participation in crime prevention noted above.

More generally, if the ACT program achieved its crime reduction goals, we would also expect to see improvements in residents' feelings of safety and reduced concerns about crime and disorder. We measured feelings of safety using a 6-item scale assessing whether respondents felt safe in different situations, including at home, at work, and on the street during the day and night (α = .851; Table A2). "Concerns about crime and disorder" was a 17-item scale measuring the extent to which respondents were worried about certain types of crime (e.g. gangs, drugs, and various types of victimization such as burglary and robbery), and whether they thought disorder issues such as graffiti, vacant lots, and signs of drug or alcohol use were problems on the block (α = .913; Table A3).

Finally, we measured police legitimacy using 7 items examining whether respondents thought the police could be trusted, treated people fairly and with respect, and so on (α = .918; Table A4). While officers were not specifically trained in the procedural justice model, elements of the model were seen as part of developing positive collaborations with the public. Accordingly, we might expect to see increases in citizens' perceptions of police legitimacy in the treatment condition.

[INSERT TABLE 7]

Analysis of citizen perception data. We estimated the effects of the intervention on our scaled survey outcomes using mixed-effects linear regression models with robust standard errors to account for the nesting of responses within hot spots. The model can be expressed as follows:

$$y_j = b_0 + b_1 Wave + b_2 Treatment + b_3 (Wave \times Treatment) + b_{4-7} Block + r_j + e_i$$

The treatment effect of interest in this model is the interaction of *Wave* (wave 1/wave 2) and *Treatment* (treatment/control), which estimates the difference-in-differences between the treatment and control groups between Wave 1 and Wave 2. The model also includes fixed effects for the statistical block (b_{2-6}), a random effect for hot spot (r_j), and the error term (e_i). We did not account for nesting of individuals within addresses within hot spots because only a very small proportion of individuals were interviewed at both waves and/or nested within addresses.[13] In these analyses we report the outcomes for the full sample only.[14]

Our two questions on "collective efficacy actions" had dichotomous outcomes. We assessed involvement in problem-solving using mixed-effects logistic regression, including a random effect for hot spot, as before. However, the inclusion of the random effect made the model for speaking with the police about a problem unstable, so we used a simple one-level logistic regression with robust standard errors to account for the clustering.

[13] We drew a new sample of addresses for the follow-up survey, but six addresses were surveyed in both Wave 1 and Wave 2. Seventeen respondents in the follow-up survey said they remembered taking the survey before, although we cannot verify they were correctly remembering our survey. In most cases we only conducted one survey per address; however, a few hot spots had fewer than seven addresses on the street and at others multiple people were willing to take the survey (this was usually the case at business addresses where a number of employees were working). Only 24 individuals (7.6% of all individuals surveyed) were nested in addresses at Wave 1 and 11 (3.7%) at Wave 2. As a sensitivity analysis, we also ran the models including random effects for individuals and addresses. Due to the lack of variability, the random effect for individuals created instability in the models. The random effect for address also did not contribute to several of the models, but where we were able to include it the results were very similar to the models presented here.

[14] We did not expect the survey results to be influenced by the exclusion of Block 2. Accordingly, we only report the analysis of the full sample. We present the descriptive statistics and outcomes for the survey, excluding respondents in the Block 2 sites, in Appendix B. Those analyses do not indicate any changes in the interpretation of our findings.

Results

Impact on crime

Table 8 shows the ANOVA models for the unadjusted impact of the intervention on crime incidents, including and excluding Block 2. For this analysis we logged (using the natural log) the dependent variable and pre-intervention crime covariate because of the skew of the count data. We do not, as expected given the crime inflation findings reported earlier, find a statistically significant crime prevention outcome (at the .10 level, using a one tailed test, as noted earlier), whether we examine the full sample, or the sample excluding Block 2. However, if we adjust crime incidents using the call inflation measure described above, our results do suggest crime prevention benefits (Table 9). We decided to use a global approach to this correction because we view this analysis as speculative. In some sense we are asking the question: If the crime incidents have been biased upwards in the treatment group by increased collaboration and especially reporting behavior to the police, what would our estimate of crime incidents be, correcting for this general trend? We assess this by multiplying each treatment site score by .76, representing a downward correction for the treatment sites based on the division of the crime inflation measure for control divided by treatment (1.27/1.67). As before, we logged the outcome measure before running the models.[15]

Looking first at the model with all of the cases in Table 9, we find that the treatment effect is statistically significant at the .10 level using a one-tailed test (p = .055). In this case, the treatment group had a relative average decrease per site in the period during the experiment of 4 events, while the control group had an increase of 13 events. Excluding Block 2, the results are

[15] Because of the very high correlation between block and logged pre-intervention crime, we also ran the models for the adjusted crime outcomes with block or logged pre-intervention crime excluded. For the model with block excluded, the observed one-tailed p-value was .047 for the full sample. The p-value excluding pre-intervention crime was .099, still significant at the .10 level.

very similar, with a *p*-value of .053. The treatment group has an estimated average reduction of 3 events per site, while the control group had an average increase of 13 events. We recognize that these findings are speculative, but they do suggest program impacts for crime outcomes, if our measure of crime inflation is capturing the bias that comes from increased reporting behavior.

[INSERT TABLES 8 AND 9]

Impact on collaboration and cooperation, collective efficacy and other citizen perceptions

As we noted earlier, we have two measures that directly reflect the willingness of citizens to participate in solving problems. The first is whether respondents were involved in problem-solving with neighbors in the past year. The second asks whether the citizen spoke to a police officer about a problem in the past year. We see these as reflecting measurement of collective efficacy behaviors or "actions." The treatment-by-wave interaction effect is positive in direction and statistically significant at the .05 level for both of these measures, showing that the treatment did have the desired effects of increasing collaboration with police and other citizens (see Table 10). Looking at the marginal means of differences between the two survey waves, we can see that these effects are meaningful (Figure 2). While self-reports of speaking to a police officer decline in the control condition, they increase in the treatment group. The relative reporting of involvement in problem solving activities also is much larger for the treatment group. While there is sharp decline in involvement in the control group, there is overall stability for the treatment group. This finding points to the importance of using an experimental design, since it allows us to see the comparative impact of treatment rather than simply the pre-post effects within each group. Together, these findings support the position that collective efficacy actions were positively impacted by the treatment—a key goal of the experiment.

[INSERT TABLE 10 AND FIGURE 2]

However, the positive effects of the intervention do not hold for traditional measures of collective efficacy or perceptions of police legitimacy. Collective efficacy increased in both the treatment and control groups between the baseline and follow-up surveys. As reflected by the marginal means (see Figure 3), the trend lines are very similar and there is not a significant difference between the groups as reflected in the treatment-by-wave interaction term (see Table 11). We also found no statistically significant effects on police legitimacy (Table 11). Both treatment and control conditions showed increasing levels of legitimacy, although the change was larger in the treatment condition (Figure 3).

[INSERT TABLE 11 AND FIGURE 3]

The impact of the program on citizens' concerns about crime and disorder and feelings of safety also did not meet our .05 significance threshold (Table 11). Both groups show decreases in concerns about crime and disorder, though surprisingly the decline is greater in the control group (Figure 3). Feelings of safety followed a similar trend: respondents in both groups felt safer in the second wave (Figure 3), but the treatment-by-wave interaction term is negative and statistically significant at the .001 level, indicating that treatment group respondents felt less safe than those in the control group after the implementation of ACT. We return to this issue in greater detail in our discussion of these findings.

Discussion

The ACT program in Brooklyn Park set out to achieve three goals. The first was to show that the patrol force could be used to carry out specialized hot spots policing efforts. In the past, when activities beyond increasing patrol or police presence have been developed for hot spots policing programs—particularly those involving collaborative problem-solving—specialized units have been employed. Our second goal was to show that we could increase cooperation and collaboration with the police and collective efficacy among the communities that were targeted. We also hoped that these collaborative activities would lead to increased perceptions of police legitimacy. Finally, we sought through these activities to decrease both actual and perceived crime and disorder levels at crime hot spots. We review what we have learned about each of these goals through our evaluation.

Our project expands the role of ordinary patrol officers by giving them responsibility for carrying out crime prevention efforts at crime hot spots. An immediate objection to this expansion of the police role is that the approach may be unrealistic at a time when most police agencies in the United States are facing budget constraints and contracting for services as a means of improving efficiency while reducing costs. In turn, asking patrol officers to carry out specialized efforts such as community organizing and problem-solving might be seen as asking too much from rank and file patrol officers.

We think that the “financial objection” to our approach is wrong in good part because of the structure of police patrol in the United States. ACT sought to leverage patrol officers’ discretionary, or uncommitted, time outside of responding to calls and incidents for building connectivity and trust with and among hot spot residents. Research suggests that discretionary time can constitute as much as 50 percent of an officer’s shift (Famega, 2009); in Brooklyn Park it is approximately 32 percent of a 12-hour shift. Specialized police units are usually assigned to community- or problem-oriented police work, but are typically cut when departments experience fiscal constraints. Our study shows that patrol officers can support specialized functions as part of their regular work through the modified problem-solving approach inherent in ACT, just as many hot spots policing programs were created from the reallocation of existing patrol resources (Sherman & Weisburd, 1995; Telep et al., 2014). The most abundant resource for any police department is the discretionary time of the patrol officer. We think that it is particularly timely in this context to utilize patrol officers for innovative policing programs.

Overall, our study findings support the idea that patrol resources can be used for programs of this type. The SharePoint data indicating police activities during the course of the program clearly show that patrol officers were carrying out the expected activities of ACT in the treatment areas. They contacted hundreds of assets, and organized meetings across the sites with the goal of "coming together" with the community to solve problems. At the same time, the police commander who oversaw the program did observe that some officers found it more difficult to carry out ACT than others. Supervision in this program was direct and ongoing, aided by the SharePoint database. A program without this level of supervision and without such quantitative measurement on a timely basis might not have yielded these encouraging results.

When we turn to the effects of the program on collective efficacy, we find contradictory outcomes depending on whether we examine actions that reflect collective efficacy or traditional measurements of citizens' perceptions of collective efficacy. We have two primary measures of what we have termed 'collective efficacy actions': whether survey respondents spoke to a police officer about a problem, and whether they were involved in problem solving efforts with their neighbors. Both of these measures show that citizens were more likely to intervene in community problems in the treatment group. In this sense, the measures reflect efforts to exert informal social controls. This is certainly the case with being involved in problem solving activities with one's neighbors, which also reflects to some degree the trust needed to intervene as well as the action of intervention itself. But even in the case of speaking to the police about a problem, the action of reaching out to the police reflects a willingness of the citizen to be involved in solving community problems—a key element of collective efficacy. These findings are also reflected in our measure of call inflation. Treatment location residents appear to be more likely than their counterparts in control areas to call the police about problems.

We think the survey results are particularly important because they were based on a general sample of residents at the hot spots, not just the assets identified by police who participated in ACT. On many of the hot spot streets there were scores or even hundreds of apartments, and clearly many citizens did not become involved with the program. The fact that we observe general effects here suggests that the community engagement and collective efficacy-building parts of this program were well implemented and had impact. Nonetheless, we did not observe significant impacts using traditional measures of collective efficacy: there was no increase in the treatment hot spots relative to the control locations. How can we understand these seemingly contradictory findings?

It is important to note that that our measures of collective efficacy actions—calling the police and participating in problem solving with neighbors—are directly related to the activities of the program. The general measure of collective efficacy that we drew from prior studies asks whether citizens would be willing to intervene in a broad range of problems, from juvenile delinquency on their block to issues unrelated to crime and justice such as stopping the closure of community facilities. Perhaps the specific focus of our program increased collective efficacy relating directly to the crime problems on the street, but not to the more general problems that these communities faced. This suggests that police researchers should measure collective efficacy not only in terms of the broad measures that are traditionally used to assess collective efficacy in the community, but in terms of the specific collective efficacy actions that are likely to be influenced directly by interventions.

A recent National Academy of Sciences report on proactive policing has noted more generally the failure of the police interventions to impact community outcomes in short time periods:

> In addition to enhancing perceived police legitimacy, an important goal of community-oriented policing is to build, improve, or sustain communities. Such transformations rarely take place in the span of months or even a few years. Yet most studies of community-oriented policing's effects (and associations with outcomes) use a time frame that is short-term: a year or less. (Weisburd & Majmundar, 2017, p. 222)

We think that this concern is relevant to our study as well. Perhaps collective efficacy in its general form is likely to emerge over long periods of time, not within the 18-month period that we measured in this program. Nonetheless, our survey findings indicating increased participation in problem-solving among neighbors in the treatment hot spots, which suggests that while there was no statistically significant change in general measures of collective efficacy in these locations, willingness to intervene was beginning to develop in the specific areas examined by the program.

We think this perspective also helps us to understand the failure of the program to impact police legitimacy. Scholars who have examined the impact of officer behavior on police legitimacy argue that the immediate interactions between citizens and the police have the most salience (e.g. Tyler, Goff, & MacCoun, 2015). This perspective suggests that the key influence on perceptions of the police are the visceral and recent contacts that citizens or the wider community have had with police in recent years. If this were the case, we would predict that the ACT program, with its emphasis on positive communications with the public and procedurally just approaches, should have had significant influence on citizens in the experimental sites—especially since citizens clearly experienced more interaction with the police.

However, this assumption has come under increasing criticism in recent years. Nagin and Telep (2017, p. 7) argue in a review of the research on police legitimacy that “evidence of exogenous manipulations affecting citizens’ perceptions and behavior is in short supply,” and that research has failed to examine the broader social context of people’s lives and how this shapes perceptions. They note that the impacts of recent experiences with the police may be greatly outweighed by the “accumulation of a lifetime of cultural, community, and familial influences” (Nagin & Telep, 2017, p. 7; see also Thacher, forthcoming; Weisburd, White, & Wire, forthcoming). This is particularly important at crime hot spots, where citizens often belong to minority and disadvantaged communities that may have a long history of negative experiences with the police. The recent National Academy of Sciences report on proactive policing notes in this regard:

> In sum, while there have been important changes in the scope for racial bias and animus in policing, with respect to the impact of proactive policing on racial bias and disparate outcomes, law enforcement in the United States does not start with a clean slate. As noted by Chief Terrence M. Cunningham in his presidential address to the International Association of Chiefs of Police, “this dark side of our shared history has created a generational—almost inherited—mistrust between many non-White communities and the law enforcement agencies that serve them.” (Weisburd & Majmundar, 2017)

In this context, we do not think our failure to find positive impacts on police legitimacy is surprising. Like collective efficacy, individual and community-level attitudes toward the police may be built over long periods of time, and are influenced by experiences well beyond the specific ACT program. It may be that there will be long-term influences on perceptions of police legitimacy from a program like ours. We think that such long term outcomes are important to examine. But given the weight of evidence more generally, it would seem overly optimistic to believe that short-term changes in policing will have large influences on public attitudes toward police legitimacy in the short-term.

Crime prevention was also a key goal of this study. Our findings here point to the difficulties of measuring crime outcomes in a study of this type. We find that the program led to increased cooperation with the police, and through those activities increased the likelihood of calls to the police in the treatment as contrasted with the control condition. Of course, calls to the police are a key mechanism for the identification of crime. We called this “crime inflation” in our report, and we showed that there were strong and significant impacts of the intervention on crime inflation. When we examined crime incidents per se, we did not find evidence of a crime prevention impact of the program. But when we accounted for this crime inflation, we found indications that the program did reduce crime in the treatment areas. We think these findings are speculative, but they have importance for the evaluation literature on programs that seek to increase community collaboration and cooperation. Arguably, we have strong evidence that there is crime inflation because of the program, and when such inflation is taken into account we observe crime prevention outcomes. Of course, we do not know that the crime inflation we observe captures the true correction for this process. We think however, there is strong reason to believe that a correction is warranted.

Our approach to the problem of crime in this study has important implications for both existing evaluations of community policing and future evaluations of ACT in other settings. The recent National Academy of Sciences Panel on proactive policing concludes there is little evidence that community policing affects crime (Weisburd & Majmundar, 2017; see also Gill et al., 2014). A possible explanation for this finding is that there are very few long-term evaluations of community policing. But our data suggest another explanation, which emerges because of the strong success of our program in increasing collaboration and cooperation with the public. Traditional official crime statistics are likely to be confounded measures of crime prevention in these types of programs because of the impact of increased collaboration on citizen's reporting behavior. It may be necessary to consider alternative measurement schemes. We have suggested a correction method that uses the inflation in crime calls relative to crime incidents to overcome such biases. Whatever the approach, programs that influence cooperative behavior on the part of citizens must be assessed in terms of crime prevention outcomes by measures that are not reactive to the program. Simply put, we cannot use traditional uncorrected crime measures if those measures are responsive to both possible crime prevention outcomes and possible crime inflation outcomes. The end result will be no effect observed. That is the present state of the knowledge about community policing, and our report suggests that it should be critically examined.

Finally, we have the seeming contradiction between the possible crime prevention outcomes in the treatment condition, and our finding that feelings of safety declined among survey respondents in the treatment condition as compared to the control condition. The success of the program in getting police to the hot spots, and in their efforts to be visible to interact with the community, may have had the unintended negative impact of increasing fear of crime among residents. Especially for those citizens who were not directly identified as "assets," seeing police interacting with citizens regularly on their block could have led to the impression that crime was increasing, or more generally that there were a lot of crime problems in their area. It would be difficult to undo this impression in such programs more generally, because the increased police attention is due to heightened problems at those places. Nonetheless, our findings suggest that when visiting hot spots to engage citizens, police should emphasize that they are not there because of specific crime events, but are trying to work with citizens to reduce crime problems at those places.

Before concluding, we think it important to recognize the relatively small number of sites in our experiment, and the implications of this design limitation to the interpretation of our findings. We have emphasized earlier the tentative nature of our findings regarding crime. We think our study provides important new insights, but these need to be examined in larger experimental studies with greater precision in identifying impacts. Our survey provides more confidence because of the increased statistical power created by having multiple subjects at each site. But even here, it is important to emphasize that we are looking at only a relatively small number of sites in a specific city. More research in a jurisdiction with a larger sample of hot spots is needed.

A further limitation of our study is that we did not examine displacement of crime or diffusion of benefits. We considered assessing these effects, but our findings about crime inflation and its implications for measuring crime outcomes led us to view such analyses as likely to have considerable shortcomings. Moreover, because of the layout and clustering of hot spots in Brooklyn Park, it would have been difficult to identify uncontaminated treatment and control areas for examination. Given that displacement is uncommon even in deterrence-based interventions, we believe it is unlikely to occur as a result of a problem-solving and community-building approach that may not be immediately noticeable at the street level. Nonetheless, there may have been a diffusion of benefits to neighboring streets or buildings, particularly around treatment hot spots where community events and other highly visible activities were conducted that may have attracted the attention and even participation of other nearby residents.

Conclusions

Our evaluation of the ACT program in Brooklyn Park has provided important new information about policing crime hot spots. First, we have shown that the unallocated time of patrol officers can be used successfully to develop a concentrated community based problem solving effort at crime hot spots. Our experiment shows clearly that the patrol officers did carry out the intervention successfully. Our study also shows that the program can influence the willingness of citizens to intervene with police and their neighbors, which we have called 'collective efficacy actions.' This is reflected in the level of interaction we observe with assets, or identified partners, in the program, and in findings in our survey concerning involvement with the police and in problem solving with other community members. After correction for what we have termed *call inflation*, we also find significant impacts on crime. While tentative, these findings suggest that the ACT model should be tested in other settings.

While we have strong evidence of collective efficacy actions in areas directly related to the program, this does not lead to either increases in more general assessments of collective efficacy or improved perceptions of police legitimacy. We argue that ACT's failure to impact these measures likely results from the short-term assessments in the program, and the reality that such perceptions are affected by many factors outside of policing. We think that longer term evaluations are needed to identify the effects of such outcomes. Program developers and researchers need to recognize the difficulty of changing such attitudes not only in the short run, but also in the long run, with specific policing programs.

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Tables

Table 1

Number and Type of Activities Recorded by Police, by Month and Implementation Stage

Activity Type	2015						2016										Total	Total
	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sept	Oct	N	%
Stage 1: Asset & Liability Identification																		
Contact with Resident or Asset	61	32	1	1	1	1	1				2				1	2	103	18.4
Data Collection/Office Work	47	17	3	1													68	12.1
Extra Patrol	138	131	9	1										1			280	49.9
Foot Patrol	39	28															67	11.9
Meet and Greet	24	5															29	5.2
Neighborhood Meeting	1	2			1												4	0.7
Pot Luck or Fun Activity	3	1															4	0.7
Response to Crime/Enforcement	5	1															6	1.1
Stage 1 Total	*318*	*217*	*13*	*3*	*2*	*1*	*1*				*2*			*1*	*1*	*2*	*561*	*100.0*
Stage 2: Coming Together																		
Contact with Resident or Asset		8	33	37	145	95	67	6	1							2	394	36.6
Data Collection/Office Work		5	20	17	33	25	13	1									114	10.6
Extra Patrol		21	155	94	36	26	38	2	3					1			376	34.9
Foot Patrol		4	32	26	6	14	9				1						92	8.6
Meet and Greet		1	8	6	13	8	13	1									50	4.6
Neighborhood Meeting			1	9	5	11	11										37	3.4
Phone Call					1	1											2	0.2
Pot Luck or Fun Activity				2													2	0.2
Response to Crime/Enforcement				1	2			1									4	0.4
Survey/Community Training				2	3												5	0.5
Stage 2 Total		*39*	*249*	*194*	*244*	*180*	*151*	*11*	*4*		*1*			*1*		*2*	*1,076*	*100.0*
Stage 3: Taking Action																		
Contact with Resident or Asset							20	117	91	56	72	74	94	66	49	37	676	37.7
Data Collection/Office Work							4	41	21	10	12	2	6	4	9	4	113	6.3
Extra Patrol							18	102	113	57	117	95	77	89	93	35	796	44.3
Foot Patrol							3	24	13	11	18	13	7	14	6	5	114	6.4
Meet and Greet							1	6	7	5	8	8		2	3	2	42	2.3
Neighborhood Meeting							1	2	3	5	6		8	1	2	1	29	1.6
Pot Luck or Fun Activity											2	2	2	7	2		15	0.8
Response to Crime/Enforcement								4		2			1	1	1		9	0.5
Survey/Community Training													1				1	0.1
Stage 3 Total							*47*	*296*	*248*	*146*	*235*	*194*	*196*	*184*	*165*	*84*	*1,795*	*100.0*
Grand Total	**318**	**256**	**262**	**197**	**246**	**181**	**199**	**307**	**252**	**146**	**238**	**194**	**196**	**186**	**166**	**88**	**3,432**	

Table 2

Description of Study Sites by Block

	Block									
	1		2		3		4		5	
N of Hot Spots		10		2		8		14		8
Unit of Analysis[a] (N [%])										
Segment	7	(70.0)	-	(-)	-	(-)	3	(21.4)	4	(50.0)
Polygon	3	(30.0)	2	(100.0)	8	(100.0)	11	(78.6)	4	(50.0)
Land Use Type (N [%])										
Commercial	7	(70.0)	-	(-)	-	(-)	-	(-)	-	(-)
Residential	-	(-)	2	(100.0)	8	(100.0)	14	(100.0)	8	(100.0)
Mixed	3	(30.0)	-	(-)	-	(-)	-	(-)	-	(-)
Police District[b] (N [%])										
51CC	2	(20.0)	1	(50.0)	2	(25.0)	4	(28.6)	1	(12.5)
51ES	3	(30.0)	-	(-)	-	(-)	2	(14.3)	3	(37.5)
51NW	3	(30.0)	-	(-)	3	(37.5)	5	(35.7)	3	(37.5)
51SW	2	(20.0)	1	(50.0)	3	(37.5)	3	(21.4)	1	(12.5)
Crime Incidents[c]										
Mean (SD)	181.7	(66.3)	1,748.5	(1,202.8)	589.6	(152.8)	235.4	(53.6)	96.8	(16.4)
Range		93–297		898–2,599		420–754		167–362		68–118

[a] "Segments" include single street segments (the portion of a street block from intersection to intersection) or a cluster of segments. "Polygons" are groupings of buildings or a large building (e.g. apartment complex, strip mall).
[b] For geographic reference, police districts are labeled in Figure 1.
[c] 2011–2013. We excluded crimes that we did not believe could be affected by the intervention (e.g. forgery, DUI).

Table 3

Descriptive Crime Statistics Before (July 2014–June 2015) and During the Experiment (July 2015–October 2016)

	Full Sample (N = 42)				Excluding Block 2 (N = 40)			
	Before		During		Before		During	
	Mean	(SD)	Mean	(SD)	Mean	(SD)	Mean	(SD)
Crime Outcome								
Calls for Service								
Control	122.3	(106.2)	157.1	(139.8)	107.0	(81.6)	138.3	(112.7)
Treatment	146.3	(224.4)	207.0	(328.4)	101.6	(93.6)	140.8	(128.5)
Crime Incidents								
Control	44.5	(41.1)	56.5	(51.8)	38.8	(32.5)	50.9	(46.3)
Treatment	58.2	(89.7)	71.1	(110.8)	40.6	(39.5)	48.9	(44.5)

Table 4

Assets who Assisted the Police in 2015 and Through October 31, 2016

Treatment site ID[a]	N of assets	% of assets who called police during this period	N of calls by assets or their family members during this period	% of assets who called during this period but had never called BPPD before
1736	7	42	3	75
poly27	11	27	20	33
1423	11	9	9	0
poly19	12	23	3	33
poly24	13	50	35	50
1502	13	18	76	33
1616	13	23	4	66
911	14	56	50	56
poly12	15	53	35	38
poly18	16	40	20	33
1116	18	83	43	28
1575	18	47	22	13
poly23	20	75	61	47
poly16	23	42	14	40
13	23	52	35	42
poly21	24	66	107	33
poly0	25	36	13	55
poly26	25	54	35	77
poly8	26	50	61	50
poly13	38	63	173	50
poly5	40	51	55	15

[a] Due to the small numbers of survey respondents at each treatment site, we use our project identification numbers for each treatment location here rather than the name of the street or apartment building to preserve confidentiality.

Table 5

Univariate ANOVA for the Crime Inflation Factor

	Crime Inflation Factor					
	Full Sample			Excluding Block 2		
	F	df	*p*	*F*	df	*p*
Fixed Factors						
Intercept	165.12	1, 36	< .001***	223.30	1, 35	< .001***
Study Condition	4.74	1, 36	.018*	4.17	1, 35	.020*
Block	.19	4, 36	.470	.21	3, 35	.430

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. p-values are one-tailed.

Table 6

Characteristics of survey participants by wave and by group at baseline

	Full Sample		Treatment	Control
	Wave 1	Wave 2	at Wave 1	at Wave 1
Survey setting (%)				
House/row house/townhouse	34.5	37.4	33.8	35.3
Apartment complex or development	47.7	45.6	50.0	45.3
Business	17.7	17.0	16.2	19.3
Gender (%)				
Female	57.5	58.9	58.5	56.3
Male	42.5	41.1	41.5	43.7
Age (%)				
18-25	20.4	22.8	23.2	17.3
26-35	28.3	24.8	24.4	32.7
36-45	22.6	22.5	26.2	18.7
46-55	15.0	13.4	13.4	16.7
56-65	7.3	9.1	7.3	7.3
66-75	5.1	6.0	4.3	6.0
Over 75	1.3	1.3	1.2	1.3
Race/ethnicity (%)				
Black/African-American	38.3	41.4	40.5	36.0
African immigrant/refugee	12.5	22.6	14.1	10.7
White	23.0	22.2	19.0	27.3
Asian	8.3	8.4	9.2	7.3
Hispanic	8.0	3.0	8.0	8.0
Other	4.5	1.0	2.5	6.7
More than one race	5.4	1.3	6.7	4.0
Born in United States (%)	60.2	76.2	60.4	60.0
Has children under 18 (%)	46.7	36.6	48.8	44.4
Education (%)				
Some middle/high school	10.2	4.0	12.8	7.4
High school diploma/GED	24.0	18.5	20.7	27.5
Some college	26.2	29.6	27.4	24.8
Associate's degree	14.7	20.5	12.8	16.8
Bachelor's degree	18.8	23.2	20.1	17.4
Masters/graduate/professional degree	6.1	4.0	6.1	6.0

Characteristics of survey participants by wave and by group at baseline (continued)

	Full Sample		Treatment	Control
	Wave 1	Wave 2	at Wave 1	at Wave 1
Employment (%)				
Full-time	58.8	66.4	59.1	58.4
Part-time	17.9	12.8	20.7	14.8
Not working/not looking	4.5	5.0	4.9	4.0
Not working/looking	9.9	2.0	8.5	11.4
Retired	5.4	8.1	4.9	6.0
Other	3.5	5.7	1.8	5.4
Currently in school (%)				
Full-time	12.4	8.8	13.4	11.3
Part-time	13.1	16.2	11.6	14.7
Owns home (%)	23.9	16.2	21.2	27.1
Main activity at hot spot (%)				
Live	80.0	80.5	82.9	76.8
Work	15.9	15.8	14.0	17.9
Own a business	1.6	3.4	.6	2.6
Own property/land	.3	.0	.0	.7
Use local resources	.3	.0	.0	.7
Other	1.9	.3	2.4	1.3
Duration of main activity (%)				
Less than 1 year	31.2	18.1	30.5	32.0
1 year or more, but less than 5 years	41.7	38.9	43.9	39.3
5 years or more, but less than 10 years	13.7	30.5	14.0	13.3
10 years or more	13.4	12.4	11.6	15.3
Hours per day spent at hot spot (%)				
Less than 1 hour	1.3	.0	.6	2.0
1 hour or more, but less than 8 hours	20.6	22.3	22.6	18.5
8 hours or more, but less than 12 hours	38.4	49.0	40.9	35.8
12 hours or more	39.7	28.7	36.0	43.7

Note: There were no significant differences between treatment and control group participants at baseline.

Table 7

Descriptive statistics for survey outcomes

	Scale α	(Items)	Wave 1			Wave 2		
			N	Mean	(SD)	N	Mean	(SD)
Involved in problem-solving with neighbors in past year[a]	-	-	312	.26	(.44)	296	.15	(.35)
Spoken with police about problem in past year[a]	-	-	311	.37	(.48)	298	.33	(.47)
Collective efficacy[b]	.893	(6)	313	2.85	(.60)	298	3.19	(.67)
Feelings of safety[c]	.851	(6)	315	3.10	(.44)	298	3.32	(.54)
Concerns about crime and disorder[c]	.913	(17)	315	2.32	(.45)	298	2.12	(.45)
Police legitimacy[c]	.918	(7)	313	2.80	(.60)	296	2.90	(.52)

[a] Outcomes based on a binary measure (1 = yes, 0 = no)
[b] Outcomes based on a 4-point likelihood scale (1 = very unlikely, 4 = very likely)
[c] Outcomes based on a 4-point agreement scale (1 = strongly disagree, 4 = strongly agree)

Table 8

Unadjusted univariate ANOVA for Crime Incidents

	Crime Incidents					
	Full Sample			Excluding Block 2		
	F	df	*p*	*F*	df	*p*
Fixed Factors						
Intercept	1.086	1, 35	.153	1.312	1, 34	.130
Study Condition	.000	1, 35	.493	.004	1, 34	.474
Block	.048	4, 35	.498	.038	3, 34	.495
Covariates						
Pre-Intervention Crime Incidents	35.991	1, 35	< .001***	32.839	1, 34	< .001***

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. *p*-values are one-tailed.
Note: The dependent variable and covariate are the natural log of crime incidents.

Table 9

Univariate ANOVA for Crime Incidents Adjusted by the Crime Inflation Factor

	Crime Incidents					
	Full Sample			Excluding Block 2		
	F	df	*p*	*F*	df	*p*
Fixed Factors						
Intercept	.640	1, 35	.215	.772	1, 34	.193
Study Condition	2.683	1, 35	.055†	2.753	1, 34	.053†
Block	.048	4, 35	.498	.038	3, 34	.495
Covariates						
Pre-Intervention Crime Incidents	35.991	1, 35	< .001***	32.839	1, 34	< .001***

† $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. *p*-values are one-tailed.
Note: The dependent variable and covariate are the natural log of crime incidents.

Table 10

Impact of the Intervention on Community Collaboration

	Involved in Problem-Solving[a]		Spoken with Police about Problem[b]	
Fixed effects	*b*	(Robust SE)	*b*	(Robust SE)
Wave 2	-1.440***	(.411)	-.609*	(.248)
Treatment	-.694*	(.303)	-.505*	(.242)
Wave 2 × Treatment	1.268*	(.553)	.810*	(.351)
Block (Ref = Block 4)				
1	.778	(.438)	1.036***	(.231)
2	-.171	(.390)	-.064	(.476)
3	1.000*	(.425)	.428	(.252)
5	.953*	(.386)	.334	(.258)
Constant	-1.341***	(.406)	-.694**	(.212)
Random effects	σ	(Robust SE)	σ	(Robust SE)
Hot spot	.239	(.160)	-	-
Log pseudolikelihood	-288.122	-	-378.305	-
Pseudo R^2	-		.037	
Wald χ^2	36.219***		27.684***	
N	608		609	

[a] Multilevel mixed effects logistic regression with robust standard error
[b] One-level logistic regression with robust standard error
* $p < .05$, ** $p < .01$, *** $p < .001$. p-values are two-tailed.

Table 11

Impact of the Intervention on Collective Efficacy, Police Legitimacy, Feelings of Safety, and Concerns about Crime and Disorder

	Collective Efficacy		Police Legitimacy		Feelings of Safety		Concerns about Crime and Disorder	
Fixed effects	*b*	(Robust SE)	*b*	(Robust SE)	*b*	(Robust SE)	*b*	(Robust SE)
Wave 2	.403***	(.070)	.062	(.085)	.351***	(.055)	-.274***	(.050)
Treatment	-.033	(.082)	-.042	(.093)	.092	(.057)	-.055	(.055)
Wave 2 × Treatment	-.126	(.099)	.058	(.114)	-.257***	(.076)	.138	(.070)
Block (Ref = Block 4)								
1	-.004	(.088)	.083	(.068)	-.075	(.057)	.134*	(.058)
2	-.271	(.163)	-.201**	(.073)	-.268*	(.106)	.280**	(.107)
3	-.192*	(.095)	-.006	(.108)	-.197**	(.061)	.220***	(.062)
5	-.012	(.095)	.129*	(.058)	.003	(.061)	.079	(.062)
Constant	2.917***	(.075)	2.792***	(.092)	3.113***	(.051)	2.253***	(.050)
Random effects	σ	(Robust SE)	σ	(Robust SE)	σ	(Robust SE)	σ	(Robust SE)
Hot spot	.020	(.010)	.010	(.010)	.004	(.004)	.007	(.004)
Residual	.373	(.022)	.303	(.303)	.223	(.013)	.189	(.011)
Log pseudolikelihood	-577.553		-508.714		-415.117		-368.157	
Wald χ^2	57.96***		43.91		61.88***		54.23***	
N	611		609		613		613	

Note: Multilevel mixed effects linear regression models with robust standard errors.
* $p < .05$, ** $p < .01$, *** $p < .001$. *p*-values are two-tailed.

Figures

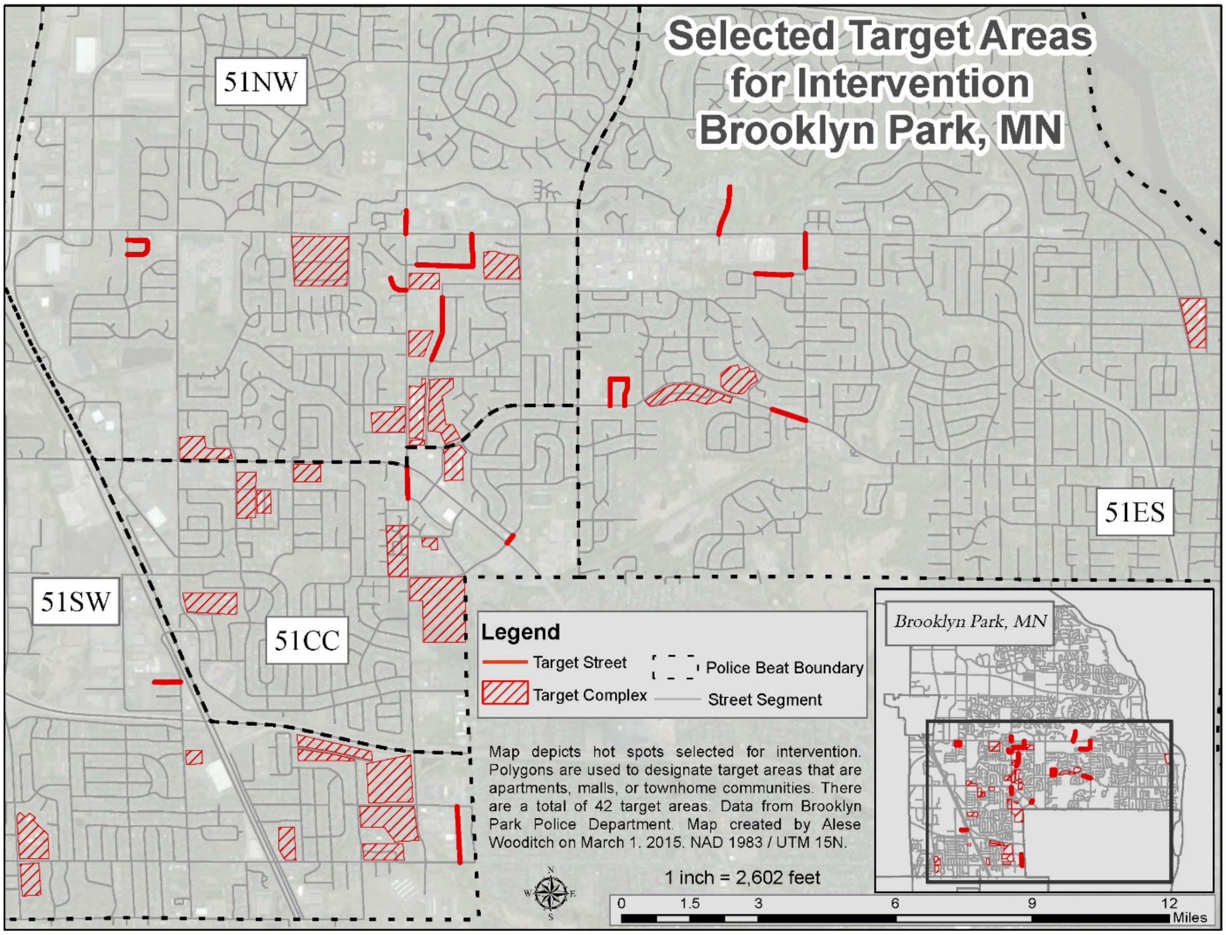

Figure 1. Hot Spots Selected for the Randomized Experiment

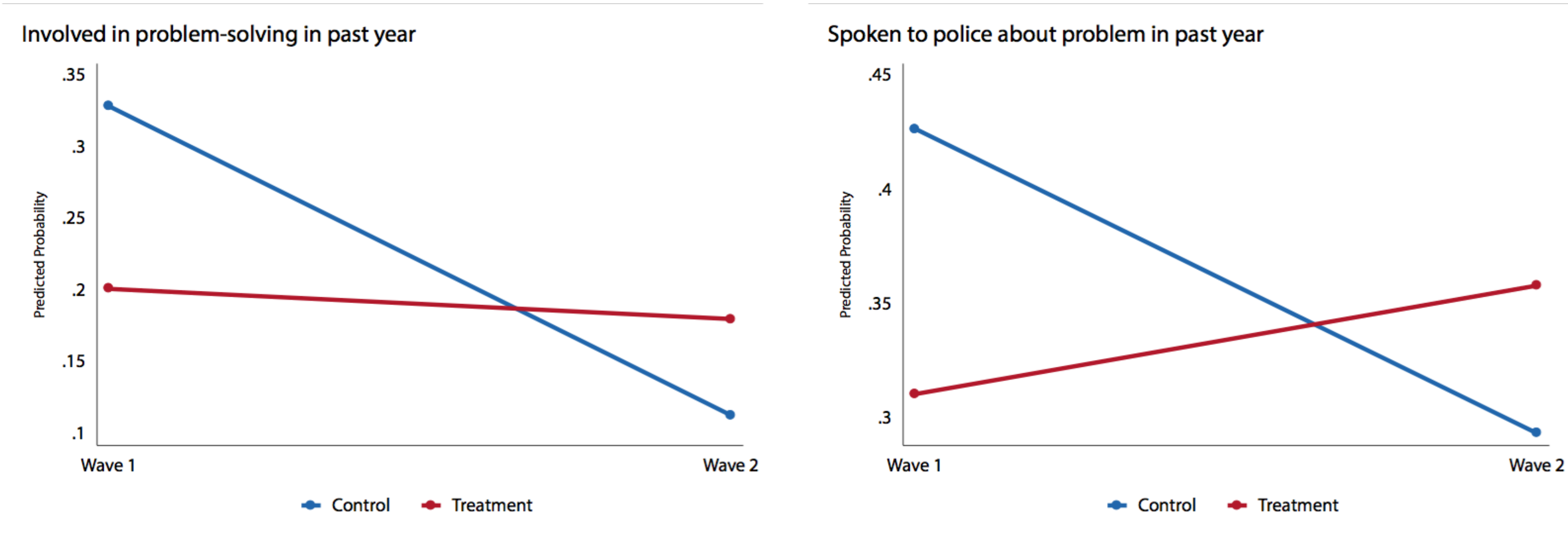

Figure 2. Marginal Means for Community Collaboration

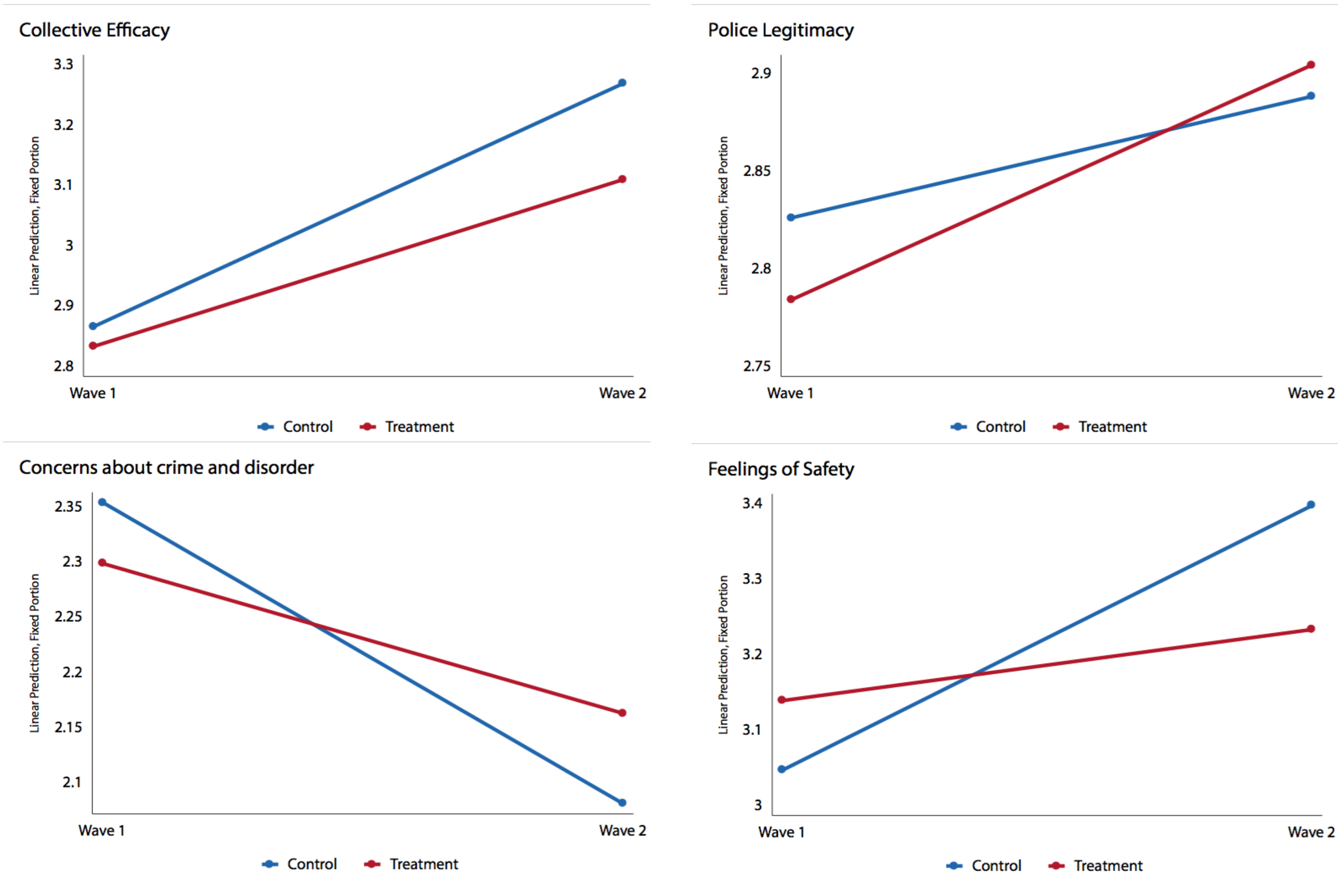

Figure 3. Marginal Means for Collective Efficacy, Feelings of Safety, Concerns about Crime and Disorder, and Police Legitimacy

Appendix A: Supplementary Tables and Figures

Table A1

Scale Construction for Collective Efficacy Measure

	N	Item-Rest Correlation	Avg. Inter-item Covariance	Cronbach's α
Someone would do something about it if…				
kids were skipping school and hanging out on the block	572	.681	.379	.879
a young person was showing disrespect to an adult	584	.739	.382	.869
a fight happened in front of your house or place of work	600	.749	.390	.868
kids were spraying graffiti or vandalizing property	602	.753	.395	.868
a group of kids was climbing on a parked car	595	.760	.395	.868
the local community center was going to be closed down because of budget cuts	538	.569	.414	.895
			.392	**.893**

Note: All questions were asked in relation to the respondent's block/apartment complex.

Table A2

Scale Construction for Feelings of Safety Measure

	N	Item-Rest Correlation	Avg. Inter-item Covariance	Cronbach's α
I feel safe walking on the street during the day	609	.678	.202	.817
I feel safe walking on the street at night	588	.659	.167	.835
It is safe for children to play outside	569	.533	.211	.843
I feel safe using public transportation	359	.628	.201	.830
I feel safe in my home	567	.700	.202	.814
I feel safe at my job or business	430	.671	.203	.820
			.198	**.851**

Note: All questions were asked in relation to the respondent's block/apartment complex. A seventh item, "this block is becoming more dangerous" (reverse coded), was excluded from the scale as it reduced α to .802.

Table A3

Scale Construction for Concerns about Crime and Disorder Measure

	N	Item-Rest Correlation	Avg. Inter-item Covariance	Cronbach's α
I am worried about…				
gangs	604	.628	.195	.906
drugs	602	.674	.191	.905
becoming a victim of violent crime	607	.709	.192	.904
becoming a victim of a property crime	612	.713	.191	.904
someone breaking into your home or business	601	.689	.192	.904
someone breaking into your car	571	.672	.192	.905
gun violence	603	.658	.194	.906
graffiti and vandalism	609	.691	.194	.905
people from other neighborhoods committing crime here	589	.487	.198	.911
The following are problems on the block/apartment complex				
Buildings with broken windows	604	.574	.202	.908
Graffiti	610	.608	.202	.908
Vacant lots	602	.602	.203	.908
Abandoned or boarded-up buildings	605	.572	.204	.909
Abandoned cars	607	.579	.203	.908
Trash and broken glass	613	.408	.205	.913
Poor street lighting	603	.168	.216	.920
Signs of drug or alcohol use	602	.631	.194	.906
			.198	**.913**

Note: All questions were asked in relation to the respondent's block/apartment complex. Items were taken from two separate survey questions asking about crime and disorder; α was slightly higher when fear of crime was tested separately (α = .917) but lower for the separate disorder scale (α = .850)

Table A4

Scale Construction for Police Legitimacy Measure

	N	Item-Rest Correlation	Avg. Inter-item Covariance	Cronbach's α
The police…				
are often dishonest (reverse coded)	540	.501	.336	.929
can be trusted to make decisions that are right for your block/ apartment complex	581	.755	.305	.905
treat people fairly	567	.811	.292	.898
treat people with respect	586	.782	.302	.901
care about problems on your block/apartment complex	573	.745	.302	.905
take time to listen to people on your block/apartment complex	551	.779	.290	.900
protect people's rights on your block/apartment complex	568	.780	.304	.901
			.304	**.918**

Note: All questions were asked in relation to the respondent's block/apartment complex.

Table A5

Univariate ANOVA for Crime Incidents and Calls for Service (Excluding Block 2)

	Crime Incidents			Calls for Service		
	F	(df1, df2)	*p*	*F*	(df1, df2)	*p*
Fixed Factors						
Intercept	.14	(1, 34)	.358	.83	(1, 34)	.388
Study Condition	.45	(1, 34)	.270	.51	(1, 34)	.240
Block	1.01	(3, 34)	.200	.19	(3, 34)	.450
Covariates						
Pre-Baseline DVs	50.8	(1, 34)	<.001***	46.1	(1, 34)	<.001***

* $p < .05$, ** $p < .01$, *** $p < .001$. p-values are one-tailed.

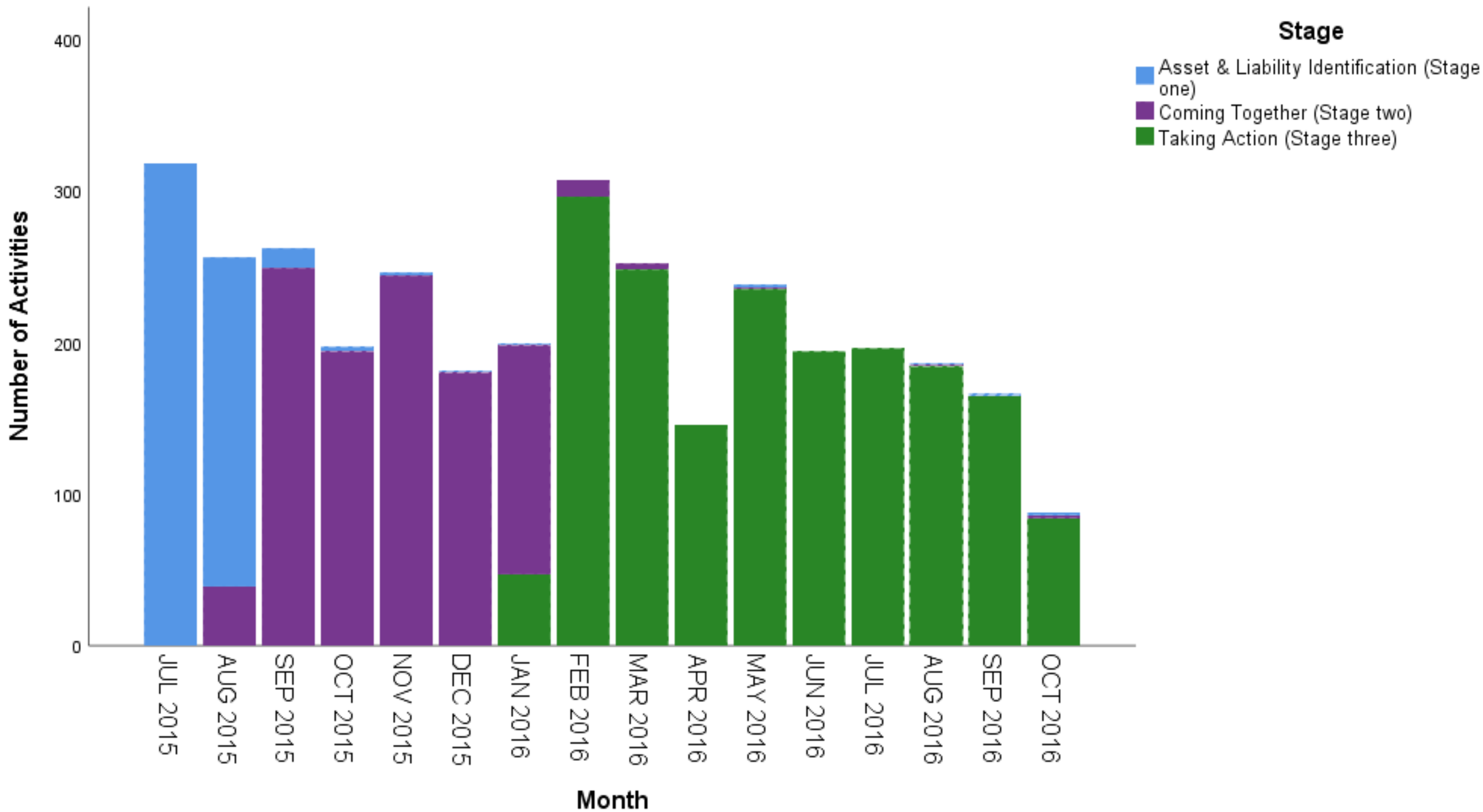

Figure A1. Number of Police Actions by Intervention Stage and Month

Appendix B: Survey Analysis Excluding Block 2

Table B1

Characteristics of survey participants by wave and by group at baseline, excluding Block 2

	Sample excl. Block 2		Treatment	Control
	Wave 1	Wave 2	at Wave 1	at Wave 1
Survey setting (%)				
House/row house/townhouse	36.1	39.1	37.1	35.3
Apartment complex or development	45.3	43.1	42.7	47.7
Business	18.6	17.8	20.3	17.0
Gender (%)				
Female	57.8	59.4	54.9	60.5
Male	42.2	40.6	45.1	39.5
Age (%)				
18-25	19.7	22.9	17.5	21.7
26-35	29.0	23.9	32.9	25.5
36-45	22.3	23.2	17.5	26.8
46-55	15.0	13.4	17.5	12.7
56-65	7.7	8.8	7.7	7.6
66-75	5.3	6.3	6.3	4.5
Over 75	1.0	1.4	.7	1.3
Race/ethnicity (%)				
Black/African-American	38.0	40.3	35.7	40.1
African immigrant/refugee	12.3	22.6	10.5	14.0
White	24.0	23.3	28.7	19.7
Asian	8.7	8.5	7.7	9.6
Hispanic	6.7	2.8	6.3	7.0
Other	4.7	1.1	7.0	2.5
More than one race	5.7.	1.4	4.2	7.0
Born in United States (%)	61.0	76.1	60.8	61.1
Has children under 18 (%)	46.5	36.3	43.8	49.0
Education (%)				
Some middle/high school	9.4	4.2	5.6	12.7
High school diploma/GED	23.7	18.0	27.5	20.4
Some college	26.4	29.3	25.4	27.4
Associate's degree	15.1	20.5	16.9	13.4
Bachelor's degree	19.1	23.7	18.3	19.7
Masters/graduate/professional degree	6.4	4.2	6.3	6.4

Characteristics of survey participants by wave and by group at baseline, excluding Block 2 (continued)

	Sample excl. Block 2		Treatment	Control
	Wave 1	Wave 2	at Wave 1	at Wave 1
Employment (%)				
Full-time	60.5	66.2	61.3	59.9
Part-time	17.1	13.0	13.4	20.4
Not working/not looking	3.3	5.3	2.8	3.8
Not working/looking	10.0	2.1	11.3	8.9
Retired	5.7	8.1	6.3	5.1
Other	3.3	5.3	4.9	1.9
Currently in school (%)				
Full-time	11.7	8.8	11.2	12.1
Part-time	13.3	16.6	14.7	12.1
Owns home (%)	25.3	17.2	28.8	22.3
Main activity at hot spot (%)				
Live	79.1	79.6	75.7	82.2
Work	16.6	16.5	18.8	14.6
Own a business	1.7	3.5	2.8	.6
Own property/land	.3	.0	.7	.0
Use local resources	.3	.0	.7	.0
Other	2.0	.4	1.4	2.5
Duration of main activity (%)				
Less than 1 year	30.0	17.6	30.8	29.3
1 year or more, but less than 5 years	42.0	38.4	39.2	44.6
5 years or more, but less than 10 years	14.0	31.0	14.0	14.0
10 years or more	14.0	13.0	16.1	12.1
Hours per day spent at hot spot (%)				
Less than 1 hour	1.3	.0	2.1	.6
1 hour or more, but less than 8 hours	20.9	22.7	18.8	22.9
8 hours or more, but less than 12 hours	38.5	48.2	36.8	40.1
12 hours or more	39.2	29.1	42.4	36.3

Note: There were no significant differences between treatment and control group participants at baseline.

Table B2

Descriptive statistics for survey outcomes, excluding Block 2

			Wave 1			Wave 2		
	Scale α[d]	(Items)	N	Mean	(SD)	N	Mean	(SD)
Involved in problem-solving with neighbors in past year[a]	-	-	298	.28	(.45)	283	.14	(.35)
Spoken with police about problem in past year[a]	-	-	297	.37	(.48)	284	.33	(.48)
Collective efficacy[b]	.893	(6)	299	2.86	(.61)	284	3.20	(.67)
Feelings of safety[c]	.851	(6)	301	3.10	(.43)	284	3.33	(.52)
Concerns about crime and disorder[c]	.913	(17)	301	2.32	(.45)	284	2.11	(.44)
Police legitimacy[c]	.918	(7)	299	2.80	(.62)	282	2.92	(.50)

[a] Outcomes based on a binary measure (1 = yes, 0 = no)
[b] Outcomes based on a 4-point likelihood scale (1 = very unlikely, 4 = very likely)
[c] Outcomes based on a 4-point agreement scale (1 = strongly disagree, 4 = strongly agree)
[d] Scale alphas are based on the original scale construction using the full sample. Ns and means are based on the sample excluding Block 2.

Table B3

Impact of the Intervention on Community Collaboration, excluding Block 2

	Involved in Problem-Solving[a]		Spoken with Police about Problem[b]	
Fixed effects	*b*	(Robust SE)	*b*	(Robust SE)
Wave 2	-1.616***	(.411)	-.631*	(.253)
Treatment	-.705*	(.307)	-.587*	(.247)
Wave 2 × Treatment	1.390*	(.559)	.872*	(.358)
Block (Ref = Block 4)				
1	.795	(.445)	1.036***	(.231)
3	1.021*	(.432)	.428	(.252)
5	.975*	(.393)	.333	(.259)
Constant	-1.319**	(.411)	-.658**	(.213)
Random effects	σ	(Robust SE)	σ	(Robust SE)
Hot spot	.279	(.171)	-	-
Log pseudolikelihood	-276.414	-	362.107	-
Pseudo R^2	-		.038	
Wald χ^2	24.40***		27.29***	
N	581		581	

[a] Multilevel mixed effects logistic regression with robust standard error
[b] One-level logistic regression with robust standard error
* $p < .05$, ** $p < .01$, *** $p < .001$. *p*-values are two-tailed.

Table B4

Impact of the Intervention on Collective Efficacy, Police Legitimacy, Feelings of Safety, and Concerns about Crime and Disorder, excl. Block 2

	Collective Efficacy		Police Legitimacy		Feelings of Safety		Concerns about Crime and Disorder	
Fixed effects	*b*	(Robust SE)	*b*	(Robust SE)	*b*	(Robust SE)	*b*	(Robust SE)
Wave 2	.420***	(.099)	.099	(.081)	.351***	(.081)	-.273***	(.061)
Treatment	-.028	(.067)	-.040	(.095)	.073	(.049)	-.061	(.045)
Wave 2 × Treatment	-.154	(.133)	.040	(.110)	-.235*	(.106)	.120	(.089)
Block (Ref = Block 4)								
1	-.004	(.086)	.083	(.068)	-.074	(.062)	.134**	(.050)
3	-.192	(.104)	-.006	(.108)	-.197**	(.067)	.220***	(.058)
5	-.012	(.099)	.128	(.058)	.002	(.055)	.079	(.079)
Constant	2.913***	(.050)	2.778***	(.093)	3.118***	(.049)	2.260***	(.048)
Random effects	σ	(Robust SE)	σ	(Robust SE)	σ	(Robust SE)	σ	(Robust SE)
Hot spot	.022	(.009)	.012	(.007)	.005	(.004)	.007	(.005)
Residual	.372	(.023)	.298	(.025)	.214	(.011)	.184	(.014)
Log pseudolikelihood	-551.169	-	-481.813	-	-385.362	-	-343.287	-
Wald χ^2	36.57***		8.99		50.90***		46.22***	
N	583		581		585		585	

Note: Multilevel mixed effects linear regression models with robust standard errors.
* $p < .05$, ** $p < .01$, *** $p < .001$. p-values are two-tailed.

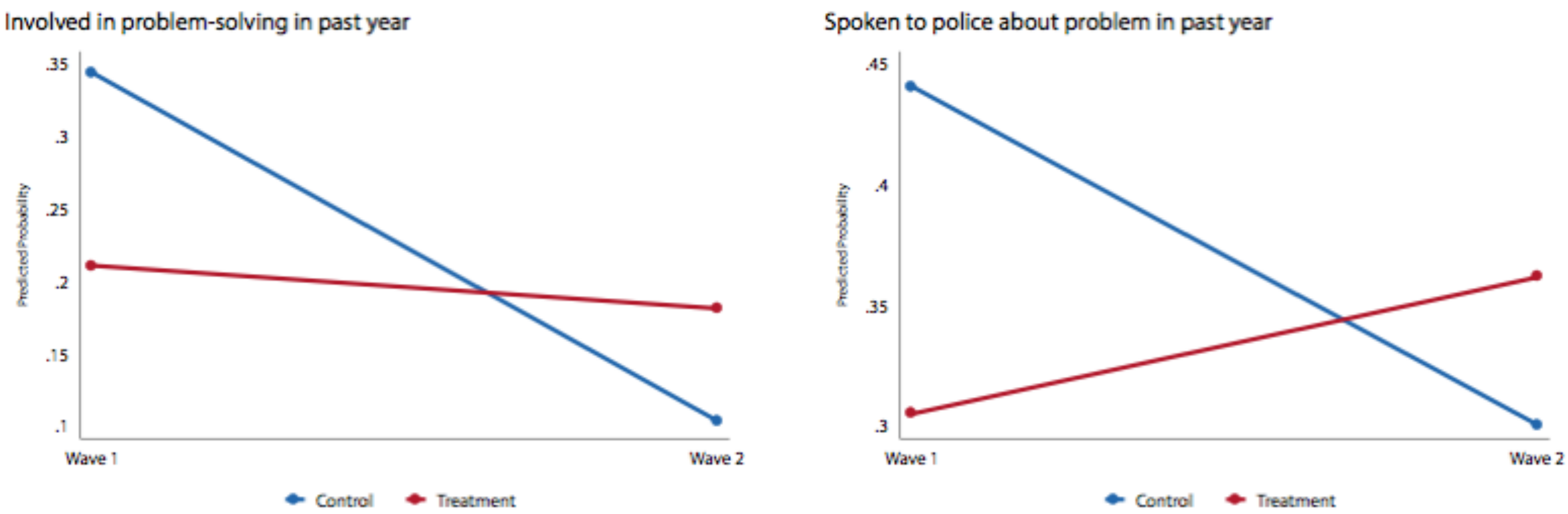

Figure B1. Marginal Means for Community Collaboration, excluding Block 2

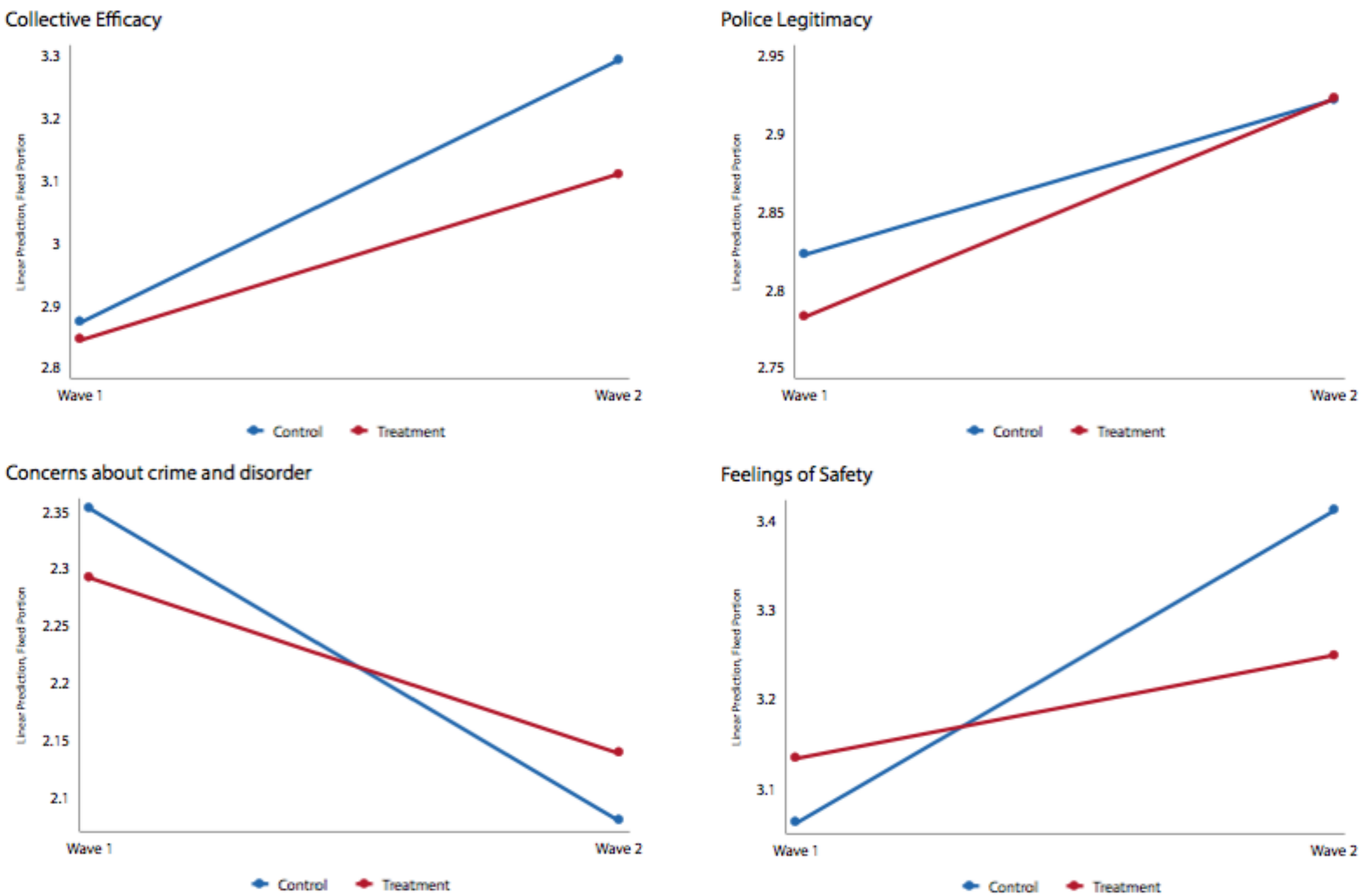

Figure B2. Marginal Means for Collective Efficacy, Feelings of Safety, Concerns about Crime and Disorder, and Police Legitimacy, excl. Block 2