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"Soft" policing at hot spots—do police community support officers work? A randomized controlled trial

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Abstract
Objectives To determine whether crime-reduction effects of increased police patrols in hot spots are dependent on the "hard" threat of immediate physical arrest, or whether "soft" patrols by civilian (but uniformed) police staff with few arrest powers and no weapons can also reduce crime. We also sought to assess whether the number of discrete patrol visits to a hot spot was more or less important than the total minutes of police presence across all visits, and whether effects based on counts of crime would be consistent with effects on a Crime Harm Index outcome.
Methods We randomly assigned 72 hot spots into 34 treatment units and 38 controls. Treatment consisted of increases in foot patrol by uniformed, unarmed, Police Community Support Officers (PCSOs) who carry no weapons and hold few arrest powers beyond those of ordinary citizens. GPS-trackers on every PCSO and Constable in the city yielded precise measurements of all patrol time in all hot spots. Standardized mean differences (Cohen's *d*), OLS regression model, and Weighted Displacement Quotient are used to assess main effects, to model the interaction effect of GPS data with treatment, and to measure the diffusion-of-benefits of the intervention, respectively. Outcomes included counts of incidents as well as the Cambridge Crime Harm Index.
Results As intended, patrol visits and minutes by Police Constables were equal across the treatment and control groups. The sole difference in policing between the treatments groups was in visits to the hot spots by PCSOs, in both the mean daily frequency of discrete visits (T = 4.65, C = 2.66; $p \leq .001$) and total minutes across all visits (T = 37.41,

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C = 15.92; $p \leq .001$), approximately two more ten-minute visits per day in treatment than in control. Main effect estimates suggest 39 % less crime by difference-in-difference analysis of reported crimes compared to control conditions, and 20 % reductions in emergency calls-for-service compared to controls. Crime in surrounding areas showed a diffusion of benefits rather than displacement for treatment hot spots compared to controls. A "Reiss's Reward" effect was observed, with more proactive patrols predicting less crime across treatment hot spots, while more reactive PCSO time predicted more crime across control hot spots. Crime Harm Index estimates of the seriousness value of crime prevented ranged from 85 to 360 potential days of imprisonment in each treatment group hot spot (relative to controls) by a mean difference of 21 more minutes of PCSO patrol per day, for a potential return on investment of up to 26 to 1.
Conclusions A crime reduction effect of extra patrols in hot spots is not conditional on "hard" police power. Even small differences in foot patrols showing the "soft power" of unarmed paraprofessionals, holding constant vehicular patrols by Police Constables, were causally linked to both lower counts of crimes and a substantially lower crime harm index score. Correlational evidence within the treatment group suggests that greater frequency of discrete PCSO visits may yield more crime reduction benefit than greater duration of those visits, but RCTs are needed for better evidence on this crucial issue.

Keywords Patrol · Hot spots · Arrest powers · Police Community Support Officers · Experiment · Deterrence · GPS-trackers

Introduction

Hot spots of crime and disorder have received much attention in recent years. An abundance of rigorous evidence converges on two lines of research. First, predictive and diagnostic approaches show that crime is disproportionately concentrated into a "power few" "micro" areas of land afflicted by a disproportional number of antisocial events (Pierce et al. 1988; Sherman 1987; Sherman 1995; Sherman et al. 1989; Weisburd 2015; Weisburd et al. 2004). These small pieces of land—street segments, intersections, city blocks or unique addresses—account for much of the crime in *any* city described in published research to date. This phenomenon has led to the discovery of what is called a "law of concentration of crime in place" (Weisburd 2015; Weisburd & Amram 2014; Weisburd et al. 2010, p. 16), or what might be termed the criminal careers of places (Sherman 1993, 2007; see also Sherman et al. 1989).

The second area of work tests the preventive effect of police presence at these hot spots. Repeated randomized trials show that targeted increases in police patrol deployment reduce recorded crime in the targeted areas, compared to control areas, both patrolling in marked police cars (Sherman and Weisburd 1995; Telep et al. 2012) and on foot (Ratcliffe et al. 2011). Systematic reviews of the evidence on hot spots policing experiments suggest that the benefits associated with it exceed the costs (Braga et al. 1999, 2012), without much evidence of spatial displacement to adjacent areas in the vicinity of the targeted hot spots (Bowers et al. 2011; Weisburd et al. 2006).

The success of hot spot policing in reducing crime is generally attributed to deterrence theory (Nagin 2013a; Sherman and Weisburd 1995; Sherman et al. 2014). The efficacy of deterrence has been argued to be borne out of the perceived likelihood of apprehension (Nagin 2013a; Weisburd et al. 2013b): uniformed power-holders who can make arrests for criminal transgressions that they detect cause rational actors to become substantially less likely to commit crime. Police powers of arrest and legal authority to apply necessary force create a threat to anyone contemplating a crime.

This theoretical perspective has gone generally uncontested. Police officers and scholars equally assume that the threat of immediate incarceration, or at least interdiction, deters offenders. As Durlauf and Nagin write, "for criminal decisions, what matters is the subjective probability a potential criminal assigns to apprehension" (Durlauf and Nagin 2011; see also Groff et al. 2014). Yet there has been little attention to operationalizing subjective threat in experimental work. Nagin (2013a, b), Loughran et al. (2012b), and more recently Nagin et al. (2015) have modeled the necessary conditions in which deterrence exerts an effect on decision-making. Yet how much of a threat, with what immediacy and what kind of social control has generally been untested.

These theoretical questions have practical meaning. Think, for instance, about the common view that crime is most effectively deterred by police officers who carry firearms, as distinct from unarmed security guards—or even from police who routinely patrol without firearms, as in the United Kingdom. This maxim of deterrence through superior force implies that police officers must apply a *direct* threat of total intervention, including immediate death, in order to create a localized general deterrent effect. Yet so far, there is no experimental evidence to address that claim. In this paper, we try to address that shortcoming. We test a form of "soft" policing (see Burke 2004) on crime and disorder in an English city, with a paraprofessional police role first proposed in the US (President's Commission 1967) and later adopted by police forces in England and Wales, which employ some 15,000 Police Community Support Officers (PCSOs) to complement some 110,000 Police Constables.

Police community service officers

The option of "soft policing" was enshrined in law with the introduction of "police community support officers" (PCSOs) through the Police Reform Act for England and Wales of 2002. From that date, PCSOs have been entitled to wear uniforms that look very much like that of police constables, but with far less powers than constables may exercise (Johnston 2006). PCSOs are not *police* officers; "they are civilian members of police staff." They work alongside their "warranted" police officer colleagues (who carry a "warrant card" that empowers to make arrests) "to provide a highly visible, accessible and familiar presence" (www.met.police.uk/pcso, downloaded 5th February 2016). Warranted police officers (also called Constables) have powers of arrest and are trained in first aid. PCSOs are non-warranted, and are barred from investigating crimes. They cannot carry firearms or any other weapons. They do, however, have specific powers to deal with specific minor offenses, such as ordering public beggars to desist and confiscating tobacco from persons under 16. They can even (in a quasi-judicial role) issue on-the-street fixed penalty notices requiring people to pay a fine for an offense the PCSO has witnessed (for example, cycling on the pavement, dog fouling, littering, and graffiti). Thus, their neighborhood policing role often amounts to increasing reassurance and visibility (see Innes 2005). They are also involved in administrative roles under direction by warranted officers, in such operations as seizing illegal narcotics or collecting CCTV evidence.

While our use of the term “soft” to describe the power of PCSOs does not conform exactly to the meaning suggested by Burke (2004), our use conveys the actual extent of power—if not a style of persuasion—that PCSOs have relative to Constables. The powers of Constables substantially exceed those of PCSOs, with full arrest powers and weaponry (including nightsticks and tasers). PCSOs, in contrast, lack the most classic police powers beyond their policing insignia. They do not carry any type of weapon or handcuffs, not even a nightstick (“billyclub”). They cannot make arrests, conduct stop-and-frisks, or (in most kinds of cases) detain suspects or use any form of physical force against suspects of crime. Their tasks focus on providing reassurance, visibility and to serve as a link between the police and the community. According to the Cambridgeshire Constabulary website, PCSOs “carry radios to enable them to call for assistance, should it be required…wear protective vests, but [do not] carry other personal protection equipment such as CS spray or batons…PCSOs [in Peterborough] have blue bands around their hats, blue ties, and blue epaulettes on their shoulders. On the back of their coat or jacket, it says POLICE COMMUNITY SUPPORT OFFICER. All PCSOs carry personal identification,” although notably these are not police warrant cards (https://www.cambs.police.uk/recruitment/pcso/faq.asp).

Yet, given budgetary constraints in British policing, PCSOs are, for the most part, the only officers who conduct proactive and visible foot patrol—including in hot spots of crime and disorder. And at a distance, as Photo 1 shows, the appearance of a PCSO is extremely similar to that of a Police Constable.

Thus, this new “softer” police role provides an opportunity to test the lower limits of deterrence through reduced intrusiveness of legal powers and threats of summary use of force. They allow us to ask, experimentally, whether effective guardianship against crime and disorder in hot spots can be achieved with almost nothing but signals of social control, without hard power to implement control immediately.

Our study looked at the deterrent effect of these community-support officers at the most chronic, persistent and “hottest” hot spots of crime and disorder in Peterborough, a medium-sized English city in Cambridgeshire. Our randomized controlled trial assigned extra PCSO patrols to about half of the population of the 72 hottest hot spots, while the other half served as control hot spots. The extra PCSO patrol tasks were comprised of community engagements and visible foot patrols for 15 minutes, 3 times per day, during the hottest hours in terms of crime frequency, over a period of 12 months. Our outcome measures included the numbers of (1) crime reports, (2) calls for service, (3) assaults

Photo 1 Police Community Support Officer (*left*) and Police Constable (*right*)

against officers, and (4) harm measures. We paid particular attention to the possibility of spatial displacement of crime to areas around these hot spots.

To refine our test of the "soft power" hypothesis, we created an unusually precise measure of the "dosage" (or minutes) of police patrol presence, by category of police officer (PCSO vs. Police Constable). While the original hot spots patrol experiment (Sherman and Weisburd 1995) used 18 graduate students with stopwatches and clipboards to measure how much police presence each hot spot received, most hot spots experiments since then (listed in Braga et al. 2012) had no ongoing tracking of police patrol time. Our quest for precision was especially necessary so that we could distinguish a difference in PCSO time from any difference (or similarity) in Police Constable time. We were able to take advantage of recent advancements in GPS technologies, using them in ways that British police (or police elsewhere) had never used them before.

With a GPS transponder in every police officer's body-worn radio, we were able to track the dosage of police presence by type of police officer in each of the 72 hot spots. We measured the precise number of minutes that every officer's radio was located within each of the hot spots, in both treatment and control conditions, as well as the numbers of discrete visits (defined as arrivals and departures). We were also able to use these measures to distinguish "soft" policing time of the PCSOs from the "hard" policing time of the Police Constables.

This paper begins with a review of the available evidence on hot spots policing, particularly findings from field experiments. Despite the impressive growth of this line of research since the original hot spots policing experiment (Sherman and Weisburd 1995), there are nevertheless major pieces missing from the puzzle—for instance, the police engagement necessary to exert a deterrent threat, which is the focus of our experiment. There is a substantial body of research on policing hot spots more broadly, which we touch on later, yet we found very limited direct investigations of what officers do to police hot spots. We conclude our literature review by looking at previous studies that called for a more granular analysis of deterrence theory and its application for the criminology of places, with specific attention to the work of Nagin (2013a, b).

We then move on to describe our experiment with the Cambridgeshire Constabulary. We describe our design, our measures, the random assignment procedure and partial blinding, the interventions through the PCSOs and a description of the statistical procedures we used to analyze the results. We then discuss the findings and their implications for both theory and police practices.

Background

Effects of more policing at hot spots of crime and disorder

"Hot spot policing"—once crudely described as a tactic of placing "cops on dots"—has been tested in dozens of rigorous tests. A recent Campbell Collaboration systematic review showed that most tests of hot spot policing were associated with significant reductions in crime in treatment hot spots compared to control conditions (Braga and Clarke 2014; Braga et al. 2012). The list of hot spots experiments is continuously growing (e.g., Ratcliffe et al. 2011; Rosenfeld et al. 2014; Telep et al. 2012). Taken together, it reflects a "strong body of evidence [which] suggests that taking a focused geographic approach to crime problems can increase the effectiveness of policing" (Skogan and Frydl 2004, p. 247).

There is also evidence to suggest that successful hot spot policing of crime does not displace crimes to adjacent areas in the vicinity of the targeted hot spots (Bowers et al. 2011; Weisburd et al. 2006). Instead, there seems to be diffusion of benefits of these social control mechanisms to surrounding areas (Clarke and Weisburd 1994), or "radiation" of the treatment effect (Ariel 2014), not only "around the corner" from the targeted hot spots (Weisburd et al. 2006) but also to larger geographic areas (Telep et al. 2014).

Police engagement tactics in hot spots of crime and disorder

Thus, the evidence on hot spot policing is clear: when police officers *target* hot spots, they are able to reduce crime and disorder compared to control conditions. Directing the police to micro-places so that officers will apply social control consistently prevents crime. Despite this robust conclusion, it is still an open question as to what is the best tactical approach to policing hot spots. Put differently, what *dimensions* of police engagement work best, and under what conditions, in preventing crime at hot spots? There is an increasing focus in the literature on determining "the optimal strategies" of hot spot policing (Koper 2014). Some recent studies continue to reaffirm Sherman and Weisburd's (1995) original finding based on pure saturation, with no effort to structure what police actually do in hot spots (Telep et al. 2012). Yet others have begun to look more closely at precisely what specific aspects of police presence may more effectively prevent crime than others. For example, some have looked at problem-oriented policing (e.g., Braga and Bond 2008; Braga et al. 1999; Taylor et al. 2010; Weisburd and Green 1995), drug enforcement operations (e.g., Weisburd and Green 1994, 1995), increased gun searches and seizures (e.g., Sherman and Rogan 1995a, b), foot patrols (e.g., Ratcliffe et al. 2011), crackdowns (Sherman and Rogan 1995a), "zero-tolerance" policing or "broken windows tactics" (Caeti 1999; Weisburd et al. 2011), and intensified engagement (Rosenfeld et al. 2014). Yet few of these studies provide detailed measures of exactly what police were doing in the experimental hot spots; even one that did (Sherman and Rogan 1995a, b) failed to measure what police did in the control hot spot.

The deterrent effect of policing

Despite the undocumented treatment content and its variations in previous hot spots experiments, there are nevertheless common attributes to all hot spot policing approaches. First, it seems that the police must be *focused* on these micro-places of crime and disorder. In all studies of police initiatives that target hot spots with high spatial concentrations of events, officers have consciously focused both resources and efforts on these places. Once officers are tasked with applying *any* sort of intervention, crime generally goes down compared to hot spots not exposed to these focused treatments. We do not have a strong indication of which approach works "better" or in a "more" cost-effective way compared to other approaches (cf. Taylor et al. 2010), but the overall direction of virtually all hot spots studies suggests reductions in crime following focused engagement by police in the hot spots (Braga et al. 2012).

The second, and crucial, common theme is a clearly stated mission for police to serve as "sentinels" deterring crime in the hot spots (Nagin 2013a, p. 10). As opposed to police in hot spots acting as apprehension agents (incidentally, apprehension risk is probably not materially increased by improved investigations; Nagin 2013b, p. 89; see also Braga et al. 2011), hot spots patrol officers are trained to serve as "crime preventers" when they are visible to the public. This view of police officers as predominately guardians was anticipated in Cohen and Felson's (1979) routine activities theory: police in their role as sentinels act as guardians who reduce *opportunities* for committing a crime. A drug store with a police officer standing outside it, for example, is not as attractive a criminal target as one with no police officer nearby. Opportunity theories of crime, such as routine activity (Cohen and Felson 1979; Cornish and Clarke 1986), rational choice (Clarke and Felson, 1993), and crime pattern theory (Brantingham and Brantingham 1993a, b), have often been used to understand the place characteristics, situations, and dynamics that cause criminal events to concentrate at particular places. As the authors suggest, the increased presence of police augments the level of guardianship in targeted places. Heightened levels of patrol prevent crimes by introducing the watchful eye of the police as a guardian to protect potential victims from potential offenders. Even when officers are tasked to problem-solve, or engage the public through neighborhood policing, officers are nevertheless uniform-wearing, (often gun-carrying) power-holders who communicate the authority of the state by their presence. This quality, which is embodied through police insignia, contains a literal threat of apprehension which sends an unequivocal message. No matter the tactic applied, the presence of officers intensifies the cognitive perception of plausible apprehension for any transgression of the law. Even "softer" police approaches like community policing still contain an ingredient of deterrence, at the very least for the duration of officers' physical presence within the hot spots.

To be sure, this presumption of effective threat is not just theoretical; based on interviews with 589 arrestees in New York City following the police's quality of life initiatives, "the most important factor" behind behavioral changes—that is, reductions in the likelihood of committing crime and disorder—was police *presence* (Golub et al. 2003, p. 690). Wright and Decker (1994) reported similar results: offenders appear to be aware of police presence when they select their targets; they avoid neighborhoods with increased police presence when making a decision to commit robbery.

With this in mind, we were particularly drawn to the question: how much of a deterrence threat is needed in order to materially motivate offenders away from committing crime? Answering this question is critical, not just for administrative criminologists who investigate law enforcement, but also for theorists who look at choices, risk perceptions and opportunity theories. Risk perceptions are perceptions of the likelihood and controllability of the event, as well as perceptions of the impact of the event if it were to occur (see review in Jackson and Kuha 2015, p. 10). Constructing these risk probabilities requires some level of rational thinking—even though the decision to commit crime is often described as irrational and suboptimal (Matsueda 2013; see also Cornish and Clarke 1986). Still, these perceptions are particularly pertinent to deterrence theory, which finds many examples of deterrent effects, *on average*, across large groups.

There are clear individual differences in deterrability, perhaps based on differences in perceptions of the risks of getting caught. For example, experienced offenders seem to place relatively more weight on their prior subjective probabilities, unlike inexperienced offenders (Nagin 2013b, p. 94). The basic need to believe that things are stable, certain and predictable also varies between individuals (Kruglanski and Webster 1996). Within this framework, the decision to commit crime is heavily affected by one prominent factor: the individual's *perceived* risk of apprehension (Loughran et al. 2011; Nagin 1998, 2013a). Perception of this risk was found to be highly influenced by proximate variables, including objective sanction risks (Apel 2013), and among these objective risks, police presence is an important ecological cue that inhibits criminal conduct (Golub et al. 2003; Sherman 1990). But we remain entirely unclear about how much the presence of police doing what, has how much effect, for how long.

Quantifying the certainty of apprehension

At a high level of abstraction, there is ample evidence that the perceived certainty of punishment is causally associated with less crime (Bushway and Reuter 2008; Cullen et al. 2008; Lochner 2003; Loughran et al. 2012a; McCarthy 2002; Paternoster 2010; but cf. Berk and MacDonald 2010; Tonry 2008). More than severity of sanctions and likely to be more than celerity of sanctions (Nagin 2013a; Von Hirsch et al. 1999), increasing the likelihood of being caught is inversely linked to the likelihood of committing an offense. This "certainty effect" carries wide probabilities, over a range of settings in which the criminal justice system attempts deterrence.

That conclusion allows at least two possibilities. First, while a minimum threshold of a punishment certainty effect is required to deter crime, at some point any incremental addition of certainty will no longer enhance the effect. Certainty may come and go by an "on–off switch," rather than being a continuous linear increase or decrease in decibel level. Second, that on–off switch may be set at higher thresholds of certainty for some people than for others.

On reflection, these thoughts may be unsurprising. As Loughran et al. (2012b, p. 714) explained, "certainty effects are predictably non-linear, [and] the prevailing detection probability before change occurs becomes a key moderator of certainty effects and a key consideration in policy formation." Loughran et al. (2012b) came the closest to empirically scrutinizing this effect, using subject-level data (as opposed to aggregated data); they show evidence of a "tipping effect", whereby perceived risk deters only when it reaches a certain threshold and a substantially accelerated deterrent effect occurs for individuals at the high end of the risk continuum. This is unsurprising, because the phenomenon is well acknowledged in psychology and in economics in the framework of "diminishing marginal sensitivity" (Stevens 1957; see also Chamlin 1991; Logan 1972; Tittle and Rowe 1974). It seems that "*beyond* [our emphasis] some point, any further increase in the perceived certainty of punishment is associated with only very small decreases in the mean number of crime" (Erickson et al. 1977, p. 311).

Yet a diminished marginal sensitivity does not address the *minimal* degree under which the certainty effect takes place. It addresses the tipping point in punishment risk and a point along the punishment risk continuum where punishments no longer matter. We still do not know how much policing is needed to affect average perceptions of apprehension risk in communities of different crime rates. How much policing is "enough"? As far as we can tell, there are few studies that have explicitly and *directly tested* the dosages of risk thresholds and its effect on the decision to commit crime (but cf. Braga and Bond 2008; Koper 1995). Even the broader literature on how much police presence affects perceptions of apprehension risk is scant (see Nagin 2013b; Wikström et al. 2011). Differentiating between absolute and marginal deterrence is difficult (Nagin 1998, p. 53). Police may often create only marginal impacts from incremental policy changes.

As we reviewed earlier, the evidence does seem to suggest strongly that *more* police presence reduces crime, but the dosage–response curve question and its effect on risk perception is largely missing (but see Koper 1995). There is some research which suggests that when police are abruptly *not* present, crime rates increase (Andenæs 1974; Deangelo and Hansen 2014; Di Tella and Schargrodsky 2004; Heaton 2010; Sherman and Eck 2002; Shi 2009). These studies show that sharp decreases in police presence and activity substantially increase crime—which essentially supports the basic argument that risk perceptions are affected by the presence of police officers. However, our ability to measure incremental differences in dosage and its link to crime reductions has been crude. In fact, the term "dosage" itself is unclear, to the extent that it could operationally reflect (1) the "amount" of police presence (for instance, the number of minutes), or (2) the number of visits (such as the number of patrols), or (3) the average number of officers physically present at any given time to send such a deterrent message. Police in Paris, for example, perform patrols in groups of four to eight officers; San Diego (CA) police patrol in one-officer cars. Does this difference matter?

Contextualizing sanction risks as "threats"

Our interest in the quantification of certainty effects goes beyond the dosage/measurement question and aims to look at a more profound dimension of deterrence theory. Because police provide a symbolic crystallization of power exercised over society, the "amount" of threat applied can vary in both *form* and *shape*, and not just in *magnitude*. The quantity of deterrence is not just about "how much" in observable units of time and number of visits to hot spots, but also in terms of "symbolic quantification" of power (see Butterworth 1999; Deheane 1997; Pierce 1885). To begin with, some research suggests that there is no relationship between the number of police officers per capita and perceptions of the risk of arrest (Kleck and Barnes 2010), thus suggesting that increases in police resources will not increase general deterrent effects, and that decreases will not reduce deterrent effects. It is not the aggregate sum of police visible that matters, but the *ways* in which they become visible—spatially, temporally and procedurally. The latter dimensions can potentially change perceptions, and ultimately the rate or seriousness of criminal behavior.

The risk of apprehension by sentinels is *firstly* associated with the degree to which power-holders are perceived as capable agents of the law (Bottoms and Tankebe 2012; Tankebe 2013). If an offender holds the view that the officer would not act to apprehend him, it makes little difference how frequently and for how long the officer is present at the hot spot. We do not assume that offenders generally think police officers are ineffective, or "toothless" (Ariel 2012, p. 39)—at least in western democracies—as the evidence suggests otherwise (Braga et al. 2012), but it still begs the question, what is it about the "police officer" that elevates the risk of apprehension? As Wilson (1968) showed many years ago, and Smith (1986) confirmed, the odds of a police officer making an arrest after witnessing a crime vary widely across communities. Why, then, should we expect more policing to reduce crime in hot spots in all communities?

One answer is a perceived (and often real) change in the base rate of punishment. Whatever the level of apprehension risk, the presence of the police provides a symbol that embodies the state. The "costs" of punishment associated with that symbol may make crime unattractive the more visible the symbol becomes: past experiences, vicarious experiences and collective memories "instruct" the offender to reevaluate the motivation to commit an offense when a capable guardian such as a police officer is present (see discussion in Apel and Nagin 2014; Devos 2014; Kleck 2014; Sherman 1993, pp. 468–9). We are particularly interested in the symbolic association of these costs, which are embodied through police officers.

But still, what is it about "police officers" that sends out these cost messages? The literature we reviewed earlier on actual risk of punishment may miss our important point: the *symbolic* quantification that people ascribe to these costs. "If there is anything that is distinctively human," explains Hauser (2003, p. 566), "is our capacity to represent quantities with symbols, to use such symbols with abstract functions or operators…all cultures have a system of symbolic quantification[…]for distinguishing (minimally) one object from many."

In this respect, then, what is it about the police officer's presence that captures these perceptions of risk? The uniform, shield, weapons, insignia? Take the case of firearms, for instance: in some cultures, it is a direct symbol of police authority, but not in others. Are firearms necessary—beyond personal protection [1]—if the symbol of legitimate power-holding is transpired through an insignia of "lesser" authority, such as PCSOs? For example, uniforms convey power and authority, as clothes carry a social significance (De Camargo 2012; Form and Stone 1955; Johnson 2001; Nickels 2008). So the certainty effect can therefore be exerted with the most minimal of "threat symbols"—as is the case in England, Wales, Scotland, Ireland, some states in India, New Zealand, Iceland, and other jurisdictions in which police officers do not routinely carry firearms on their person while on general duty.[2] These "lesser" symbols include badges, patches, headgears, and uniforms that identify authority. Anything beyond these might prove unnecessary *to deter* offenders from committing crime. US and Australian citizens, for example, may comfortably assume that it is *both* the badge and the weapons of control that are the universal symbols of authority and power (De Camargo 2012). But sanction risks may be perceived based solely on the badge, without any need for the bullets.

The problem with these sets of questions is that we have had no systematic assessments to answer them. One cannot just turn to "arrest risks" as directly linked to symbolic quantification of sanction threats (see Bouchard and Tremblay 2005; Richards and Tittle 1982; Viscusi 1986). Arrest carries the symbolic embodiment of sanction threats, and we often assume that the arrest is intertwined with apprehension risks ("I do not want to get arrested"). But in the context of *prevention* and deterrence, why is the *threat* of *immediate* seizure or forcible restraint necessary at all? For the most part, this question remains untouched. Nagin (2013b, p. 85) addresses this to some extent, by distinguishing between formal and informal sanction costs, which are costs that are separate from those that attend the imposition of formal sanctions. Formal sanctions include "loss of freedom or fines," while informal sanctions "include censure

[1] One could also argue that the uniform itself protects the officer from assaults; when a police officer has a distinguishable uniform, it can help prevent his or her injury or death.

[2] http://www.loc.gov/law/help/police-weapons/index.php?loclr=bloglaw

by friends and family and loss of social and economic standing… [and] the magnitude of informal costs may be largely independent of the severity of legal consequences.” Merely being arrested for committing a crime, then, may trigger the imposition of informal sanctions. Williams and Hawkins (1986) use the term “fear of arrest” to label the deterrent effect of informal sanction cost. But again, what part of “police presence” exacerbates these perceptions of risk? Is it the power to use force, or the visible firearm? The potential of immediate arrest?

These questions lead us to wonder whether we can advance our theory of deterrence by “watering down” the threat of apprehension to its lowest threshold, with nothing but a sign of the state, stripped naked of weapons and most arrest powers. Would that symbolic representation of authority still effectively cause offenders to commit less crime with more police presence? On the one hand, that *mere* presence of police officers might be a necessary condition to effective deterrence. But if deterrence requires hard power, a merely symbolic presence might not be sufficient. The material capacity of power-holders to immediately apply incapacitation, when needed, may be required as well, in order for individuals to make the decision not to commit crime. If that is true, then officers must instil “fear of arrest”, with some degree of actual threat of incarceration, in order for deterrence to exist. On the other hand, it is possible that most offenders are generally discouraged by even the most minimal threat of apprehension exerted by police officers who are “simply there” but do not need to apply their powers. If this is true, then deterrence is exerted through symbolic signals of authority, and the use of “hard” power may be excessive in relation to the objective of preventing crime.

The Peterborough hot spots experiment

Our objective in this study is to address the gaps in the literature in three major ways: treatment content, treatment measurement, and local crime displacement. First, by experimenting *only* with the allocation of PCSO patrol, we test a “softer” policing intervention aimed to reduce crime in hot spots: The only difference between the two randomly assigned groups of crime hot spots in our experiment is the amount of time spent by uniformed yet weapon-less foot-patrolling PCSOs with no powers of arrest. Second, by measuring both “soft” and “hard” police presence precisely and reliably, we contribute the first published hot spots policing experiment that tracks both foot and vehicle patrols with personally-issued GPS trackers (in body-worn radios). This technology of measurement allows us to present the most accurate (to date) treatment versus control comparisons of the independent variables of police patrols at hot spots. Third, we measure whether such policing causes spatial displacement to adjacent areas in relation to the known magnitudes of difference in the independent variables between treatment and control groups.

Methods

Settings and design

Peterborough is a city of 200,000 residents that occupies nearly 133 square miles (Peterborough City Council 2013). Located in the east of England within the county of

Cambridgeshire, Peterborough is ranked as the 27th largest city in England and Wales. Once an industrial center, employment is currently diversified across many sectors. The city's demographics include 81 % Whites, 12 % Asian and approximately 3 % Blacks. The city experienced 6.85 crimes per 100 residents in 2013, while the UK mean is 6.57.

Peterborough's police, comprising a division of Cambridgeshire Constabulary, employed approximately 60 PCSOs in the year of the experiment. Broadly speaking, PCSOs in Peterborough were assigned to neighborhood commanders. Interestingly, because of budget constraints in England and Wales forces, the primary utility of PCSOs in neighborhood policing is foot patrol, while most warranted Constables conduct vehicle-based patrols. Thus, the findings of this study have implications for the future of foot patrols more broadly in the UK, as well as in any other country that adopts a "paraprofessional" model for neighborhood patrols (Ratcliffe et al. 2009; Wain and Ariel 2014).

In order to test the effectiveness of PCSOs, we were granted permission to work with the Constabulary's Information Management team to analyze crime data by location. We identified the 72 highest-crime hot spots in the city, and then (independently of any consultation with police) randomly assigned 38 to treatment conditions and 34 to control conditions. The precise locations of the treatment hot spots and their boundaries were communicated to local commanders directly: they were given their assigned hot spots and were informed that "these are the hottest hot spots" in the city. But in an effort to keep the control locations on a business-as-usual condition, we kept the police commanders as well as the PCSOs blind to the control locations. At no point during the experiment were they informed the location of the control hot spots, in order to avoid any possible bias or contamination—thus maintaining a "partially-blinded experimental design."

Defining the hot spots

There is a voluminous body of literature on how to map crime and disorder hot spots. This literature describes many different methods of drawing small areas of land that tend to have a disproportional concentration of crime and disorder. From a theoretical perspective, it seems that the size of hot spot does not change the overall pattern of a skewed concentration of events in these hotspots, of all events in the city (Eck et al. 2005; Hart and Zandbergen 2014). Whether GIS systems define a hot spot as a cluster, street segment, or the archaic method of creating arbitrary circles or grids of crime, usually less than 5 % of the land "produces" at least 50 % of crime and disorder. Different methods support the "law" of concentration of crime in place, which means that "crime hot spot maps can most effectively guide police action [as long as the] production of the maps is guided by crime theories (place, victim, street, or neighborhood)" (Weisburd 2015; Weisburd et al. 2012).

However, there are *practical* implications for how a hot spot is defined. Our practical challenge was to define hot spots that made sense for defining foot patrol assignments. While we concur in principle with Weisburd et al. (2012) that defining hot spots as street segments (i.e., between two street intersections) is coherent and theoretically driven in a US grid system, we found that the approach is less applicable to the street topography and layout of ancient Cathedral cities in England and Wales, such as Peterborough. The streets in pre-streetcar era British cities (let alone the automobile) are not arranged at right-angles. They are, rather, a messy patchwork of short streets with long names. Using a street-segment approach to defining the hot spots produces a very small number of eligible hot spots, and the potential for local deterrence to materialize through a robust approach of “seeing and being seen” (Sherman and Weisburd 1995) was greatly minimized. Instead, we used 150-meter radius polygons, which allowed the officers some discretion about where to go *within* these constrained hot spots, but also embraced the location of a substantial number of crimes each year. There may also be additional social control benefits using this definition. Police officers are more likely to be seen by non-criminal elements in main streets *around* crime attractors, which are more susceptible to being crime hot spots (Ariel et al. 2014; Weisburd et al. 2006), and which may both reduce fear of crime and increase public confidence through visible policing (cf. Sosinski et al. 2015).

In order to measure possible displacement effects, we supplemented the 72 hot spot polygons with a 50-meter zone beyond the hot spot boundaries. This plan was essential for our research objectives, despite the available research evidence against local displacement and in favor of diffusion of benefits to adjacent areas around the hot spots (Bowers et al. 2011; Weisburd et al. 2006; but cf. Sorg et al. 2014).[3] The officers were instructed to patrol the hot spots, not the buffers, but whatever the treatment effect would be, it was measured in these buffer zones as well as within the hot spots.

In order to insure statistical independence of the units of analysis, we created a third zone beyond the buffer zones for each hot spot of at least 100 meters. This third area served as an additional cushion so that no two hot spots would come any closer than the combined width of their cushions. This criterion helped avoid a situation in which one treatment and one control hot spot, for instance, are so close to each other that the treatment effect could spill over into the control area. We thereby reduced the risk of violating the Stable Unit Value Transfer Assumption (SUTVA) that the effect of the treatment condition on each unit (treatment or control) is independent of the effects of treatment on any other units (Sampson 2010). This necessary procedure, unfortunately, greatly reduced the number of eligible hot spots. Yet the gains for internal validity of the design outweighed any loss of statistical power from a smaller sample.

We defined a “hot spot” in this experiment as an area of land (150 m radius) with no less than 36 calls for service in both the 12 months before the experiment was conducted and in a further 12 months before the experiment (or the 24th to 13th months before the experiment, as well as the previous 12 months). The two-year baseline procedure followed the method used by Sherman and Weisburd (1995) in the first hot spot policing experiment, in order to insure stability of the hot spot locations during the experiment with hot spots that were persistent and chronic (Weisburd et al. 2004). The types of incidents included in the definition of the hot spots were calls about any street crime categories that visible officers would be able to deter in the public domain, such as antisocial behavior (49 % of all incidents), robberies, violence, vehicle theft, and graffiti—but not predominantly indoor crimes such as domestic disturbances.

Random assignment and partial blinding

The exclusion criteria we used created a list of all 72 eligible hot spots across the entire city. We conducted pure simple random assignment, which created a 53 % treatment

[3] The catchment zone radii are relatively small (cf. Weisburd et al. 2006), because street segment lengths in Peterborough are often shorter than 50 meters. Moreover, due to the city size, increasing the buffer zones would reduce the number of eligible hot spots even further, thus jeopardising the statistical power of the test.

and 47 % control group split of the full population of eligible hot spots. As noted earlier, stations and district commanders were not given the full list of hot spots; local police forces were informed of the location of their *treatment hot spots*, but they were blinded about the location of the control hot spots. This blinding process decreased the chance of contamination and police-initiated SUTVA violations (see Sampson 2010). The number of treatment hot spots ($n=34$) also fitted the operational availability of patrol officers seconded to the experiment.

Treatment conditions

There were three major characteristics of the treatment in this experiment. First, the treatment was delivered by uniformed but civilian police staff with no weapons or arrest powers who were tasked to "be visible" to deter crime and antisocial behavior in hot spots. They patrolled alone, on foot, out of sight of other police staff, with only a radio to connect them to their colleagues. Second, their treatment was targeted on the "hot hours" and "hot days" for Peterborough. Based on the 24-month baseline analysis, the temporal crime peaks were on Tuesdays through Saturdays, between 1500 and 2200 hours. These were the patrol hours for the PCSOs, within which temporal boundaries we measured the treatment effect. At any given moment during these hours and days of the week over the 12 months of the experiment, there were about a dozen PCSOs on the street conducting these patrols. Given the resources constraints and the distance between the hot spots, each hot spot was assigned to receive three separate one-PCSO patrol visit per day, or 780 visits per hot spot over 12 months. Each visit was required to last 15 minutes, based on prior evidence about how to maximize residual deterrent effects after PCSOs left the scene as demonstrated by the *Koper Curve* (Koper 1995; Telep et al. 2012). Throughout the experimental period, the assignment of PCSOs to the hot spots was on a rotating basis, so that PCSOs patrolled different combinations of the 34 treatment hot spots rather than being limited to a few hot spots for all patrols.

Third, the delivery of the treatment was tracked (Sherman 2013; Wain and Ariel 2014) on a daily basis, although it was not fed back to either the Sergeants or the PCSOs on patrol (see Sherman et al. 2014). As we reviewed above, recent hot spot studies have now begun to measure more systematically *what* the officers do rather than simply measure *when* and *where* they were deployed. Note, however, that even with the recent evidence there is a lack of precision in measurement of both the time and the location of the officers, as personal GPS locators have not been used thus far (see below). The Rosenfeld et al. (2014) study is the only study that reports a sufficiently accurate account of *outputs* (e.g., arrests, stop and account). Systematic social observations (see Sampson and Raudenbush 1999) would have been the appropriate approach here, but the cost of that method exceeded our budget. Instead, we provide the following qualitative account of the treatment tactics and training.

PCSO activities in hot spots

The PCSOs were told to concentrate on being visible, to the exclusion of any other task. They were not tasked to problem-solve (Goldstein 1979) or to conduct community policing in the classic sense (e.g., Skogan and Hartnett 1997) or to conduct patrols targeted on any particular social or crime problem (e.g., McGarrell et al. 2002; Sherman and Rogan 1995a, b). The aim was to deter crime through their signals of police authority: the symbolic representation of power which the PCSOs carried, with uniforms and a two-way radio. We told them just what our theory was: that potential offenders would be discouraged by even the most minimal threat of apprehension, exerted by police staff who are "simply there" but do not need to (and cannot in any case) apply *any* level of force beyond a citizen's arrest.

Practically speaking, there is very little else that can be achieved by way of active engagement in the 15-minute patrols allocated to each hot spot. Each officer was accountable for 3–4 hot spots per evening. The walking distance between the hot spots, often a mile apart, made the experiment operationally challenging. The PCSOs repeatedly told the research team that they were pressured to "beat the clock" in rushing between hot spots. This is not to say that officers were instructed *not* to deal with events as they occured in the hot spots. If members of the public required assistance, or if the PCSOs encountered crime or disorder, they were still required to report the event to the police. However, in terms of ordinary "allocated time" (see Weisburd et al. 2015) during the experiment, these officers focused on a theoretically preventive, "saturated" presence compared to control conditions.

The no-treatment hot spots were not exposed to these proactively directed patrols. Nevertheless, the control areas were also visited frequently by police constables or PCSOs sent to them *reactively* after citizen calls to police about crime and disorder. We were able to fully measure the time and location of all these visits, regardless of whether they were reactively or proactively mobilized (Reiss 1971), for both treatment and control conditions, and for PCSOs as well as "regular" cops (see below). In fact, both treatment and control hot spots continued to be visited by Police Constable "response officers" both reactively and proactively (e.g., stop-and-search, problem-solving or crackdowns). The crucial distinction between the treatment groups was that control hot spots were *not* exposed to a prescribed daily level of proactive patrols, five days a week, in fixed hours.

Control conditions

Due to recent budget cuts, relatively little proactive directed patrols by PCSOs were deployed across the city in the period before and during the experiment. Both PCSO and Constable police officers were engaged primarily in reactively responding to emergency calls for service, with very little targeted foot patrol anywhere, with the exception of the treatment group. Our decision to withhold the locations of the control hot spots did not free them up to receive proactive patrol. Rather, it avoided raising the question of whether they should receive some of the patrols that were assigned to the treatment group. This experiment can therefore be characterized as a randomized comparison between PCSOs providing proactive and focused foot patrols (as treatment) versus both Constables and PCSOs providing reactive policing (as control).

Dependent variables

We used two primary outcome measures to assess the treatment effect: the number of calls-for-service to the police ("999 calls") and the number of victim-generated crimes within the participating hot spots, 24 months prior to the experiment, and then again during the 12 months of the experiment. We compared changes in these two outcomes between the periods before and after the beginning of the trial, and then compared this difference among the two study groups (treatment and control conditions). Crime reports proactively generated by police activity rather than by victim or witness reports, that is, incidents that are essentially *outputs* rather than treatment outcomes, such as drug offenses, stop-and-searches, were clearly marked in police records; we excluded them from the dependent variable measures (see Sherman and Weisburd 1995).

A third outcome measure is a transformation of the crime count. Rather than just treating all crime types as equal, we also applied the Cambridge Crime Harm Index (CHI) to the types of crime reported in the hot spots (Sherman 2007, 2013; Sherman et al. 2016). This procedure applies the "starting point" for sentencing persons convicted of each type of crime, without taking into account the defendant's prior criminal history or circumstantial mitigating or aggravating factors. Measured in number of days of imprisonment recommended, the Cambridge CHI provides a "bottom line" for any mixture of crimes of different types and frequency within type, across offenders, victims, times or places. It is ideally suited for comparing benefits of patrol time in practical terms, supplementing the more esoteric concept of "standardized mean differences", which have little face value for public policy. For other applications of CHI in policing, see Bland and Ariel (2015).

GPS tracking data

As far as we can tell, this study is the first to report dosage measures in both treatment and control hot spots, of all police officers in the city, down to the second, derived from body-worn GPS trackers (not Automatic Vehicle Location transponders in police cars). Every police two-way radio in the city was equipped with a global positioning system that tracked the movement of the officer, at every given moment on duty. The only theoretical exception would occur if the officer removed the radio attached to their uniform and walked over 50 meters away from it; this unlikely scenario was never observed, primarily because it is a safety issue for officers on duty to risk losing contact with the central communications team. The GPS trackers inside each two-way short-wave radio set (see Photo 2), in fact, were originally installed to provide personal security of officers, and only subsequently to allocate officers in real-time settings to calls for service.

GPS technology can therefore be used to measure how much time officers spend in particular areas, and how many visits, defined as follows. Every radio tracker was set to transmit to the satellite a "ping" with the spatiotemporal coordinates (latitude, longitude and

Photo 2 Police radio with embedded GPS tracker

a timestamp) of the tracker (see Wain and Ariel 2014). For the purposes of the experiment, the system was set to ping every 1 minute. Each visit to a hot spot was therefore defined as an uninterrupted period between the first ping inside the boundaries of a hot spot and the first ping thereafter locating the officer outside those boundaries, within the limits of the frequency of readings. The boundaries were defined by our programming of the GPS back-office systems to “geo-fence” our targeted 72 areas of land. By counting how many pings the trackers send from within these geo-fenced areas, we were able to measure with high precision and accuracy how many minutes and how many visits each officer made to these geo-fenced areas.[4] This “point in polygon” analysis was applied for all included hot spots.

Measures of displacement

Despite the accumulated evidence *against* spatial displacement to areas around the hot spots, it was nevertheless important to measure it in the current settings. We measured the number of victim-generated crimes that took place within the 150-meter-radius hot spots (both treatment and control conditions), and then the number of crimes in the catchment zones of 50 meters radii around the circumferences of the hot spots. As described more fully below, we used Guerette’s (2009; but see Bowers and Johnson 2003) Weighted Displacement Quotient (WDQ) to determine displacement or diffusion effects as a result of the RCT. WDQ determines the presence of displacement or diffusion in relation to changes in the treatment and control areas.

Statistical procedures

First, we estimated the magnitude of any treatment effect. We started by calculating the raw counts of incidents (calls for service) and crimes within the hot spots, before and during the intervention. We then calculated the difference-in-differences in means of these measures between the treatment and control groups. To estimate any treatment effect, we computed the standardized mean difference (Cohen’s *d*; Cohen 1988). These procedures allowed us to compare any intervention effect in the present study with the available hot spots research summarized in Braga et al.’s (2012) systematic review.

Second, we estimated the magnitude of any treatment differences. We calculated the raw GPS data for the number of patrol visits per day and the number of minutes spent in the hot spots, for both the PCSOs and the warranted constables. Because we had no such measures before the experiment began, we are limited to comparisons during only the 12 months of the experiment. We compared mean values by treatment group for magnitude of differences and conducted *t* tests in order to assess the statistical significance of any differences.

We then estimated the cross-sectional observational model of the experiment across all 72 hot spots when taking into account their individual police presence data. We used a linear model to assess these differences between experimental and control groups in terms of calls for service, and then in terms of crime. Group assignment (“experimental” /“control”) was the main predictor, with the addition of interaction terms between treatment group status and treatment delivered as measured by the GPS data. This model incorporates the time PCSOs spent in the hot spots and the number of visits to the hot spots and the interaction terms with

[4] A member of the research team tested the accuracy of the GPS recording by comparing the readings provided by the GPS trackers compared to manual recording of time spent in the hot spot.

the treatment condition, while controlling for the visits of the Police Constables to all 72 hot spots. We present the unstandardized and standardized coefficient models, and use the estimated marginal means (for more on marginal means, see McCulloch et al. 2008), in order to report the mean interaction responses, adjusted for all other covariates in the model. We plotted these responses on a scatterplot, and drew logarithmic trend lines for each group (treatment vs. control). These analyses may provide a novel approach to estimating the aggregation of the gains with each additional unit of patrol time.

Fourth, we used the WDQ to measure the displacement effect associated with treatment condition. We measured both the gross effect and the net effect for the experiment. A positive outcome in the gross effect indicates there was a decrease in crime just within the target area, without reference to any displacement. A positive outcome in the net effect indicates a decrease in crime in the target area that was greater than or different from changes in the control area. When the WDQ measure of the net effect is a negative number, it indicates there was a displacement effect; a positive number for the WDQ indicates a net crime reduction across the target hot spot and its (theoretical) local displacement measurement catchment zone.

Finally, we estimated the overall impact of the treatment group condition by using Guerette's (2009) Total Net Effects (TNE) model. The TNE gives the overall effect of the differences between the two randomly-assigned groups of hot spots by taking into account the variation in crime figures in the treatment areas compared to the control areas, while controlling for the displacement effect, if it exists. TNE is particularly valuable to assess what is the "bottom line" reductions in crime attributed to any intervention, such as the proactively assigned PCSO patrols in the 34 treatment group hot spots.

Statistical power

Statistical power is defined by Cohen (1988) as the probability of detecting a statistically significant difference in a comparison of two groups when such a difference truly exists. The study population of 72 hot spots used in this experiment offered us a sufficient level of statistical power to detect a true effect as unlikely to be due to chance. By using a statistical analysis package called Optimal Design (Spybrook et al. 2013), we estimated that the population of 72 hot spots was large enough to detect small to medium effects as significant if the cutoff point was set at .1 (a 10 % risk that the finding is due to chance), as long as the hypothesis we assumed was one-directional and the estimated power was 0.80. The power level of .80, which means that there is an 80 % chance that the test will detect true effects as within the significance cut-off level, is the conventional measure used in social science, although it is no more absolute than any other arbitrary threshold. We selected these parameters for our analysis in reference to the existing literature on hot spots (Braga et al. 2012).

Results

Baseline sample characteristics and treatment group similarity

The total number of incidents in the hot spots, broken down into calls for service (CFS) and crime counts, before and during the experimental period, are shown in Table 1. One

Table 1 Calls for service and crime data (baseline counts)

Calls for service (CFS)			Crimes		
Before			Before		
Incident category	*n* in treatment group	*n* in control group	Crime category	*n* in treatment group	*n* in control group
Antisocial behavior	5219	5570	Theft	412	393
Suspicious circumstances	2584	2327	Criminal Damage	279	285
Violence	1056	897	Burglary	196	174
Burglary	824	838	Robbery	20	13
Criminal damage	718	756	Sexual Offenses	32	31
Other calls for service	2994	2717	Common Assault	183	229
			GBH	25	23
			Other crimes	96	134
Grand total	13,000	13,500	Grand Total	1243	1282

year before the experiment began, the hot spots attracted 26,500 calls-for-service and 2,525 crimes. The majority (72.89 %) of the calls fell into the top five incident categories: antisocial behavior (40.71 %), suspicious circumstances (18.53 %), violence (7.37 %), burglary of dwelling (6.27 %) and criminal damage (5.56 %). The leading crime types, at 94.81 % of all crimes, comprised the top five major crime categories: theft (31.88 %), violence against the person (24.63 %),[5] criminal damage (22.34 %), burglary (14.65 %) and robbery (1.31 %).

The distribution of events at baseline values suggests that while the *minimum* requirement for being a hot spot was set as 36 incidents in a 12-month period, the *mean* number of CFS per hot spot at baseline period was 368.06 (SD = 252.39). The baseline frequency of calls per hot spot was asymmetrical (skewness = 1.291; SE = .283), with some eligible hot spots experiencing up to 619 incidents. The same pattern emerged for crime data, with the mean number of crimes per hot spot being 35.07 (SD = 24.54) at baseline, with a highly skewed distribution (1.263; SE = .283), ranging from 3 to 85 crimes. None of the pre-treatment between-groups comparisons were significant [$t = .4521(70)$; $p = .652$ (CFS) and $t = .4844(70)$, $p = .630$ (crime incidents)].

The GPS data for both PCSOs and Police Constables show high integrity in creating a big difference in PCSO patrol between treatment groups, while holding Police Constable patrol constant with no significant differences in PC measures between treatment groups (Table 2). This finding applies to both measures of police activity: the duration of patrol time in the hot spots (measured in minutes), and the number of discrete patrol visits per day (see definition above), for the year of the RCT.

As shown in Table 2, the treatment group hot spots received 135 % more PCSO patrol time than the control group hot spots: a mean of 37 minutes of PCSO patrol per day per treatment group hot spot versus about 16 minutes per day per hot spot in the

[5] Common assault (the lowest category of assault, including slapping and pushing), grievous bodily harm, harassment, etc.

Table 2 GPS data: means, standard deviations and *t* test scores

	Experimental hot spots (means and SD)	Control hot spots (means and SD)	*t*-test scores	*p*	Percent difference in treatment group
No. of patrol visits	4,761 (3,310.24)	4,794.87 (4,399.33)	.037	.971	
No. of hours	1,057.82 (1,101.6)	1,071.5 (2,222.5)	.033	.974	
PCSO patrol visits per day	**4.65 (1.07)**	**2.66 (1.17)**	**−7.581**	**.000**	**+75 %**
PCSO minutes per day	**37.41 (13.91)**	**15.92 (10.27)**	**−7.510**	**.000**	**+135 %**
PCs patrol visits per day	8.53 (8.65)	10.5 (11.13)	.817	.416	**(−21 %)**
PCs minutes per day	28.29 (13.87)	26.08 (13.07)	–.697	.488	**(+8 %)**

Bold entries shows significant results detected in the treatment group

control group. The treatment group hot spots also received 75 % more PCSO visits per day, on average, relative to the control group hot spots.

These findings show far more clearly than any recent hot spot experiment the exact magnitude of the difference in "dosage" measures of policing, as well as a means by which to estimate police compliance with the treatment program—which was substantial, but by no means perfect. While the PCSOs were tasked to provide 3 visits per day, for 45 minutes in total per day (that is, 15-minute patrols) in all hot spots, they delivered more visits, but fewer minutes, than required. In none of the comparisons of patrol minutes did the PCSOs deliver the treatment plan of 45 minutes per day, although they were only 18 % short of the goal (overall mean 37 minutes). In contrast, the number of visits exceeded the treatment plan by 57 %, with an "overdose" of 4.7 patrol visits per hot spot per day.

The best news was that, while Police Constables made many more visits to the hot spots than did the PCSOs, there was minimal difference between treatment groups either in the minutes or visits delivered by PCs. These "fully weaponized" (for Britain) Police Constables spent on average 28 minutes per day per hot spot across all 72 of the hot spots, with double the number of visits per day (8.5) than the PCSOs. But as Table 2 shows, the 21 % fewer patrol visits PCs made in the treatment hot spots relative to controls (8.53 PC visits per day per treatment hot spot vs. 10.5 for controls) was not a significant difference ($p = .416$). Neither was the 8 % more PC patrol minutes (28.29 per hot spot per day) in the treatment group hot spots than in the controls (26.0 per hot spot per day) ($p = .488$).

By combining the measures of PCSO and PC activity, we can answer an additional research question: how much more policing did the treatment group have, overall, than the control group? For patrol minutes, we can report that the treatment group had a combined mean of PC and PCSO patrol time of 65.7 minutes per day per hot spot, compared to a combined mean total of 42.0 minutes at each control group hot spot. For patrol visits, the comparable figures are 13 visits per day per treatment group hot spot compared to 13.16 per spot for the controls. Thus, while our experiment created a 56 % increase in total patrol time in the treatment group relative to controls, it created what was essentially no difference in (or slightly fewer) patrol visits in the treatment group.

These mean differences per hot spot create an interesting contrast to the pooled data across all treatment group hot spots. As reported in Table 2, the *total* mean number of PCSO and PC patrols and the *total* mean PCSO and PC time spent in hot spots in the treatment and control conditions are nearly identical (4761 vs. 4794 patrol visits and 1058 vs. 1072 hours, respectively). This finding, along with the nonsignificant differences in terms of the "amount" of visibility of warranted police constables in the hot spots, suggests that should any treatment effect be observed, it is unlikely to be attributed to *overall* combination of "hard" and "soft" police presence, but rather to large differences between treatment groups in "soft" policing delivered by the PCSOs.

Main effects of soft policing

Table 3 lists the difference-in-differences in means (DID) for calls for service and crimes per hot spot, while comparing the pre-treatment and post-treatment figures for each hot spot. As shown, the crime DID for the 34 treatment hot spots was −5.85 (SD = 10.190) and for CFS the DID was −207.7 (SD = 166.598), while the crime DID for the 38 control hot spots was −3.55 (SD = 13.647) and for CFS the DID was −173.37 (SD = 158.046). These figures represent, for crimes, a 64.8 % greater reduction in crimes per hot spot in the treatment group relative to the controls,[6] with an absolute decline of 16.00 % in mean crimes per treatment hot spot relative to the absolute decline of 10.5 % crimes per control group hot spot. Had the treatment hot spots experienced a 10.5 % decline, they would have had 130 fewer crimes. Instead, the treatment group had 199 fewer crimes, or 68 fewer crimes than if they had generated the same reduction as in the control group or a mean of 2.6 more crimes prevented per hot spot. They also indicate a 19.79 % relatively greater mean reduction in CFS per treatment hot spot than the control group's absolute percentage decline of 51.2 %, compared to a 61 % decline in the treatment group. Had there been a 51.2 % decline in the 34 treatment area baseline mean of 382 CFS per treatment group hot spot (the magnitude of the control group's drop), it would have yielded a new mean of 186.4 CFS per hot spot. Instead, the treatment group mean was 174.7 per hot spot during the experimental year, a mean difference of 11.7 calls per hot spot, or 399 CFS prevented.

As explained earlier, we converted these scores to standardized mean differences (Cohen's *d*), in order to be able to compare our results to the existing body of literature. These results for "soft" policing crime are highly comparable to the results for "hard" policing. The effect size for reported crime data yields an effect size of $d = -.189$ (95 % CI −.653, .27), and the effect size for calls for service is $d = -.211$ (95 % CI −.676, .252). These results mirror the overall treatment effect of hot spots policing found in Braga et al.'s (2012:58) systematic review ($d = -.184$).

Finally, we have counted nil reported incidents of assault or physical abuse against the PCSOs. Not one incident was identified in any of the crime reports or Calls for Service for the 72 hot spots during the 12 months of the experiment.

Measuring displacement

This section reports our analysis of displacement using the WDQ procedures described above. As Table 3 shows, the target hot spots in the treatment group experienced a reduction during the experiment to 1044 victim-generated crimes, from 1243 victim-generated crimes in the baseline year for a *gross* effect (GE) in the treatment group of 199 fewer crimes. The *net* effect (NE), which takes into account the baseline period for

[6] Calculated by first subtracting the treatment value from the control value, and the dividing the change by the control, multiplied by 100 to convert into percentages

Table 3 Difference-in-differences in means (DID) calls for service and crimes (baseline and post-random assignment): treatment versus control hot spots

	Control hot spots						Treatment hot spots						
	999 calls for service			Crimes				999 calls for service			Crimes		
Hotspot	Pre-	Post-	DID	Pre-	Post-	DID	Hotspot	Pre-	Post-	DID	Pre-	Post-	DID
1	1000	451	−549	74	72	−2	1	1009	318	−691	72	52	−20
2	979	619	−360	131	74	−57	2	911	270	−641	72	51	−21
3	788	213	−575	75	40	−35	3	835	460	−375	76	59	−17
4	708	520	−188	78	75	−3	4	697	275	−422	61	46	−15
5	665	250	−415	59	38	−21	5	664	289	−375	88	56	−32
6	600	226	−374	59	41	−18	6	619	422	−197	73	85	12
7	588	348	−240	55	45	−10	7	576	298	−278	46	42	−4
8	573	246	−327	48	56	8	8	554	161	−393	45	33	−12
9	543	147	−396	45	35	−10	9	542	327	−215	50	55	5
10	534	203	−331	67	68	1	10	542	232	−310	60	63	3
11	530	204	−326	63	53	−10	11	502	199	−303	44	58	14
12	489	308	−181	44	51	7	12	501	248	−253	52	38	−14
13	472	160	−312	40	27	−13	13	481	284	−197	26	20	−6
14	447	221	−226	41	43	2	14	432	152	−280	41	24	−17
15	400	173	−227	13	11	−2	15	423	165	−258	34	24	−10
16	370	282	−88	35	27	−8	16	388	82	−306	18	10	−8
17	352	151	−201	25	26	1	17	348	196	−152	26	23	−3
18	294	158	−136	16	24	8	18	321	130	−191	24	16	−8
19	285	216	−69	27	13	−14	19	287	109	−178	28	36	8
20	251	124	−127	21	32	11	20	283	276	−7	20	33	13
21	250	167	−83	23	29	6	21	245	119	−126	21	14	−7

Table 3 (continued)

	Control hot spots						Treatment hot spots						
	999 calls for service			Crimes				999 calls for service			Crimes		
Hotspot	Pre-	Post-	DID	Pre-	Post-	DID	Hotspot	Pre-	Post-	DID	Pre-	Post-	DID
22	244	60	−184	15	16	1	22	240	111	−129	26	31	5
23	237	226	−11	19	46	27	23	231	101	−130	29	14	−15
24	235	124	−111	16	15	−1	24	206	73	−133	21	21	0
25	230	119	−111	29	27	−2	25	191	106	−85	23	21	−2
26	195	104	−91	22	18	−4	26	168	86	−82	22	17	−5
27	168	100	−68	11	16	5	27	149	120	−29	40	32	−8
28	147	67	−80	20	10	−10	28	123	83	−40	24	15	−9
29	125	77	−48	11	10	−1	29	106	60	−46	19	17	−2
30	110	93	−17	14	12	−2	30	101	49	−52	15	11	−4
31	105	55	−50	10	13	3	31	93	42	−51	15	11	−4
32	105	96	−9	19	14	−5	32	88	38	−50	16	3	−13
33	93	39	−54	8	9	1	33	78	42	−36	6	6	0
34	87	52	−35	12	10	−2	34	66	16	−50	10	7	−3
35	87	83	−4	18	29	11							
36	83	37	−46	8	8	0							
37	69	129	60	4	6	2							
38	62	64	2	7	8	1							
TOTAL	13,500	6,912	−6,588	1,282	1,147	−135		13,000	5,939	−7,061	1,243	1,044	−199
Mean			−173.368			−3.553				−207.676			−5.853
SD			158.046			13.647				166.596			10.190

the target areas of both treatment and control (1243 and 1282, respectively) is (1243/1282) – (1044/1147), or 0.06. The positive number indicates that there was a decrease in crime in the treatment target area that was greater than the changes in the target control area.

Next, the WDQ was computed in order to determine displacement into the 50-meter buffer zones around the target areas, or the presence of displacement in relation to changes in the treatment and control areas (Eq. 1)[7]:

$$WDQ = \frac{(486/1147)-(533/1282)}{(1044/1147)-(1243/1282)} = -.134 \quad (1)$$

$$TNE = [1243*(1147/1282)-1044(+)533*(1147/1282)-486] = 58.98 \quad (2)$$

As shown in Eq. 1, the WDQ yielded a negative outcome, which indicates that there was a displacement effect. However, because the number is less than negative one (<−1), it means that the displacement was not greater than the reduction achieved in the intervention area. This outcome becomes clearer when looking at the overall impact of the RCT, as determined by the TNE model: the TNE gives the overall outcome of the project (Eq. 2). The TNE of this RCT were positive and relatively large (Guerette 2009), suggesting substantial treatment effect: The overall reduction in crime relative to control areas—adjusting for estimated displacement effects—was 59 crimes prevented (Eq. 2). That number would be higher if we did not count 100 % of the increase in crime in the displacement catchment areas as crimes that would have occurred in the hot spots had the extra PCSO patrols not pushed those crimes "around the corner" (Weisburd et al. 2006).

Outcome variations exist when observing the treatment effect on specific crime categories (Table 4), with the most pronounced effect found for burglaries, thefts, criminal damage and grievous bodily harm (against person) offenses: in all of these categories, not only were the total net effects above 20 crimes, but there was also a diffusion of the benefits effect that was greater than the reduction achieved in the intervention area, most notably in the cases of theft, burglaries and crimes against persons.

Estimating the treatment effect with frequency vs. duration in hot spots

To test the effect of treatment delivery with the GPS data, we used a linear model. The results are displayed in Table 5. We present the treatment condition as a predictor, and post-random assignment incident data as a dependent variable; we included the GPS-related variables in the model (covariates): the frequency of patrols per day and the number of minutes the PCSOs spent per day in the hot spots. We then included the interaction terms of each of these covariates with the treatment predictor. We also controlled for the visits of the Police Constables.

Model I examines the effect of PCSO foot patrols on calls for service. It shows that the main effect of randomly assigned treatment condition on CFS was only marginally significant ($\beta = .48$; $p = .13$) when all other variables are controlled for. The effect of

[7] We were not granted access to measure the presence of officers in the catchment areas via GPS data.

Table 4 Measures of diffusion of benefits (weighted displacement quotient)

Coefficients	Burglary	Theft	Criminal damage	Sexual **offenses**	Robbery	Common assault	GBH
GE	55	71	39	1	1	−6	1
NE	0.180	0.160	0.063	−0.208	0.272	−0.180	0.259
WDQ	1.503	0.674	1.553	2.335	0.528	0.882	1.711
TNE	67.172	103.031	42.091	−17.323	6.231	−65.428	20.391

GE gross effect; *NE* net effect; *WDQ* weighted displacement quotient; *TNE* total net effect

Police Constables, expected from Table 2, was negligible and nonsignificant. In contrast, the variations across the 72 hot spots in both duration (of the total PCSO time present in hot spots) as well as frequency (number of discrete PCSO visits) were significant predictors of CFS reductions [($\beta = .85$; $p \leq .01$); ($\beta = .52$; $p \leq .01$) respectively]. Total time spent in hot spots was slightly more important in Model I than the number of visits per day. Yet when we look at the interaction term between group assignment and frequency in Model 1, we see that with every 1 additional PCSO visit per day in the treatment hot spots, the number of calls for service decreases across the group by approximately 34 calls for service ($\beta = -.67$; $p \leq .1$), when all other variables in the model are controlled.

Similarly, when looking at the interaction effect of duration within the hot spots, Model 1 suggests that an increase of 1 additional minute in the hot spot is associated with 5 fewer calls for service ($\beta = -.87$; $p \leq .05$). This also happens to be the strongest predictor in the model. Overall, the model explains 32 % of the variance.

Model II uses crime reports as the dependent variable. It shows, once again, that group assignment is not statistically significant as a stand-alone predictor when all other variables are controlled. Police Constable visits, as expected, remain nonsignificant

Table 5 Estimating the treatment effect with GPS data (*n* patrols per day and *n* minutes per day): calls for service (*CFS*) and crime data

	Model I: CFS		Model II: Crimes	
	B (SE)	Beta	B (SE)	Beta
Treatment condition	116.072 (103.148)	0.48^	14.650 (17.14)	0.37
PCSO duration per day (minutes)	6.441 (1.746)	0.85***	1.083 (0.297)	0.88***
PCSO frequency of visits per day	42.243 (15.411)	0.52***	3.994 (2.735)	0.28*
PC visits	0.003 (0.004)	0.10	0 (0.001)	0.09
PCSO frequency of visits per day × treatment interaction	−33.593 (23.472)	−0.67*	−4.071 (3.846)	−0.50^^
PCSO duration per day × treatment interaction	−5.089 (2.22)	−0.87**	−0.695 (0.377)	−0.73**
(Constant)	−43.81 (48.981)		0.674 (8.542)	
R^2	30.20 %		24.70 %	
F-score	3.552***		4.546***	

* $p \leq .1$; ** $p \leq .05$; *** $p \leq .01$; ^$p = .13$; ^^$p = .15$

and negligible. Yet as we found in Model I for CFS, Model II shows that both duration and frequency of PCSO visits were significant predictors of crime reduction, and time spent at hot spots carries a greater weight [($\beta = .88$; $p \leq .01$), ($\beta = .28$; $p \leq .1$), respectively]. The interaction term for duration × treatment, again, had a greater impact on crimes compared to the interaction term for frequency alone [($\beta = -.73$; $p \leq .05$), ($\beta = .5$; $p \leq .15$), respectively], while the latter is only marginally significant. With every 1 additional visit to the treatment hot spots per day, the number of crimes declined by approximately 4, and with every additional 1 minute in the treatment hot spots the number of crimes was reduced by 0.7. Overall, the model explains 25 % of the variance.

We then computed the estimated marginal means, which report the mean response for each factor, adjusted for all other covariates in the model. We ran the analysis twice. One analysis focused on the interaction term of duration (therefore holding frequency constant). A second analysis focused on the interaction term of frequency of visits (holding duration constant). The covariates in the first analysis were fixed at the following mean values: police constables' visits = 3.488.8; PCSOs' minutes per day = 26.1. The covariates in the second analysis were fixed at the following mean values: police constables' visits = 3,488.8 and PCSOs' visits per day = 3.6. Scatterplots in Figs. 1 and 2 depict our analyses of the changes in the number of incidents with every additional minute spent patrolling the hot spots, or with every additional patrol per day, while controlling for the covariates. The scatterplots show logarithmic trend lines for each group (treatment vs. control).

As shown in Fig. 1, more frequent PCSO patrol visits in *treatment* hot spots are associated with *fewer* incidents, while more frequent PCSO patrol visits in the *control* hot spots are associated with *more* incidents. In nearly every comparison between the groups on the *x*-axis (patrols per day), the treatment patrols can be linked to fewer incidents, compared to control conditions. The trend lines seem to depart further away with more patrols in the hot spots, with the largest estimated difference between treatment and control conditions found at the upper-bound number of visits per day ($n = 8$).

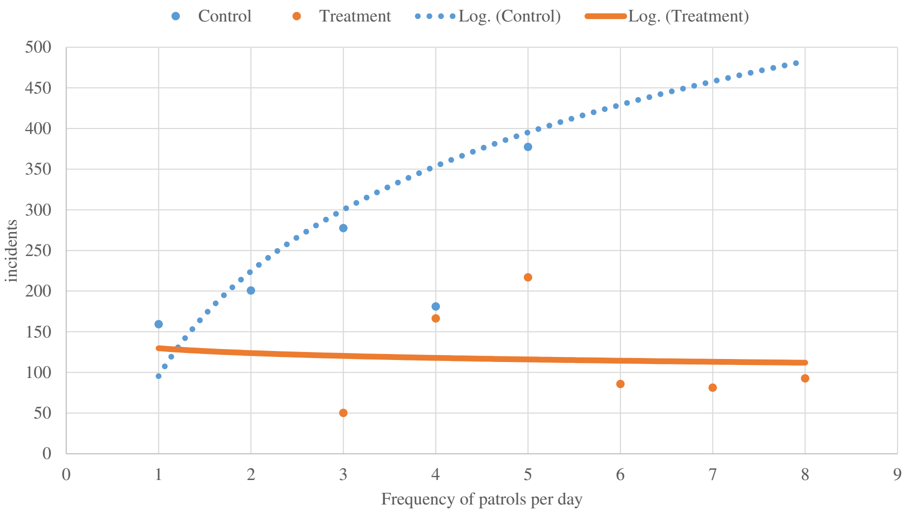

Fig. 1 Treatment vs. control conditions: Estimated marginal means (incidents) by number of PCSOs patrols per day

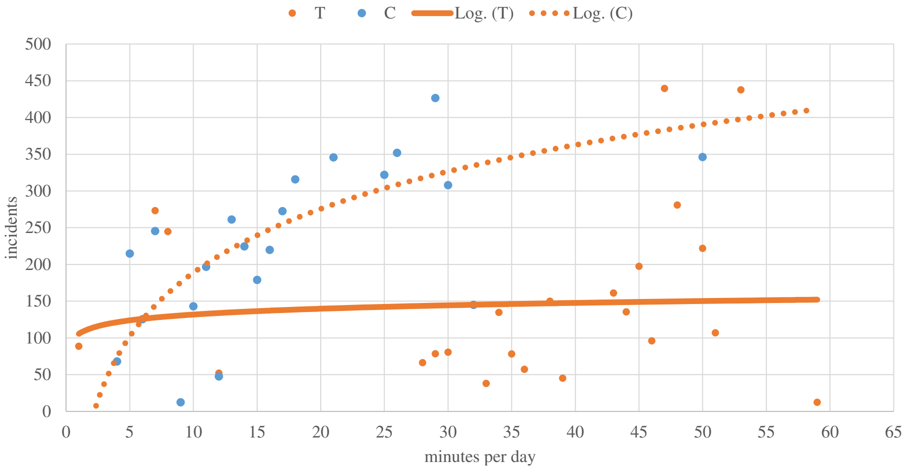

Fig. 2 Treatment vs. control conditions: Estimated marginal means (incidents) by number of PCSOs minutes per patrol

A similar pattern emerges in terms of time on patrol (Fig. 2), when comparing total time spent in the hot spots by PCSOs in the treatment hot spots to their time in the control hot spots. The trend line once again suggests that *more* time spent in control hot spots is associated with *more* incidents. There is also some increase in the number of incidents in the treatment hot spots (with more time spent in these hot spots); however, the pattern is less pronounced than in the control group.

Dosage to response benefit: a crime harm index analysis

Combining data from Tables 2 and 3, we computed the crime prevented in relation to soft policing activities. With 21 extra minutes of PCSO time per day in each of the 34 treatment hot spots = 714 minutes per day, × 5 days per week = 3,570 minutes, × 52 weeks = we see a total additional resource commitment of 185,640 minutes of PCSO time (3,094 hours) across all treatment hot spots. That seems roughly equivalent to the cost of two fulltime PCSOs, presently no more than £50,000 (Boyd et al. 2011). On that basis, we estimate that 68 fewer crimes and 399 CFS were prevented. In terms of PCSO patrol visits, the resource difference was 2 visits per day × 34 hot spots = 68 visits, × 260 days = 17,680 visits, or one crime prevented for every 260 visits, and one CFS prevented for every 44 visits.

Yet this dosage to benefit analysis is deeply flawed by a patently untrue assumption: that all crimes are created equal. The crimes prevented in this experiment ranged from theft to grievous bodily harm (GBH), which is close to attempted murder. What we lack is precise classifications of each offense, which would allow us to be more precise in weighting them according to the sentences for each offense type recommended by the Sentencing Council for England and Wales. The recommendation for GBH, for example, is 15 days imprisonment if the crime is committed without intent, but 1460 days if the crime is committed with intent. Because this is the widest range for any crime type, we offer a low and a high estimate for the CHI value of crime prevented based upon which type of GBH is selected.

Table 6 parses the difference-in-difference estimate of crimes prevented by category, and does not compute a total difference. Its purpose is to demonstrate the method used to produce Table 7, which shows the CHI value for each offense type derived from the Sentencing Guidelines for England and Wales. As Table 7 shows, the estimate varies widely depending on whether we assume that the GBH offenses are committed with or without intent. Overall, there was a reduction in CHI in nearly all crime-specific categories (except a minor increase in sex crimes and common assault).[8]

Assuming that the GBH crimes in Table 7 were not committed with intent, we see that the equivalent of two extra PCSO officers across 34 hot spots prevented about 86 days of potential imprisonment at each of 34 hot spots in the treatment group, or 2914 days of imprisonment (had every crime had just one offender who was convicted and sentenced at the starting point). That amount is about 8 years of imprisonment, at a minimum cost to the public of £35,000 per year, which is equal (× 8) to £280,000. Divided by the roughly £50,000 cost of the PCSOs, this makes the return on investment of PCSOs in hot spots as busy as Peterborough's approximately £5.6 saved for every to £1 invested in PCSOs.

If we assume that every GBH prevented was committed with intent, the cost to benefit ratio is far higher. As Table 7 shows, with that assumption, the total days of imprisonment prevented per hot spot over one year would be 360, times 34 hot spots = 12,240 days or 34 years of imprisonment (costing £1.17 million) with an investment of only £50,000, a return on investment of over £23 saved for every £1 invested in PCSO patrol.

By casting the results of this experiment in much more concrete terms for public understanding, the use of the CHI may be a far more powerful way to present these findings.

Discussion

Our experiment with one of England and Wales' largest forces attempted to "cool down" 34 of the hottest 72 crime and disorder hot spots in an ancient, Norman Cathedral city. The treatment and control group did not differ in the background levels of Police Constables' presence, largely comprised of responses to citizen-generated calls for service. The only difference between the treatment and control groups was more total time and separate visits to the treatment hot spots by "soft policing" civilian staff who hold no special powers of arrest, carry no weapons or handcuffs, and patrol on foot by themselves. These PCSOS visited the treatment hot spots 4.65 times per day, for an average of 37 minutes a day (or 8 minutes per visit), over 12 months.

We tracked these visits for their precise dosage by monitoring the GPS transponders in each PCSO's (and each PC's) body-worn radio. All hot spots were geo-fenced and the number of visits and the number of minutes spent in the hot spots, by all officers on the force, were recorded using personally-issued GPS trackers, allowing us to measure the precise dosage of the intervention. We compared these patrols to lower levels of

[8] While these two offense types did have very small increases, the purpose of CHI is to combine increases and decreases in a rational fashion to compute a bottom line. That is why, no matter what we assume about GBH, there is still a clear reduction in CHI for the experimental group. The sum of the days of potential imprisonment prevented across all of the crime types that produces our CHI prevention estimates.

Table 6 Difference-in-difference analysis by type of crime

Crime type	Treatment group				Control group				Treatment differences before–during	Control differences before–during	DID T vs. C
	Before count	During count	Before rate per hot spot	During rate per hot spot	Before count	During count	Before rate per hot spot	During rate per hot spot			
Theft	412	341	12.1	10.00	393	384	10.3	10.1	−2.1	−0.2	−1.9
Criminal damage	279	240	8.2	7.1	285	262	7.5	6.9	−1.1	−.06	−0.5
Burglary	196	141	5.8	4.1	174	149	4.6	3.9	−1.7	−0.5	−1.2
Robbery	20	19	0.59	0.56	13	15	0.34	0.39	−.03	+.05	−.08
Sex crimes	32	31	0.9	0.9	31	25	.82	.66	0	−.16	+.16
Assault	183	189	5.4	5.6	229	193	6.0	5.1	+.02	−.09	+0.92
GBH	25	24	0.74	0.71	23	29	0.6	0.76	−.03	+.016	−.19
Other	96	59	2.8	1.7	134	90	3.5	2.4	−1.1	−1.1	0

Table 7 Cambridge crime harm index impact analysis

Crime type	CHI days per crime	Difference-in-difference	CHI days prevented In treatment group per hot spot
Theft	20	−1.9	−38
Criminal damage	2	−0.5	−1
Burglary	15	−1.2	−18
Robbery	365	−0.08	−29.2
Sex Crimes	15	+0.16	+2.4
Common assault	1	+0.92	+0.92
Grievous bodily harm (intent) OR No intent	1460 15	−0.19	−277.4 OR −2.85
TOTAL	N.A.	N.A.	−360.28 OR −85.73

both independent variables in the control conditions. The police force was not made aware of the location of the control hot spots, in order to avoid contamination.

We measured the outcomes of the treatment differences by counting the number of emergency incidents and the number of victim-generated crimes before and after assigning the 72 hot spots into experimental and control groups. Finally, we measured for a possible displacement effect, addressing the continuing concern many have about hot spots policing despite a large body of literature to the contrary.

Police Community Support Officers were not authorized by Parliament with a specific purpose of "fighting crime", let alone to cool down hot spots, or target-harden vulnerable or criminogenic places. Their training, equipment and legal powers are ostensibly about reassurance and "soft" community policing, not "crime-fighting." Nor can they confront crime in the classic correspondence between the police and offenders: with the use of force as necessary, threat of immediate use of force, or both.

Nevertheless, the results of this experiment suggest that more time on patrol and visits to hot spots by PCSOs can in fact prevent crime and disorder. Particularly when controlling for the effect of Police Constables' presence in the hot spots, the foot patrols by lone PCSOs have caused clear reductions in the number of incidents and the number of crimes, compared to control conditions, even after accounting for minor displacement of crimes to the vicinity around the target areas, as shown in the WDQ model. The effect of this "soft policing" role, as measured by the standardized mean difference between treatment and control groups, is very similar to the average effects of "hard policing" increases in hot spots, as estimated in the systematic reviews by Braga et al. (2012) and Braga and Clarke (2014). The results further suggest that PCSO "saturation" in the treatment compared to control hot spots, even of these "soft" officers, was actually somewhat marginal, adding only 37 minutes per day per hot spot to the 28 minutes per day provided by Police Constables in response policing (Table 2).

What do these findings mean for both deterrence theory and police policy? For theory, they suggest that the probability of encountering an agent of the state is more important than the severity of the summary response an agent can make. For police policy, they suggest that the frequency and duration of visits deserves far more attention and further research. Yet the limitations of our research must lead to caution in drawing either conclusion as a settled answer to questions that may still have different answers in different communities or countries.

Implications for deterrence theory

Our main theoretical interest in this study was the question of whether "soft" policing in hot spots could achieve effect sizes comparable to those of "hard" policing in hot spots. Our finding that it did, at least in this experiment, has major implications for deterrence theory. What is it about the presence of a police uniform that cools down a crime hot spot? If it is the threat of immediate arrest and detention, that is a more severe response than PCSOs are able to invoke. Since they managed to reduce crime without that level of severity, the result suggests that deterrence may be associated more with any engagement by the state, with even weak agents deploying a powerful symbol of the entire police apparatus to which they are connected and can quickly mobilize with their radios.

While this study cannot pierce the walls of the "black box" of how offenders make decisions to commit crimes, it can observe that the objectively lower level of police powers in this experiment did nothing to reduce the magnitude of crime reduction the "soft" police achieved compared to what other experiments found with hard policing. We did not aim to understand offenders' decision-making processes through surveys or qualitative methods. We cannot measure to what extent people perceived the deterrence message carried by PCSOs as effective or not. Yet the data demonstrate behavioral changes in hot spot populations that took effect once PCSOs became more visible in the hot spots. In the treatment group compared to control hot spots, offenders did not commit as many crimes, nor were their crimes markedly displaced to other locations in the vicinity of the targeted hot spots. Incidents were prevented by a handful of unpredictable daily visits of "moving insignias" to the hot spots averaging 8 minutes, and lasting not more than 10–15 minutes. Even though a single PCSO cannot exercise *force,* the PCSO can present a clear message of *power*: "Beware. I can summon many police officers instantly, and testify against you in court." We therefore interpret the evidence to suggest that the threat of sanctions may not necessarily be about the severity of force each agent can deploy on the spot, but rather the agent's symbolic demonstration of the *power* of the police organization (Butterworth 1999; Deheane 1997; Pierce 1885). The extra presence in the hot spots of official power-holders, even though they were not weapon-holders, was causally related to crime prevention.

Another strong conceptual link between this policing tactic (i.e., soft power foot patrols) and deterrence theory is the repetition and intensity of the signal. There may well be a critical aspect of deterrence provided by foot patrol which is missed in vehicle-based patrols: direct and proximate co-presence of power-holders and members of the public (see Collins 2014 on the emotional impact of co-presence in Goffmanian interaction rituals, including restorative justice conferences led by police). When a police vehicle quickly drives by citizens on the street without even looking at them, public–police encounters are necessarily swift and lacking emotional impact. When a PCSO walks alone past people at a distance of under five meters, the likelihood of eye contact and perhaps even "hello" is probably far greater than if the same officer was in a car. Thus, if Police Constables are "'too busy' to walk the streets and prevent crime" (Marsden 2013) and are replaced by PCSOs, offenders may limit their criminal activities more than if hot spots are patrolled by invisible people inside cars. The repetitive and stable visits to the hot spots sent a clear message: "We see you often, and your face is familiar to us. We are coming back and will know you to be a law-obeyer, so please remain so!"

There is, however, a strong assumption in this theoretical analysis that deterrence depends on people's awareness of the increased number of police visits to the hot spots with increased minutes of patrol. We have no evidence to offer on that causal link. It is one possible component of a "black box" of the causal mechanism that our input–outcome analysis suggests is true. But *why* the PCSO patrols reduced crime cannot be revealed by our methods. We can say that the PCSOs caused the crime reductions, but we cannot describe the micro-mediation of the causal mechanism.

Implications for policing policy

One implication of these results addresses an intense debate in contemporary Britain. The PCSOs conducted single-person *foot* patrols, which had been a classic role for British "Bobbies" since 1829, but is no longer the case in most forces in England and Wales. Contemporary policing is characterized by a "reactive, fire-brigade" style of policing in automobiles (Wakefield 2006, p. 16). As one official report suggested, "bobbies on the beat are disappearing from swathes of the country and being replaced by community support officers", primarily due to budgetary constraints (Her Majesty's Inspectorate of Constabulary 2010). Instead, it is the PCSOs who walk the beats, while the predominant role of Police Constables is to *respond* to calls for service and attend non-emergency jobs by car (Wain and Ariel 2014). Thus, PCSOs are now the primary power-agents on foot in most forces in England and Wales. Read this way, our study joins Ratcliffe et al.'s (2011) Philadelphia foot patrol study. Our evidence strengthens support for the conclusion that this historic method of policing is still effective, at least in cities with substantial pedestrian populations on public sidewalks and plazas.

At first glance, this study may appear to be just one more experiment in hot spots policing with unsurprising results, given the accumulated evidence (e.g., Braga et al. 2012). Yet, we must recall that few if any recent lines of research in criminology have led to as much change in public policy as place-based initiatives (Weisburd et al. 2012, but cf. Weisburd 2015). Few policies developed through criminological research and development have even been proclaimed "successful" (Skogan and Frydl 2004), as hot spots policing has been. As we noted at the outset, "putting cops on the dots" is steadfastly turning into part of modern policing's DNA (Telep and Weisburd 2014). Yet, much like other matured areas of research, the more we know, the more granular and reliable our recommendations for policy can be.

We envisage that our findings have implications for greater precision and reliability on at least one broad domain of hot spots policing strategy: the allocation of patrol dosage. The question of dosage has fed the policing literature for some time now. How many officers does the force need (Wilson and Weiss 2012)? What is the optimal length of time for each visit of police to a hot spot (Koper 1995)? What is the right staffing model (Bittner 1990)? How do officers spend their time in the hot spots, or indeed any communities (Parks et al. 1999; see also Webster 1970)? These are clearly important questions, particularly in an era of austerity when more than 80 % of police budget is allocated to salaries. However, as Sherman (2013) argues, the degree of sophistication of the available evidence has been quite limited and we are left with rather poor studies on the tracking of policing. As this experiment demonstrates, that situation is now changing with the advent of officer-specific GPS tracking data.

These GPS data allow us to make two novel observations from this experiment. One is a phenomenon we call "Reiss's Reward," revealing a crucial difference between proactive and reactive patrolling. The other is a possible falsification of the hypothesis that the more time police spend in a hot spot, the less crime there will be.

Reiss's reward Our study allows us to speculate beyond the *Koper Curve* (Koper 1995), which did not distinguish between proactively and reactively generated visits. The GPS data reveal two distinct patterns of the link between patrol dosage and crime, implying we can hypothesize that crime reductions are driven more by the number of discrete patrol *visits* than by the average number of *minutes* of patrol they deliver (Table 5). That is just what we observe when the visits are proactively generated at random times. Yet, as Fig. 1 shows, we observe exactly the opposite phenomenon in the control group condition, where the patrol visits are reactively generated after someone has committed—and perhaps gotten away with—a crime. While the proactive visits may often surprise (and chasten) those considering the commission of a crime, the reactive visits may advertise the success of the last person who got away with a crime, thus encouraging more crime. As further shown in Fig. 1, the log-linear trend lines suggest an increasing gap as the number of patrols within the hot spots: a decrease in the number of events in treatment conditions and an increase in the number of events in the control conditions, while controlling for the effect of covariates. The more PCSOs visit the hot spots, the larger the effect on crime and disorder. We hereby label this gap as "Reiss's Reward," in honor of the late criminologist Albert J. Reiss, Jr., to whom the Oxford English Dictionary credits the invention of the word "proactive," and whose seminal work on the difference between proactive and reactive policing (Reiss 1971) stimulated the first hot spots patrol experiment to be carried out by two of his former students (Sherman and Weisburd 1995).

We suggest that "Reiss's Reward" explains the *increase* in the number of crimes and calls with increasing numbers of police visits across the 38 hot spots randomly assigned to the control group, even while more proactively generated patrol visits in the experimental condition predicted less crime. The "reward" is that, as Reiss might have said, "If police do *proactive* work to prevent crime, they will be rewarded with less *reactive* work to investigate crime." Under business-as-usual response policing conditions (as in the control group), PCSOs (or Police Constables in automobiles) visit hot spots *after* crimes have already occurred; that would explain why police visits and incident counts would be positively correlated with one another in the control conditions. In the treatment hot spots, however, we observed the opposite relationship. Because we know that in treatment group hot spots PCSO visits were proactively assigned (while in control group hot spots they were not), we can infer that the proactive PCSO visits *prevent* the events from happening in the first place. Thus in treatment conditions, more police patrol visits cause less crime. The treatment group trend line represents prevention, while the control group trend line represents reactive policing.

Total patrol time Unlike the pattern with frequency of visits, the duration of all patrol time at each hot spot (Fig. 2) seems to have little relationship with crime at treatment hot spots, while more time spent in control hot spots was still associated with more incidents. Thus, the "Reiss's Reward" logic we posited about the relationship between frequency of visits and crime in treatment hot spots cannot be found with more duration of patrol time. In the control hot spots, however, more incidents are associated with more time spent in the hot spots, as well as frequency. Crucially, however, when comparing the treatment and control groups, at virtually every point on the *x*-axis there seems to be clear differences between the two groups. Collectively, these two graphs do suggest that we need to focus our attention not just on duration, but also—and perhaps more so—on the number of times officers go to the hot spots. In our mind, pursuing this question is a major policy implication to explore more robustly in future research. The kind of research design needed to definitively compare frequency versus duration of hot spots patrol visits is, as ever, a large randomized controlled trial. A small trial of this comparison by Police Constables and PCSOs on foot patrol was recently conducted in central Birmingham, England (Williams 2016), and another in the Police Service of Northern Ireland (Goddard and Ariel 2014), but larger trials including motor vehicle patrols are also needed. No other direction for improved precision in preventive policing strategy seems more important than, in effect, randomly assigning the different levels of patrol duration identified non-experimentally as the *Koper Curve* (Koper 1995). The implication from this study is to deploy randomly assigned comparisons of different numbers of visits to two or more groups of hot spots while holding total minutes of patrol constant. Yet, even theorizing about such designs brings the discussion back to what is actually achievable in police agencies on the streets.

Implications for future research on hot spots and deterrence

Treatment integrity and hot spots' shapes

It was difficult for the officers to maintain treatment integrity in the sense of consistent delivery of 15-minute patrols, 3 times per shift, over time. Why was this the case? One major reason was the shape of our hot spots: polygons are difficult to manage. While there are ample statistical reasons for using polygons in the analysis of hot spots, they pose operational difficulties. Patrol officers are often drawn to vulnerable facilities or crime generators (see Bowers 2014), and are likely to "spread the patrol thinly" within the polygon. When the hot spots are shaped like polygons, it is also more likely that the patrol will "spill over" the boundaries of the hot spot (see Sorg et al. 2014), since officers are less likely to construct their patrols within what seem to be arbitrary digital lines on the map. In these cases, the expected treatment can fluctuate greatly between hot spots. Future research should revisit the polygon-ic approach and implement street-segments approach instead (see Weisburd et al. 2012).

Treatment delivery

A major lesson we learned from the use of GPS trackers is how much crime reduction can come out of relatively small increases in policing. When observing how little time

PCSOs spent in the hot spots compared to all other officers on the force, it is quite surprising that *any* significant treatment effect would be detected. Future studies on hot spots policing must now acknowledge that "saturation" or "more policing" means very little without the accurate estimation of dosage delivery, *in both treatment and control conditions,* for the ring-fenced team of officers who take part in the experiment as well as for all the other uniformed officers in the agency. Most hot spots policing experiments thus far were unable to measure separately the interaction between the effect of the proactive patrols of hot spots officers and the effect of all other police units; absence of suitable measures made those analyses impossible. With better technology, we were able to do so. Yet without body-worn videos (e.g., Ariel et al. 2014), we were unable to provide any qualitative analyses comparing PCSOs to Police Constables in their interactions with the public, something future research should pursue.

Additional study limitations

There are two additional limitations, beyond the concerns raised above, which future research should address. In the first place, we have no measure of actual delivery of policing *tactics*. What precisely did the PCSOs do while on patrol? How many stop-and-searches did they conduct? With whom have they engaged? How did they engage? How many arrests did they make? While we lacked funding to pursue these questions, what police do in hot spots is a practical as well as theoretical concern. A second limitation is our inability to explain how PCSOs affect perceptions of the police. As PCSOs were initially introduced to reduce the "reassurance gap" in British policing, we failed to test how these kinds of "soft" policing approaches affect fear of crime, collective efficacy and satisfaction with the police more broadly. Finally, the question of displacement of crime remains unresolved in public policy debates. Our study found evidence of diffusion of benefits to the vicinity *around* the hot spots, and this finding joins an increasing number of studies finding the same result (Bowers et al. 2011). Yet, we remain skeptical that our measurement of displacement is fully comprehensive. First, our study did not observe potential displacement to a not-so-near vicinity of the hot spots (but cf. Telep et al. 2014). Second, hot spot studies need to focus more thoroughly on the possibility of displacement in terms of modus operandi along with spatiotemporal transition of crime, which our data could not address.

Conclusion

Allocating crime hot spots to be patrolled by "soft" policing officers who lack arrest powers and weapons can reduce crime and disorder. Experimental evidence from Peterborough's hottest hot spots over a 12-month period shows that calls for service were reduced by approximately 20 % and victim-generated crimes were reduced by 39 % by PCSO patrols, compared to control conditions. Utilizing GPS tracking of all officers in the city, we held constant the number of patrols provided by Police Constables across the 72 hot spots. Based solely on extra patrols provided by PCSOs, the study finds that such "soft policing" reduced crime and calls for service without spatial displacement into the immediate vicinity of the hot spots. Since the magnitude of the effect sizes is highly comparable to what previous studies have found from increasing "hard" policing, we conclude that "soft" policing can achieve comparable crime reductions without displaying a threat of immediate use of force.

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