J Exp Criminol (2013) 9:213–244
DOI 10.1007/s11292-012-9170-z

A randomized test of initial and residual deterrence from directed patrols and use of license plate readers at crime hot spots

Christopher S. Koper · Bruce G. Taylor · Daniel J. Woods

Published online: 6 January 2013

Abstract
Objectives To test the effects of short-term police patrol operations using license plate readers (LPRs) on crime and disorder at crime hot spots in Mesa, Arizona.
Methods The study employed a randomized experimental design. For 15 successive 2-week periods, a four-officer squad conducted short daily operations to detect stolen and other vehicles of interest at randomly selected hot spot road segments at varying times of day. Based on random assignment, the unit operated with LPRs on some routes and conducted extensive manual checks of license plates on others. Using random effects panel models, we examined the impact of these operations on violent, property, drug, disorder, and auto theft offenses as measured by calls for service.
Results Compared to control conditions with standard patrol strategies, the LPR locations had reductions in calls for drug offenses that lasted for at least several weeks beyond the intervention, while the non-LPR, manual check locations exhibited briefer reductions in calls regarding person offenses and auto theft. There were also indications of crime displacement associated with some offenses, particularly drug offenses.
Conclusions The findings suggest that use of LPRs can reduce certain types of offenses at hot spots and that rotation of short-term LPR operations across hot spots may be an effective way for police agencies to employ small numbers of LPR devices. More generally, the results also provide some support for Sherman's (1990) crackdown theory, which suggests that police can improve their effectiveness

C. S. Koper (✉)
Department of Criminology, Law and Society, George Mason University,
4400 University Drive, MS 6D12, Fairfax, VA 22030, USA
e-mail: ckoper2@gmu.edu

B. G. Taylor
NORC at the University of Chicago, Bethesda, MD, USA

D. J. Woods
Police Executive Research Forum, Washington, DC, USA

in preventing crime through frequent rotation of short-term crackdowns across targets, as it applies to hot spot policing.

Keywords Crackdowns · Hot spots · License plate readers · Policing · Randomized experiment · Technology

Introduction

In recent years, police have increasingly sought to improve their efficiency and effectiveness through the use of new technologies. One relatively new surveillance and information-gathering technology that is rapidly diffusing throughout American policing is license plate recognition technology. License plate readers, commonly referred to as LPRs in the United States, are devices that automatically scan automobile license plates and check them against databases on stolen vehicles and other information. As with many policing technologies, little evidence is available on whether this technology helps police to reduce crime and how it should be best utilized. In this paper, we address these issues through a randomized experiment testing the crime control effects of short-term patrol operations with LPRs at crime hot spots in Mesa, Arizona. For 15 successive 2-week periods, a specialized squad of officers conducted short daily operations to detect stolen and other vehicles of interest at randomly selected hot spot road segments at varying times of day. These operations were conducted both with and without the LPR devices based on random assignment. In addition to assessing the impact of LPR use on crime, we also consider the ramifications of the experiment more generally for hot spots policing and for Sherman's (1990) crackdown theory, which suggests that police can maximize their effectiveness in preventing crime through frequent rotation of short-term crackdowns across targets.

Background

The use of license plate reader technology in policing

LPRs are high-speed camera and information systems that read vehicle license plates in real-time using optical character-recognition technology and check them instantaneously against databases which may contain license plate information on stolen vehicles, vehicles linked to fugitives and criminal suspects, and other vehicles of interest. LPRs can be installed in police cars for mobile surveillance or set at fixed positions. Although relatively new in the United States, LPR technology has been used since the 1980s in Europe to prevent crimes from vehicle theft to terrorism (Gordon 2006). LPR use is particularly extensive in the United Kingdom, where all police forces in England and Wales now have LPR capability (PA Consulting Group 2006).[1]

LPR use is also growing rapidly in the United States. Recent surveys indicate that over a third of large U.S. police agencies use LPRs, and many more are interested in

[1] In the United Kingdom, this technology is referred to as automated number plate recognition technology (ANPR).

acquiring them (Koper et al. 2009; Lum et al. 2010). However, the vast majority of agencies using LPRs—86 % according to one survey—had no more than 4 of the devices as of 2009 (Lum et al. 2010). This is likely due in part to the cost of LPRs, which generally run from $20,000 to $25,000 per unit. Consequently, assessing the utility of LPR purchases and developing optimal uses of LPRs are important issues for police.

In principle, use of LPRs may help police reduce crime through deterrence and the apprehension (and thus incapacitation) of offenders.[2] However, prior studies of LPRs conducted in the United Kingdom and North America have focused largely on the accuracy and efficiency of the devices in scanning license plates and on their utility for increasing arrests, recoveries of stolen vehicles, and seizure of other contraband (Cohen et al. 2007; Maryland State Highway Authority 2005; McFadden 2005; Ohio State Highway Patrol 2005; PA Consulting Group 2003; Patch 2005). Although these studies have demonstrated that use of LPR technology can increase arrests and recoveries of stolen automobiles and contraband, there is scant evidence on whether LPR use actually reduces crime.[3] As noted by Lum et al. (2010, 2011a), when evaluating policing technologies, it is important to distinguish between how they affect police efficiency (e.g., increasing license plate scans and recoveries of stolen vehicles) and how they affect the ability of police to reduce crime.

To date, only two evaluations have examined the impact of LPR use on crime. Both have been conducted in the United States and have focused on small-scale LPR deployment. In one of these studies, Lum and colleagues (2010, 2011a) tested the effects of LPR patrols in 15 hot spots of automobile-related crime located in two neighboring jurisdictions in Virginia—Alexandria city, from which 7 hot spots were selected, and Fairfax County, from which 8 were selected. In each jurisdiction, 2 officers conducted separate LPR patrols in marked police units for 26–30 days spread over a number of months. On these days, each LPR officer was assigned to patrol 5 hot spots, chosen at random from the experimental hot spots in that officer's jurisdiction. The officers generally patrolled each of their assigned locations once per day, conducting a mix of roaming and fixed surveillance with the LPRs for up to 30 min in each spot. Information loaded into the LPR systems for comparison to scanned license plates consisted primarily of state and national data on stolen vehicles. Over the course of the experiment, the officers conducted 280 LPR patrols at the experimental hot spots in Fairfax (for an average of 35 patrols per location) and 256 at the

[2] In addition to helping police detect auto thieves and other wanted persons, LPRs may also aid criminal investigations by providing records of vehicles that were in or near a crime location around the time of a criminal act.

[3] A limitation to the use of LPR technology for apprehending vehicle thieves is that thieves may often abandon stolen vehicles before the vehicles are reported stolen and entered into police data systems. In Mesa, Arizona (our study location), we estimate that only one-third of auto thefts are reported within 3 h of occurrence, based on analysis of data from 2006 and 2007 (these are rough estimates because the time of many auto thefts can only be approximated). These delays reflect lags in the discovery of vehicle thefts (e.g., a vehicle stolen at night might not be discovered as missing until the following morning) as well as delays in reporting by victims after their discovery of a theft. Further, some vehicle thieves switch the license plates of stolen vehicles with those stolen from other vehicles; victims who have had their plates swapped for those of a stolen vehicle may be unaware of this for a long period, thus providing thieves with additional time to operate and abandon their stolen vehicles. For a discussion of other technical limitations to LPR technology, see Taylor et al. (2011a, 2012).

experimental hot spots in Alexandria (for an average of 37 patrols per location). In Fairfax, these visits were spread over 58 days; hence, each experimental hot spot received a 30-min LPR patrol roughly once every 1–2 days for about 2 months. In Alexandra, where the intervention took place over 100 days, each experimental location received a 30-min LPR patrol once every 2–3 days on average for over 3 months.

In comparison to randomly assigned control hot spots in the two jurisdictions, the experimental LPR locations showed no change in either auto-related crimes (auto theft and thefts from autos) or other types of crime (Lum et al. 2010, 2011a). Although the experimental dosage was relatively low and the LPR databases were limited to information on stolen automobiles, the study's authors argued that the results underscore the need for further testing to show whether LPR use can reduce crime and to identify the most optimal ways of using LPRs.[4]

The only other study to examine the impact of LPR use on crime—and the only other randomized experiment assessing LPR use—is the Mesa study discussed in this paper. The Mesa experiment, which is described in detail below, involved a specialized auto theft unit that worked in hot spot locations both with and without LPRs based on random assignment. Results reported elsewhere (Taylor et al. 2011a, 2012) show that the unit was more likely to detect and recover stolen vehicles and apprehend auto thieves when using the LPR devices. However, use of the LPRs did not reduce auto theft in the targeted locations. In the study presented here, we examine the effects of LPR use on a broader range of offenses.

Studies of other surveillance technologies

Despite the null results of the LPR studies to date, research on other surveillance technologies provide some limited grounds for believing that LPR use can reduce crime. Most notably, Welsh and Farrington's (2008) meta-analysis of over 40 studies on closed circuit television (CCTV) surveillance systems shows that these systems reduce vehicle-related crimes in parking lots and public garages. CCTV has not been as consistently effective in other types of settings (e.g., center city areas and public housing) and for other types of crime. Further, the great majority of these studies have been conducted in the United Kingdom. Studies of CCTV and other camera systems in the United States have yielded more mixed results (King et al. 2008; LaVigne et al. 2011; LaVigne and Lowry 2011; Mazerolle et al. 2002; Musheno et al., 1978). Nonetheless, a number of the U.S. studies (including some completed after the Welsh and Farrington review) suggest that surveillance technologies can reduce a variety of crimes, including violent offenses, in high-crime areas—particularly if the cameras are monitored, resulting in prompt police responses to observed events, and present in higher concentrations (LaVigne et al. 2011).

These findings may have positive implications for LPR use. When LPRs are mounted on patrol cars, officers can react quickly to the identification of vehicles of interest. And while passers-by may not understand what type of camera the officer is using, it is obvious that the officer is monitoring the device, which may enhance its

[4] Note that the study tested the impact of the LPR patrols in hot spots relative to normal patrol. It did not assess hot spot patrols with and without LPRs, as we do here.

deterrent value. In addition, moving LPRs around various high-crime locations increases the coverage of the cameras while potentially increasing their deterrent value by raising both perceptions of their prevalence and uncertainty about their locations.

Deployment of license plate readers at crime hot spots

Like the Lum et al. (2010, 2011a) Virginia LPR study, our Mesa study examines the effectiveness of LPR use in the context of "hot spot" policing—i.e., police efforts focused on small areas or very specific places (e.g., addresses, intersections, or street blocks) where crime is concentrated (e.g., Sherman et al. 1989; Mastrofski et al. 2009; Weisburd 2008). It is well known that crime is highly concentrated geographically. Studies in a number of cities have shown, for example, that approximately half of crime occurs at 5 % or less of a city's addresses and intersections (e.g., Pierce et al. 1988; Sherman et al. 1989; Weisburd et al. 2004) and that the concentration of crime at these places is generally stable over time (Braga et al. 2010; Weisburd et al. 2004, 2009).[5] Moreover, evidence from several empirical evaluations suggests that directed patrol, problem-solving, and targeted enforcement activities can reduce crime and disorder at these locations without obvious signs of displacement (see reviews in Braga 2007; Lum et al. 2011b; National Research Council 2004; Weisburd and Eck 2004; for more recent studies, see also, e.g., Braga and Bond 2008; Ratcliffe et al. 2011; Taylor et al. 2011b; Telep et al. 2012).

Hot spots are logical places for deployment of LPRs. Like many crimes, auto theft is concentrated geographically in a manner linked to various environmental characteristics (Barclay et al. 1995; Fleming et al. 1994; Henry and Bryan 2000; Kennedy et al. n.d.; Plouffe and Sampson 2004; Potchak et al. 2002; Rengert 1996; Rice and Smith 2002).[6] The odds of detecting auto thieves and other wanted persons through the use of LPRs would thus seem to be greatest in these and other crime hot spot locations. Focusing LPR use in hot spots should also maximize the deterrent value of displaying the cameras; indeed, it is conceivable that patrol cars with mounted cameras generate greater deterrent effects in hot spots than do patrol cars not so equipped.

As noted above, however, most police agencies with LPR technology in the United States possess no more than four of the devices. Given this limitation, how might police best utilize their LPRs at hot spots? One possibility, grounded in Sherman's

[5] These locations are often nodes for business, leisure, and/or travel activities, and they have features or facilities that create criminal opportunities and facilitate offending (Eck and Weisburd 1995). In the language of routine activities theory (Cohen and Felson 1979), they are places that bring together motivated offenders, suitable targets, and an absence of capable guardians. Examples include locations with bars, convenience stores, parks, bus depots, apartment buildings, parking lots, shopping centers, motels or hotels, adult businesses, and the like (e.g., Braga et al. 1999: 551–552; Sherman et al. 1989: 45; see also Eck and Weisburd 1995).

[6] Rice and Smith (2002), for example, report that vehicle theft is higher in areas close to pools of motivated offenders, where social control mechanisms are lacking, and where there are suitable targets such as bars, gas stations, motels, and other businesses. Further, a number of studies have identified non-residential locations as hot spots for vehicle theft, including: parking lots close to interstate highways (Plouffe and Sampson 2004), high-traffic areas (Rice and Smith 2002), areas near schools (Kennedy et al. n.d.), mall parking lots (Henry and Bryan 2000), and entertainment venues (Rengert 1996).

(1990) crackdown theory, is to deploy LPR patrols to hot spots for short crackdowns on a rotating basis.[7]

Crackdown theory and deployment of license plate readers

Police crackdowns entail sudden increases in police presence, sanctions, and/or sanction threats focused on certain offenses or all offenses in specific places (Sherman 1990; see also Scott 2003). Directed patrol and fixed police presence at crime hot spots, for instance, can be conceptualized as geographically-focused crackdowns that increase police presence in high-risk locations. Similarly, the introduction of surveillance technology into an area may be viewed as a sort of crackdown that raises the risk of sanctions for offending in that location.

As described by Sherman (1990), a crackdown can produce initial deterrence while it is in operation and residual deterrence that lasts for some time after it has ended. To explain the latter, Sherman postulated that crackdowns increase perceptions of risk among offenders, and that it may take some period of time for those heightened risk perceptions to diminish even after a crackdown has ended.[8] Nagin (1998: 9–12), citing research on expected utility theory, also suggests that crackdowns may generate deterrence by increasing uncertainty about sanction risks and thus increasing the variance of risk estimates. This may reduce offending through a process that behavioral decision theorists refer to as “ambiguity aversion” (e.g., see Camerer and Weber 1992). Through one or both of these mechanisms, the effects of a crackdown may thus persist beyond the end of the crackdown, until such time as offenders learn that the level and/or uncertainty of risk have declined.

Sherman (1990) also found evidence that the effects of crackdowns are conditioned by decay in crackdown efforts as well as decay in both initial and residual deterrence. Crackdown decay refers to a decline in police effort over time, which could be planned (e.g., a diversion of resources) or due to a natural regression to the mean in police effort (e.g., due to fatigue or a loss of focus and momentum in the crackdown effort). Initial deterrence from police crackdowns can also decay if a crackdown is maintained for too long; in other words, offending may begin to rise again (though not necessarily to pre-crackdown levels) even while the crackdown is still in effect. Several of the crackdowns included in Sherman’s review showed evidence of such decay. Presumably, this occurs because offenders adapt to police actions and/or develop a better sense of the level and variability of sanction risks. Similarly, the residual impacts of crackdowns will fade as offenders learn through direct and vicarious experiences that the level and variability of sanction risks have returned to pre-crackdown levels.

Based on these patterns, Sherman (1990) suggested that police can maximize the effectiveness of crackdowns by rotating brief crackdowns across targets on a frequent, unpredictable and repetitive basis. He argued that this would maximize uncertainty among offenders, potentially producing enough initial and residual

[7] Similarly, in their study of CCTV in Cincinnati, Mazerolle et al. (2002) found short-term effects on antisocial behavior that they argued might be optimized by rotating CCTV across crime and disorder hot spots every 1–2 months.

[8] Some studies of hot spot policing, for instance, have shown residual crime suppression effects lasting well beyond the interventions studied (e.g., Braga et al. 1999; Weisburd and Green 1995; Taylor et al. 2011b).

deterrence to reduce crime more than either a steady dose of police effort or standard triage approaches (e.g., focusing resources on some problems and de-emphasizing others). This would also optimize the use of police resources in controlling crime.

To our knowledge, there have been almost no attempts to implement or evaluate a rotating crackdown strategy like that proposed by Sherman.[9] Key issues for operationalizing such a strategy include the types of problems and places for which crackdowns are most effective, the efficacy of different crackdown tactics, the rate of decay in initial and residual deterrence for different types of crimes, displacement or diffusion of benefits from crackdowns, and the most optimal dosages for crackdowns, measured in terms of officers deployed and operation length. To address such issues, Sherman advocated for experiments with varying target problems and areas, crackdown tactics, and dosages.

The Mesa LPR patrol operation described below provides one illustration of a rotating crackdown strategy. Officers conducted patrols with and without LPRs in selected hot spots that were rotated every 2 weeks. This rotation enabled the officers to put concentrated efforts into a relatively large number of hot spots in a relatively short period. If these dosages were sufficient to generate initial and residual deterrence, then the brief nature of the operations may have also minimized the likelihood that the operations would exceed a point of diminishing returns in each location. Below, we examine the initial and residual effects of the LPR and non-LPR patrols and consider the implications of the results for LPR deployment, police crackdowns, and hot spot policing more generally.

Research design, data, and methods

Study site

We conducted this study in the city of Mesa, Arizona, with the Mesa Police Department (MPD), an agency of about 800 sworn officers. With a population of approximately 460,000, Mesa is one of the United States' fastest-growing cities (since 2000, it has had population growth of about 13 %) and currently ranks as the 38th largest. Demographically, 84 % of the city's population is white and 26 % is of Hispanic ethnicity. The city is spread over 136 square miles, giving it a sprawling and suburban character. In 2008 (the year in which the study began), Mesa had nearly 20,000 UCR part I crimes, though its crime rate was well below the national average for cities in its size range (250,000–499,999) (calculated from the Federal Bureau of Investigation's Uniform Crime Reports at http://www2.fbi.gov/ucr/cius2008/index.html).

Description of the intervention

From August 2008 through March 2009, MPD and the research team implemented a 30-week experiment designed foremost to test the extent to which LPR use enhances the ability of police to recover stolen automobiles, apprehend auto thieves, and reduce

[9] One study of note, however, is Katz et al.'s (2001) evaluation of a quality-of-life police initiative in Chandler, Arizona, that was rotated multiple times across a number of target areas.

auto theft (Taylor et al. 2011a, 2012). At the time of the experiment, MPD had 4 LPR devices. Based on prior experience with the LPRs and consideration of practices used by other agencies, MPD chose to deploy its LPRs with a specialized vehicle theft unit consisting of four officers (and one supervisory officer who was not involved in the actual street work). The officers worked together in four cars; two were unmarked smaller cars that did not look like police cars (e.g., Chevy Cavaliers), one was an unmarked patrol car, and one was a marked patrol car without a light bar. The unmarked cars provided more investigative options for the unit (e.g., for surveillance), while the patrol cars (particularly the marked one) were used for pursuing suspects. All four vehicles were equipped with an LPR system that contained two mobile cameras mounted on the rear of the vehicle. The data loaded into the LPR systems consisted primarily of state-level data on stolen vehicles, stolen license plates, and other vehicles of interest (e.g., vehicles linked to robberies). The data also contained information on warrants for a few nearby localities (Tucson and Gilbert) but not for Mesa itself. The LPR systems did not have wireless, real-time connections; thus, information was loaded into the system manually on a daily basis. However, the officers could also add information into the system based on alerts while they were in the field.

Working closely with MPD, the research team developed a standardized approach to implementing the LPR operations uniformly across a group of assigned routes (i.e., hot spots), which, as described below, consisted of high-risk roadway segments averaging about half a mile in length. Each route assigned to the LPR intervention was patrolled by the auto theft unit using the LPR devices for approximately 1 h per day for 8 days, spread over a 2-week period. The unit conducted these operations from Wednesday through Saturday between the hours of 3:00 p.m. and 1:00 a.m. During each daily patrol, the general strategy was for officers to first “sweep” the location, checking parking lots and side streets along the route, and then conduct fixed surveillance for the remainder of the hour, with officers positioned along different sides and parts of the route. The unit would then move to another route to conduct the next operation. (The unit focused on proactive work and did not generally respond to calls for service.)

The officers would work a set of assigned routes in this manner for a 2-week period and then move to another set of routes for the following 2-week period. While moving the unit frequently among different locations had the potential to maximize initial and residual deterrence, the decision to work each route for 2 weeks was also based on a number of practical considerations: MPD’s desire to spread the use of the LPRs over several locations; the research team’s desire to include a large sample of hot spots in the experiment; and the auto theft unit’s judgment, based on past experience, that working a given location for more than 2 weeks brought diminishing returns with regard to finding stolen automobiles.

During each 2-week period, the vehicle theft unit also patrolled a second group of assigned routes without using the LPRs. In these locations, the officers patrolled and conducted manual license plate checks, entering selected plate numbers into their mobile computers. When working these routes, the officers used the same initial sweeping strategy, though they had to proceed more slowly, with frequent pauses to manually check license plates. The officers then focused their efforts on particular parts of the routes by roaming around these areas to maintain speeds with the local traffic or by parking at traffic lights to check plates. When doing manual checks, the officers conducted less stationary surveillance, because that limited their ability to see and check license plates of cars passing by rapidly. Although the officers did not use the LPR cameras in this second group of routes, it is important to note that the LPR cameras remained mounted on their cars when working these locations (this was necessary because the unit worked both LPR and non-LPR routes during each day of the study period). Table 1 summarizes the key aspects of the LPR and non-LPR patrols.

For this analysis, we conceptualize these operations as a series of brief directed patrol crackdowns that increased police presence, surveillance, and sanction risks at the hot spots. The police presence in this case was relatively passive; the officers focused on surveillance rather than order maintenance or aggressive enforcement, and they generally did not interact with citizens unless they detected a license plate that matched their database or witnessed other criminal activity. Further, as noted, most of the officers' cars were unmarked, which diminished the visibility of their presence somewhat. Nonetheless, while one patrol car was unmarked and the other did not have a light bar, both were readily recognizable as police cars. In addition, the mounted LPR cameras were very prominent on all the cars, which likely made passers-by suspect that these were police conducting speed enforcement or other activities.[10] The clustering of the officers in a small location, combined with their roving tactics (often done at slow speeds in parking lots and on side streets), also made their presence more obtrusive. Finally, the officers' regular presence at the hot spots over the course of 2 weeks should have made them more noticeable to regular users or passers-by of the locations.

In our analyses, we allow for differential effects stemming from the LPR and non-LPR patrols. Because the LPR cameras were mounted on the vehicles at all times, any deterrent effects stemming from the visibility of the cameras would have been operative in both patrol modes; hence, we cannot compare the impacts of patrols with and without mounted cameras. However, effects of the patrols could have differed by mode of operation based on how LPR use affected both the officers' tactics (as described above) and their likelihood of detecting auto thieves and other wanted persons.

Selection of hot spots

The study areas consisted of 117 roadway segments—deemed "hot routes"—that were identified as roads where vehicle thieves were most likely to drive stolen vehicles (including dumping/destination points).[11] The hot routes were 0.6 of a mile in length on average, contained a mixture of residential and business areas, and

[10] Indeed, there were a number of instances in which passers-by stopped to question the officers (sometimes in brazen ways) about what they were doing. On one occasion, a passing motorist circled one of the unmarked cars a number of times and threw rocks at it, not realizing that an officer was inside the vehicle (the windows were tinted). These incidents were perhaps manifestations of a heated public controversy that had arisen in Arizona around this time regarding the use of speed cameras on roadways.

[11] Based on discussions with MPD and analysis of MPD data, we elected not to use the LPR devices in specific vehicle theft hot spots due to the lag that often occurs between the theft of a vehicle and the reporting of that theft to police (see note 3). Therefore, we focused on roads where auto thieves are most likely to drive stolen vehicles.

Table 1 Elements of LPR and non-LPR hot route patrols

	LPR Patrol	Non-LPR ("Manual") Patrol
LPR units mounted	X	X
LPR units activated for license plate reads	X	
Manual license plate checks (LPRs not activated)		X
Initial sweep of the route area	X	X
More emphasis on fixed surveillance	X	
More emphasis on roaming surveillance		X

The LPR and non-LPR patrols were conducted by the same specialized auto theft unit

included different types of roads (interstate roads, highways, and residential streets).[12] Two-thirds of the routes were selected based on geographic analysis of theft and recovery locations. Using data on 1,668 automobiles that were both stolen and recovered in Mesa during 2007, and using routes that provided the shortest travel time between each corresponding theft and recovery location as a likely estimate of thieves' "journey after crime" (Lu 2003),[13] we selected 78 roadways that were estimated to have been used at least twice by vehicle thieves during the year.[14] [15] The other one-third of the 117 routes was selected based on interviews with detectives and officers. The routes nominated by detectives and officers were included to assure that the sample of routes was also based on the latest intelligence collected by MPD, much of which is not reflected in official crime statistics and is often of a more qualitative nature. Thus, in defining the sample, we sought a balance between having a sample large enough to provide reasonable statistical power, selecting routes that were sufficiently active, accounting for officer intelligence, and garnering officer support for the project.

For analytic purposes, we also created a 500-ft buffer around each hot route. Crime counts for each route thus include crimes that occurred on the specific street of each hot route or within 500 ft of the route. This allowed us to include parking lots,

[12] In defining the routes, we divided roads into smaller segments based on natural divisions (i.e., intersections and other natural breaks).

[13] The journey after crime is an offender's trip with the stolen vehicle in order to realize its expected utility, such as a trip to sell or strip the vehicle, a trip to another offense, or a joy-ride (Lu 2003). In a study of vehicle theft offenses in Buffalo, Lu (2003) found that vehicle thieves' trips from vehicle theft locations to vehicle recovery locations were mostly local in nature, with travel distances significantly shorter than those of randomly simulated trips. Lu found that the difference in travel direction between observed and simulated trips was a combined result of both criminals' spatial perceptions and a city's geography (e.g., street networks). Lu thus recommended that police focus on nearby locations when responding to vehicle thefts.

[14] Estimated vehicle theft trips on these roadways ranged from 2 to 57. This approach is not without its limitations given that it was based on recovered vehicles only, leaving out a considerable percentage of stolen vehicles that were never recovered. If the routes used to steal unrecovered automobiles differ systematically from those used to steal recovered ones, then our sample may not be a representative sample of all hot routes. But while this may affect the generalizability of the findings, it does not affect the internal validity of the study.

[15] This analysis was conducted by Dr. Yongmei Lu of Texas State University.

apartment complexes, and side streets that officers covered in their roaming operations along the routes.[16] [17]

For the purposes of this analysis, the hot routes also serve well as general crime hot spots because they were concentrated in high crime areas of the city. During 2007 and 2008, the hot routes and their buffer areas experienced an annual average of 8,855 UCR part I crimes, which accounted for 42 % of the city's total.[18]

Experimental design and implementation

We evaluated the impacts of the auto theft patrols on crime using a randomized experimental design. Randomized experiments provide the most rigorous means of ensuring comparability between treatment and control groups so that differences subsequently observed between the groups can be attributed to the intervention rather than to extraneous factors (Berk et al. 1985; Boruch et al. 1978; Campbell 1969; Campbell and Stanley 1963; Dennis and Boruch 1989; Riecken et al. 1974). Hence, randomized experiments provide the best counterfactual describing what would have happened to the treatment group if it had not been exposed to the treatment (Rubin 1974; Holland 1986). Generating evidence from randomized designs is particularly important in the field of criminal justice, where non-randomized studies appear to have a bias towards results that are favorable to interventions (Weisburd et al. 2001).

For the experiment, the 117 identified routes were randomly assigned to one of three conditions using computer-generated random numbers (see Shadish et al. 2002): 45 of the routes were assigned to receive LPR-enhanced patrol by the vehicle theft unit (the LPR condition), another 45 routes were assigned to the vehicle theft unit for patrol and surveillance without the LPRs (the "manual check," non-LPR condition), and 27 routes were assigned to normal patrol (the control condition).[19] We used a stratified random allocation procedure (Boruch 1997) and randomized hot spot transit routes within statistical "blocks" to allow for the likely possibility of substantial

[16] At the outset of the project, the research team and the auto theft unit agreed that the officers would remain within approximately 500 ft of the main hot routes when working side streets, and these boundaries were drawn into maps that officers used during the operation.

[17] Because some of the routes were in close proximity to one another (e.g., intersecting streets), crimes could occur in the buffer zones of multiple routes. When this occurred, the crimes were counted against each of the routes in question. Across the various categories of calls for service analyzed below, roughly one-quarter to one-third of the calls occurred within the buffer zones of multiple routes. This overlap balanced out across the experimental design and was thus uncorrelated with the treatment and control group designations.

[18] The hot routes and their buffers cover 13 square miles, equating to approximately 10 % of the city's land area.

[19] It is worth noting that all three conditions (LPR, manual license plate checking, and the control group) received standard patrol services, but the control group received no other interventions beyond standard patrol services. The study was designed with three groups because our original objective was to assess the effectiveness of LPR technology independent of its use by a specialized unit. Therefore, we included two types of control groups: one that would be patrolled by a specialized vehicle theft unit doing manual license plate checks and another that would be patrolled by regular patrol officers (who may conduct license plate checks on a discretionary and ad hoc basis).

variation across places (Weisburd and Green 1995).[20] We did this using four stratification variables: length of route, speed limit of route, ease of surveillance for running plate checks (as graded by the auto theft unit), and whether the route was identified based on geographical analysis or designation by MPD personnel. Following the randomization, there were no statistically significant pre-treatment differences among the routes in the three groups based on these variables (all tests had p levels greater than 0.10); the average length varied from 0.57 to 0.62 miles, the average speed limit varied from 36 to 38 miles per hour, the average surveillance rating was equal at 2.8 (as rated by the auto theft unit on a four point scale with 1=very hard, 2= somewhat hard, 3=somewhat easy, and 4=very easy), and the proportion of routes determined by GIS analysis ranged from 0.64 to 0.69.

We then divided the 30-week intervention period into 15 bi-weekly periods. Routes selected for intervention by the vehicle theft unit (both the LPR routes and manual check routes) were randomly assigned to receive treatment during one of these bi-weekly periods. During each bi-weekly period, the unit worked three LPR routes and three manual check routes. As described above, each route was patrolled for approximately an hour from Wednesday through Saturday between 3:00 pm and 1:00 am. The time of day during which the unit patrolled each route was also varied according to a preset schedule so that the unit would not work the same routes at the same time each day.[21] Hence, both the bi-weekly treatment period and time of day patrolled were determined randomly for each route. This design ensured that the places and times worked with LPRs and without LPRs were comparable to one another and to the control condition.

Procedures were established to monitor the integrity of the assignment process and to measure and statistically control for any contamination (especially between contiguous hot routes). The officers maintained logs to document their time at the hot routes, deviations from the study protocol, and the nature and results of any "hits" from the LPR and manual checks. Our team also conducted detailed interviews and ride-alongs with the vehicle theft unit officers and other patrol officers to assess their use or non-use of the LPR equipment and conduct treatment integrity checks (e.g., querying them on their adherence to the study protocols). No problems were revealed through these treatment integrity checks; with the exception of a few emergency cases in which their assistance was required elsewhere, the unit followed the assignments carefully. [22]

[20] This type of randomized block design minimizes the effects of variability on a study by ensuring that like cases will be compared with one another (see Fleis 1986; Lipsey 1990; Weisburd 1993). Pre-stratification ensures that groups start out with some identical characteristics and will ensure that we have adequate numbers of places in each of the cells of the study.

[21] The LPR and manual routes were scheduled in alternating order each day (i.e., the officers would work an LPR route, followed by a manual route, followed by another LPR route, etc.). On some days, the unit could not work all scheduled routes due to special circumstances (such as making an arrest that took the unit out of commission for the rest of the shift). In these instances, the unit resumed patrolling the next day according to the schedule set for that day. These deviations cancelled out over the course of the experiment so that the unit spent equivalent amounts of time working LPR and manual check routes.

[22] The research team developed the study procedures in consultation with the auto theft unit and there were no changes to the operation during the course of the study. The composition of the auto theft unit also remained the same throughout the study with the exception that the unit's supervisory officer changed early in the project.

Outcome measures

We assess the effects of the patrols on crime at the hot spots as approximated by trends in calls for police service for a number of broad and specific offense categories: person crimes (e.g., assault, robbery, and threats), property crimes (e.g., burglaries, thefts, and criminal damage of property), disorderly behavior (e.g., disturbances, prostitution, and intoxicated persons), drug offenses, and auto theft. We used calls for service in order to assess the effects of the patrols on a range of serious and minor offenses, including crimes and disorderly behaviors that are not UCR part I offenses. Although calls for service data are subject to potential problems with both over and underreporting (Klinger and Bridges 1997), they are less affected by police discretion than are police incident or arrest data, and they are generally regarded as the most comprehensive source of data on criminal and disorderly events (Sherman et al. 1989; Warner and Pierce 1993). This makes them especially useful for measuring minor offenses and disorderly behaviors that are less likely to result in official reports or arrests. Accordingly, calls for service are widely used in studies of crime and policing (e.g., Braga and Bond 2008; Braga et al. 1999; Mazerolle-Green et al. 2000; Sherman and Weisburd 1995; Weisburd and Green 1995). Using calls for service to capture person and property offenses also provided higher base rates and greater statistical power relative to using UCR category incidents, which were rare within these small locations and time intervals.[23]

A feature of note is that the Mesa calls for service data include citizen calls as well as calls arising from traffic stops and other officer-initiated activities. Separating calls by source was not possible, as MPD did not differentiate source categories in their calls for service data during most of the study period. As an approximation, however, it was possible to examine the distribution of sources by offense type for a subset of over 2,000 calls that originated from the hot routes during early 2009. This showed that virtually all (88–95 %) of the person, property, and disorder calls stemmed from citizen reports. Similarly, 78 % of the auto theft calls were citizen-initiated, with the majority of the remainder (17 %) stemming from traffic stops. In contrast, the majority of drug calls (62 %) stemmed from traffic stops, while 29 % were citizen-initiated.

Analytical techniques

For the analysis, we created a panel database pooling data from all routes over the 15 bi-weekly intervention periods, the 2 weeks before the experiment, and the 2 weeks after the experiment.[24] The results presented here are based on 102 of the initial 117 routes; the remaining 15 were dropped from the analysis because they are segments of highways and, as such, are not specified as location points in MPD's data system.

[23] During 2007 and 2008, the hot routes experienced an average of about three UCR part I incidents per bi-weekly period (including both violent and property crimes combined). These base rates declined further during the study period, as Mesa experienced a general decline in crime. For the CFS categories we examined, the hot routes had base rates ranging from less than one to about six calls per bi-weekly period (see Table 2).

[24] Data points for the weeks before and after the experiment were included in order to examine pre-post changes and lagged effects for routes that were treated during the first and last periods of the experiment.

This yielded a total of 102 × 17=1,734 data points for analysis. Pooling the data in this fashion increased the statistical power and precision of our analyses and created a data structure making it easier to simultaneously examine initial and residual (i.e., lagged) effects from the interventions.[25] Descriptive statistics for the outcome variables from the pooled database are shown in Table 2 for the program weeks (divided into pre-intervention, intervention, and post-intervention periods for the LPR and manual patrol routes) and the corresponding weeks of the prior year (denoted by the row labeled "1 year Prior to Study"), which we use as baseline measures in the models described below.[26]

We estimated the patrols' impacts using random effects panel models (e.g., see Allison 2005) that control for dependence between observations from the same route and other selected covariates. Based on the distribution of the particular outcome variable, the models employ a Poisson or a negative binomial count distribution (we selected the distribution that provided the best fit to the data as assessed by a likelihood ratio test). In log-linear form, a general representation of the model is given by:

$$\mathrm{Log}\ \lambda_{\mathrm{it}} = \mu_{\mathrm{t}} + \delta \mathrm{LPR}_{\mathrm{it}} + \phi \mathrm{Manual}_{\mathrm{it}} + \beta \mathrm{X}_{\mathrm{it}} + \alpha_{\mathrm{i}}$$

where the outcome variable, Y_{it}, has a Poisson or negative binomial distribution with an expected value of λ_{it} (for additional details regarding these distributions, see, e.g., Allison 2005; Cameron and Triverdi 1986); δLPR_{it} and $\phi Manual_{it}$ represent the respective effects of LPR and non-LPR (manual check) patrols at route (i) and time (t); βX_{it} represents the effects of other covariates (X) measured for route (i) and time (t) (for simplicity, we use this term to include both time-varying and time-invariant covariates); μ_t represents an intercept term that is allowed to vary with time; and α_i represents an unobserved route effect that is distributed as a random variable with a mean of zero and constant variance σ^2 (Allison 2005).[27]

We present models testing for both short-term and long-term effects from the patrol interventions. Conceptually, the models of short-term effects can be represented by the following illustration which, for simplicity, is based on four hypothetical routes and five time periods.

[25] Our desire to boost the statistical power of the analysis by pooling the data stemmed from the fact that the auto theft unit operated at each hot route for only 2 weeks. Disaggregating the data down to these very small locations and short time intervals resulted in relatively "noisy" data, with outcome measures that have very low means and standard deviations that are large relative to their corresponding means (see Table 2). Under such conditions, even relatively large percentage reductions in an outcome measure may produce only small standardized effects (Cohen 1988). Preliminary calculations with some of these measures suggested, for example, that reductions on the order of 20 % might produce only standardized effects in the range of 0.10–0.20 or less for ANOVA analyses. Detecting such effects at standard levels of statistical significance (and with a design having the customarily preferred statistical power level of 80 %) would require sample sizes with hundreds in each group (calculated using GPower software; see Faul et al. 2007).

[26] As shown in Table 2, calls for service generally declined in the LPR and manual check routes during the post-intervention weeks. However, this pattern may be misleading because crime was declining in Mesa throughout the study period. Post-intervention call levels in the intervention routes were generally more comparable to those in the control routes, averaged over all weeks of the study period. In the models below, we examine these patterns more rigorously by controlling for common time trends and other noted factors.

[27] Our use of the random effects approach allows for dependence between observations from the same route and assumes that unmeasured differences between routes are uncorrelated with the treatment effects by virtue of the experimental design. We estimated the models using STATA 10.1 xt commands for cross-sectional time series data.

Table 2 Means and standard deviations of calls for service variables by treatment group and time period

Assigned treatment	Time period	Auto theft	Person crime	Property crime	Disorder	Drugs
Control	1 year Prior to Study (n=391)	0.65 (1.07)	1.68 (1.93)	5.56 (4.55)	5.88 (5.12)	0.76 (1.62)
	All Program Periods (n=391)	0.42 (0.82)	1.21 (1.51)	4.98 (4.00)	4.65 (4.34)	0.47 (0.86)
LPR	1 year Prior to Study (n=680)	0.86 (1.41)	1.90 (2.17)	6.38 (5.25)	6.76 (6.88)	0.91 (1.75)
	Pre-Intervention Periods (n=329)	0.86 (1.44)	1.64 (1.99)	5.61 (4.70)	6.01 (5.96)	0.66 (1.15)
	Intervention Period (n=40)	0.65 (1.08)	1.68 (1.89)	5.65 (4.24)	5.50 (5.60)	0.45 (0.81)
	Post-Intervention Periods (n=311)	0.50 (1.23)	1.21 (1.60)	5.04 (4.73)	4.64 (5.22)	0.39 (0.70)
Manual	1 year Prior to Study (n=663)	0.55 (0.81)	1.78 (1.94)	5.34 (3.77)	6.00 (4.81)	0.61 (0.93)
	Pre-Intervention Periods (n=313)	0.38 (0.74)	1.67 (1.81)	4.77 (3.51)	4.99 (4.14)	0.41 (0.72)
	Intervention Period (n=39)	0.38 (0.59)	1.69 (2.03)	4.77 (3.81)	4.82 (5.04)	0.51 (0.88)
	Post-Intervention Periods (n=311)	0.27 (0.58)	1.27 (1.60)	4.43 (3.19)	4.36 (4.01)	0.45 (0.77)

Hot route 1: O O X O O
Hot route 2: O X O O O
Hot route 3: O O O X O
Hot route 4: O O O O O

In this illustration, hot routes 1, 2, and 3 are treatment routes that received one of the patrol interventions at varying points in time as designated by the Xs (for ease of illustration, we do not distinguish between the LPR and non-LPR patrols). Hot spot 4 serves as a control route. (Note that intervention routes also outnumber control routes by roughly 3 to 1 in the actual data.) The models show whether places and times that received the intervention differed significantly from places and times that did not receive the intervention. These models can also be extended to include temporary residual effects that last for some specified time beyond the intervention weeks in each treated route.

In the models of long-term effects, we tested for residual patrol effects that lasted indefinitely (i.e., through the end of the study period). Hence, a hot route treated at time t was coded as a treatment observation for that time point and all subsequent time points. For these models, the hypothetical hot routes above would be coded in the following manner.

Hot route 1: O O X X X
Hot route 2: O X X X X
Hot route 3: O O O X X
Hot route 4: O O O O O

In addition to the intervention indicators, the models include covariates controlling for time trends, patrol activity in adjacent hot routes, and a number of hot route characteristics, including length, visibility, and prior levels of crime. Our measure of prior crime for each route and bi-weekly period (i.e., our time 1 crime measure) corresponds to that route's level of the same crime type during the same bi-weekly period of the prior year. We used this seasonally lagged measure (referred to below as a seasonal effect) rather than the immediately prior 2 weeks because of the possibility that the latter measure would be contaminated by displacement or diffusion effects stemming from interventions in nearby routes. Though not large, there were some differences across the experimental groups in the calls for service measures during this baseline, seasonal lag period (see the "1 year Prior to Study" rows in Table 2). Specifically, the LPR routes had more calls for auto theft, property crime, and disorder than did the control and manual patrol routes and more drug calls than did the manual patrol routes ($p<.05$ for differences in bi-weekly averages).[28] The seasonal lag variable thus adjusts for these baseline differences. We also included a bi-weekly time trend indicator to avoid confounding the effects of the patrol interventions with those of a general decline in crime that occurred in Mesa during the study period (due to the structure of our panel data, this was particularly necessary for the models of long-term intervention effects).[29]

To further enhance the precision of the treatment comparisons, the models also control for additional covariates including the route length and visibility measurements used in the stratified randomization procedure (Armitage 1996; Gelber and Zelen 1986).[30] The route length variable categorizes each route as short (less than .45 miles), mid-length (.45 to .9 miles), or long (longer than .9 miles), while the visibility covariate reflects the route's visibility for surveillance as rated by the auto theft unit (discussed above). Two additional variables included in the stratification procedure, speed limit of the route and selection method (geographic analysis versus selection by officers), were not included in the models; the former was dropped due to collinearity with other predictors, and the latter proved non-significant in preliminary analyses. Finally, as one measure of possible displacement or diffusion effects from the patrols, the adjacent route variable indicates, for each route and time period, whether an adjacent route was being treated at that time (i.e., receiving LPR or

[28] Hence, the routes randomly assigned to the LPR group were by chance locations with somewhat higher crime levels. As shown in Table 2, the bi-weekly averages of these call categories had the following ranges across groups during the seasonal lag period: 0.55–0.86 for auto theft, 5.34–6.38 for property crime, 5.88–6.76 for disorder, and 0.61–0.91 for drug incidents.

[29] As shown in the illustration of hypothetical hot routes, the intervention observations in the models of long-term effects are disproportionately clustered near the end of the study period. As a result, models of long-term intervention effects that do not include the time trend variable produce numerous coefficients suggesting that both patrol interventions produced long-term crime reductions. Most of these results change when the time trend indicator is added. In contrast, inclusion of the time trend indicator has no effect on the results of models estimating short-term intervention impacts (in these models, the intervention observations are randomly dispersed in time throughout the study period).

[30] Although not strictly necessary, the introduction of these covariates allows us to potentially improve the precision of the treatment comparisons by reducing error variance and correcting for any imbalances in the distribution of these covariates across the treatment and control groups that may have occurred due to chance (Armitage 1996). Adding covariates can also help adjust for the natural variation between cases within the comparison groups (Gelber and Zelen 1986).

manual check patrol by the vehicle theft unit).[31] Although this variable was not factored explicitly into the design, it was expected to balance out across the experimental groups by virtue of randomization (introduction of such covariates into the analysis of experimental data post hoc can introduce bias or otherwise distort results if the covariates are distributed unevenly across groups). Supplemental analyses (not shown) indicated that that inclusion of this variable and the stratification variables did not affect the estimates of the intervention effects.

Results

Elsewhere, we have discussed the impact of the LPR patrols on recoveries of stolen automobiles, apprehension of auto thieves, and the prevention of auto theft (Taylor et al. 2011a, 2012). To summarize, the unit was more likely to detect and recover stolen vehicles and apprehend auto thieves when using the LPR devices, though the numbers of recoveries and arrests were not large. Overall, the unit made 14 arrests during the course of the experiment. Four of these were for auto theft or stolen plates, all of which occurred when the unit was using the LPRs.[32] The other ten arrests, which were due to observation of criminal activity or other types of license plate hits (e.g., warrants), were spread evenly between the LPR and non-LPR patrols.

In addition, we found that the operations had residual effects that deterred auto theft for at least 2 weeks following the patrols. However, these effects were associated only with the non-LPR patrols. Based on discussions with the officers in the unit and observations of their work, we hypothesize that the additional time the officers spent slowly roaming the routes when not using the LPRs deterred vehicle theft by making the officers' presence more noticeable and unpredictable, by increasing their time in parking lots that auto thieves may have been targeting, and by making it more obvious to onlookers that they were checking vehicles. The greater use of fixed surveillance points with the LPR equipment, in contrast, may have been less threatening to vehicle thieves because it was easier to spot and avoid. It is also possible that auto thieves mistook the cameras for speeding cameras and did not recognize them as LPRs.

Our purpose in this analysis is to examine whether the patrol operations had initial and residual deterrence effects on other forms of crime and to assess how long the latter persisted if they occurred. In so doing, we assess whether brief, low-dose patrol crackdowns can reduce crime at hot spots and whether the rotation of such operations across hot spots is an improvement over standard patrolling and triage approaches at these locations. Further, we assess whether the effects of the patrols varied depending on whether they were using the LPRs.

[31] Less than 2 % of the hot route/bi-weekly observations had more than 1 adjacent route receiving treatment simultaneously (nearly all were instances in which two adjacent routes were being treated). Additional analyses differentiating between LPR and manual check patrols in adjacent routes produced similar effects for both types of patrol. In the interest of parsimony, the models presented in the text include only one indicator for the presence of either type of patrol.

[32] The unit recovered 10 stolen vehicles when using the LPR devices and 5 when doing manual license checks (they also detected 8 stolen license plates when using the LPRs).

Models of long-term and short-term deterrence effects

Estimates of the patrols' impacts on the selected crime types are presented in Tables 3 and 4. (All model coefficients in both tables have been transformed to incidence rate ratios.) For the models in Table 3, the intervention indicators (LPR or manual check patrol) were coded as 1 during the assigned 2-week treatment period and all subsequent bi-weekly periods (which, on average, included about 7 bi-weekly periods). Hence, these models test for both initial deterrence effects and "long-term" residual deterrence effects from the patrols.

Focusing first on the patrol effects, most crimes declined 1–9 % following the LPR patrols, albeit non-significantly. The exception is that drug crimes showed a statistically significant drop of 28 % in the LPR routes. The locations that received the manual check patrols had non-significant drops of 3–4 % in person, property, and auto theft crimes and a non-significant increase of 3 % in disorder crimes. However, they also experienced a statistically significant increase of 35 % in drug calls.

The models in Table 3 suggest that the patrols did not have long-lasting effects on most forms of crime. Therefore, we estimated an additional set of models that tested for shorter-term initial and residual deterrent effects using two binary variables: one

Table 3 Long-term effects of LPR and non-LPR patrols on calls for service at hot routes

	Calls-For-Service/911				
	Negative binomial				Poisson
	Auto theft IRR (SE)	Person crimes IRR (SE)	Property crimes IRR (SE)	Disorder crimes IRR (SE)	Drug crimes IRR (SE)
Treatment condition (ref: control)					
LPR patrol	0.95 (.151)	0.91 (.092)	0.99 (.051)	0.95 (.052)	0.72 (.139)*
Manual check patrol	0.97 (.170)	0.97 (.094)	0.96 (.054)	1.03 (.054)	1.35 (.147)*
Seasonal crime effect	1.10 (.024)***	1.05 (.012)***	1.01 (.004)**	1.01 (.003)*	1.08 (.020)***
Bi-weekly period	0.96 (.010)***	0.98 (.006)**	0.99 (.003)	0.98 (.003)***	0.98 (.009)**
Hot route length (ref: Short)					
Hot route length - Mid	0.51 (.223)**	0.61 (.186)**	0.59 (.150)***	0.65 (.186)*	0.58 (.219)*
Hot route length - Long	1.01 (.248)	1.03 (.208)	1.09 (.167)	1.19 (.210)	1.07 (.244)
Good visibility	0.98 (.114)	1.08 (.094)	0.91 (.076)†	1.06 (.093)	1.07 (.108)
Adjacent routes treated at time	0.95 (.108)	1.19 (.057)**	1.04 (.033)	1.07 (.032)*	0.91 (.098)
Intercept	4.44 (.482)**	9.55 (.434)***	30.45 (.317)***	28.61 (.387)***	0.51 (.381)†
Log likelihood	−1,422.8	−2,463.92	−3,878.91	−3,726.04	−1,429.19
Wald X^2	55.69	60.71	32.61	126.9	54.99
Prob > X^2	0.000	0.000	0.000	0.000	0.000

† $p \leq .10$, * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

Table 4 Short-term effects of LPR and non-LPR patrols on calls for service at hot routes

	Calls-For-Service/911				
	Negative binomial				Poisson
	Auto theft IRR (SE)	Person crimes IRR (SE)	Property crimes IRR (SE)	Disorder crimes IRR (SE)	Drug crimes IRR (SE)
Treatment condition (ref: control)					
LPR @ Treatment weeks	1.13 (.232)	1.22 (.138)	1.09 (.078)	1.04 (.079)	0.83 (.242)
LPR @ Post-2 weeks	1.15 (.238)	0.88 (.159)	1.02 (.086)	0.91 (.085)	0.51 (.321)*
Manual @ treatment weeks	1.11 (.277)	1.17 (.140)	1.09 (.078)	1.01 (.085)	1.16 (.232)
Manual @ post-2 weeks	0.25 (.582)*	0.54 (.199)**	0.91 (.090)	0.99 (.086)	1.12 (.238)
Seasonal crime effect	1.10 (.024)***	1.06 (.012)***	1.01 (.004)**	1.01 (.003)*	1.08 (.020)***
Bi-weekly period	0.96 (.008)***	0.98 (.005)***	0.99 (.002)	0.98 (.003)***	0.97 (.007)***
Hot route length (ref: Short)					
Hot route length - Mid	0.52 (.222)**	0.61 (.184)**	0.60 (.150)***	0.65 (.185)*	0.57 (.214)**
Hot route length - Long	1.01 (.247)	1.04 (.206)	1.10 (.167)	1.19 (.209)	1.03 (.238)
Good visibility	0.98 (.115)	1.08 (.093)	0.91 (.076)	1.07 (.093)	1.07 (.106)
Adjacent routes treated at time	0.97 (.107)	1.20 (.057)***	1.04 (.033)	1.07 (.032)*	0.90 (.098)
Intercept	4.45 (.485)**	9.38 (.437)***	30.83 (.318)***	28.36 (.386)***	0.51 (.373)†
Log likelihood	−1,417.97	−2,456.12	−3,877.56	−3,726.17	−1,433.65
Wald X^2	61.11	73.08	35.38	126.6	49.95
Prob > X^2	0.000	0.000	0.000	0.000	0.000

† $p \leq .10$, * $p \leq .05$, ** $p \leq .01$, *** $p \leq .001$

for the treatment condition (LPR or manual check patrol) during the assigned two-week intervention period and another for the treatment condition in the 2 weeks after the intervention (Post-2 weeks). As shown in Table 4, calls for service did not change significantly in the hot routes during the intervention weeks (most categories showed non-significant increases during the patrol weeks, though drug calls showed a non-significant decrease of 22 % in the LPR routes). However, some crime types declined significantly during the 2 weeks immediately following patrols by the auto theft unit, suggesting residual deterrence effects. Specifically, drug calls declined 49 % in the LPR routes, auto theft calls declined 75 % in the manual check routes (this replicates our earlier finding noted above), and person crimes declined 46 % in the manual check routes. Subsequent testing revealed that the reductions in auto theft and person crimes in the manual check locations did not last beyond the first 2 post-intervention weeks. In contrast, the decline in drug crimes in the LPR locations lasted through the end of the study period (see Table 3), though the overall pattern of results indicate that this effect weakened over time.

As noted, most drug calls stemmed from traffic stops rather than citizen calls, and it was not possible to examine effects on calls by source. Nevertheless, the patterns in Tables 3 and 4 seem most likely to reflect changes in drug activity rather than changes in officer enforcement activity. The LPR and manual check routes were small, randomly selected road segments that were interspersed throughout major portions of the city. It seems unlikely that officers began doing more traffic stops and drug enforcement on the specific road segments that were randomly assigned to manual license checks while simultaneously doing less on those that were randomly assigned to LPR—and that these changes coincided with the timing of the LPR and manual check interventions (neither the route designations nor the timing of the interventions were generally known outside the auto theft unit). A more plausible interpretation, in our view, is that officers doing traffic stops in the LPR locations were less likely to find motorists with drugs after the LPR intervention because people were less likely to be buying and selling drugs in those locations. The reverse occurred in the manual check routes. This interpretation is also supported by the distribution of source types for the subset of calls for which this information was available. Across all three treatment conditions (LPR, manual check, and control), 60–64 % of the drug calls were linked to traffic stops and 27–30 % were linked to citizen calls; hence, the sources of drug calls did not vary appreciably across groups.

Turning to the other results, there were significant seasonal effects for all crime types. Further, with the exception of property crimes, there was a significant downward trend in all crime types during the study period, as indicated by the bi-weekly time trend indicator. Crime was also significantly lower in mid-length routes as compared to shorter routes. A route's visibility rating was generally unrelated to its crime levels, though there was some indication that property crime calls were lower along routes with greater visibility (see the marginally significant coefficient for route visibility in Table 3).

Tests for crime displacement or a diffusion of crime control benefits

In addition to assessing how the patrols affected crime in the hot routes, we also made some exploratory assessments of whether the patrols displaced crime to nearby areas or produced a diffusion of crime control benefits to those areas. As one indicator of crime displacement or crime control diffusion effects, the adjacent route variable in the preceding models shows whether a given route was affected by LPR or manual check patrols in an adjacent route. Results in Tables 3 and 4 show that routes adjacent to treated hot routes experienced temporary but significant increases in personal crimes (19–20 %) and disorder crimes (7 %) when the auto theft unit was working the nearby treatment route. Note that the adjacent route treatment indicator corresponds only to the 2-week period during which an adjacent route(s) was treated; supplemental testing indicated that this effect did not persist beyond that time.

While these results suggest that the patrols may have temporarily displaced some forms of crime, they capture only displacement to nearby hot routes and not to other nearby streets. Although the former locations would seem more likely to have drawn displacement (because they are more likely to have features and opportunity structures similar to those of treated hot routes), we conducted additional tests for displacement or crime control diffusion effects anywhere beyond the 500-ft buffer of a treated hot route but within 2,500 ft of that route. Because many of the hot routes were in close proximity, interpretation of these analyses is complicated by a high degree of overlap in the displacement/diffusion areas surrounding different routes. Due to this limitation of the study design, the results should be viewed cautiously. Nonetheless, the random assignment of the interventions across routes and time periods should provide us with some ability to ascertain whether increases or decreases in crime in these areas was associated with interventions in nearby routes.

For these analyses, we estimated models of long and short-term effects comparable to those presented in the main analyses. Hence, we compared changes in areas surrounding LPR and non-LPR patrol routes to those in areas surrounding control routes. Models of long-term effects (not shown) revealed no indications of long-term displacement or crime reduction benefits in the areas surrounding either group of treatment routes. However, models of short-term effects in the displacement/diffusion areas, displayed in Table 5, provide some confirmation of the temporary displacement effects implied by the adjacent route indicator in the main hot route models. Specifically, disorder calls went up by almost 10 % in areas surrounding LPR routes during

Table 5 Short-term effects of LPR and non-LPR patrols on calls for service in areas surrounding hot routes

	Calls for Service/911				
	Auto IRR (SE)	Person IRR (SE)	Property IRR (SE)	Disorder IRR (SE)	Drug IRR (SE)
Treatment condition:					
LPR @ Treatment	.93 (.124)	1.05 (.062)	.99 (.047)	1.10 (.039)*	.89 (.131)
LPR @ Post-2 weeks	.99 (.122)	1.09 (.062)	.95 (.048)	.96 (.041)	.90 (.130)
Manual @ Treatment	1.14 (.117)	1.11 (.064)†	1.04 (.047)	1.03 (.041)	1.11 (.125)
Manual @ post-2 weeks	1.01 (.123)	1.03 (.067)	.97 (.049)	1.00 (.042)	.84 (.145)
Seasonal crime	1.02 (.007)*	1.002 (.003)	1.004 (.001)**	1.005 (.001)**	1.03 (.008)**
Bi-Weekly	.98 (.004)**	.98 (.002)**	1.002 (.001)	.98 (.001)**	1.01 (.004)*
Hot route length					
Hot route length - Mid	.62 (.157)**	.63 (.151)**	.51 (.122)**	1.09 (.143)	.61 (.172)**
Hot route length - Long	.75 (.175)	.84 (.172)	.64 (.133)**	1.09 (.161)	.81 (.196)
Good visibility	1.11 (.076)	1.02 (.076)	.93 (.055)	1.06 (.071)	1.07 (.082)
Adjacent routes treated at time	1.01 (.048)	1.08 (.026)**	1.05 (.019)**	1.06 (.016)**	.94 (.052)
Intercept	2.03 (.301)	3.4 (.299)	3.57 (.217)	3.52 (.261)	1.71 (.322)
Log likelihood	−3,223.42	−4,511.02	−5,926.62	−5,717.45	−3,052.66
Wald X^2	63.36	84.28	62.35	343.27	33.55
Prob > X^2	0	0	0	0	0.0002

All models are negative binomial regression models

† $p \leq .10$, * $p \leq .05$, ** $p \leq .01$

the two-week intervention period relative to patterns in areas surrounding control routes. There were weaker indications ($p=.09$) that calls for person offenses may have temporarily increased by 11 % in areas surrounding manual patrol routes. Neither effect lasted beyond the 2-week intervention period. Further indications of displacement are shown by the adjacent route variable in the person, property, and disorder models, which suggests that these types of calls also increased on the order of 5–7 % in areas surrounding a given hot route (i) when the experimental patrols were implemented in a route adjacent to route (i).

Discussion and conclusions

In sum, the Mesa LPR experiment produced a complex pattern of results. For 15 successive 2-week periods, a specialized squad of officers conducted short daily operations at randomly selected hot spot road segments at varying times of day. Based on random assignment, they patrolled and conducted surveillance at some of these road segments using LPRs, while conducting extensive manual checks of license plates on other assigned roads (at the latter places and times, their LPR cameras were still mounted on their cars but not activated). The locations that received the LPR patrols experienced reductions in calls for service for drug crimes that lasted for at least several weeks beyond the intervention. The roads that received the manual check patrols experienced short-term reductions in calls regarding person offenses and auto theft that lasted for approximately 2 weeks beyond the intervention.

However, there were also indications of crime displacement. The decrease in drug calls in the LPR locations was offset by a corresponding increase in these calls that emerged over time at the manual check locations. This suggests that, after an initial disruption, drug activity at the LPR routes gradually migrated to other similar locations. There were also indications that the patrols caused short-term increases in some types of calls, particularly those for person and disorder offenses, in areas nearby the intervention routes.

Generally, studies of geographically-focused police interventions have been more likely to find a diffusion of crime control benefits to surrounding areas than to find crime displacement to those areas (Bowers et al. 2011; Braga et al. 2012). As others have shown, crime hot spots have social and opportunity features that are conducive to criminal offending (e.g., Braga et al. 1999; Sherman et al. 1989; Weisburd et al. 2012; see also note 5). Geographical displacement of crime from these locations may thus be prevented or lessened when nearby areas do not have such features and when finding other similar locations increases difficulties, costs, and risks to offenders (e.g., see Weisburd et al. 2006).

The displacement patterns found in this study, however, may be explained by a number of factors. Some have argued that drug activity is more susceptible to displacement than many other forms of offending (Eck 1993), and studies of enforcement-centered drug market crackdowns have often shown indications of displacement (Mazerolle et al. 2007). Further, the hot routes that appeared to receive drug activity displacement in this study were likely to have social and opportunity structures similar to those where drug activity declined (by virtue of the randomized study design).

The short-term displacement of other offenses into areas very near the intervention routes also has some precedent. In their systematic review of geographically-focused police interventions, Bowers et al. (2011) found that interventions consisting primarily of increases in police manpower (i.e., patrol interventions and crackdowns) have mixed effects on nearby areas, sometimes suggesting displacement and other times a diffusion of crime control benefits (on average, these effects are statistically insignificant). In this study, the areas adjacent to the treated hot routes may have had features that facilitated displacement of certain offenses. This is an issue that may warrant more explicit attention in future hot spots studies (e.g., see Weisburd et al. 2006).

However, the short-term patterns of crime reduction and displacement found in this study are particularly complex to interpret. There was no displacement associated with the temporary reduction in auto theft calls at the manual patrol locations, but it seems that the temporary decline in personal crimes at these routes was at least partially offset by temporary increases in these calls in nearby areas.[33] Yet the timing of these changes was unusual in that the apparent displacement of personal crimes occurred during the intervention weeks, while the reduction in personal offenses at the intervention routes was not evident until the weeks immediately following the intervention. There were also small temporary increases in disorder calls at locations near the treated hot routes (particularly those near the LPR routes) even though there was no decline in disorder calls at either group of treated routes.

One possible interpretation is that these particular displacement patterns signify reporting effects rather than true crime displacement. In other words, if people in the adjacent areas noticed the additional police presence in the nearby routes (or saw the police passing through more often en route to and from these locations), perhaps this affected their propensity to call police about certain matters during the intervention weeks. If true, it is also possible that similar reporting effects masked impacts of the patrols in the treatment routes during the intervention weeks, which would help to explain the discrepancies between results in the intervention and adjacent locations.

Yet another possibility is that these patterns reflect immediate, short-term changes in behavior at hot spots that result from brief police interventions. Prior studies of hot spots policing have generally examined interventions that lasted from several weeks to multiple years (Braga et al. 2012). Studies have only rarely assessed behavioral changes at hot spots within very short time intervals like those examined here (for an exception, see Sherman and Rogan (1995) who found that 1-day raids on crack houses reduced crime on those respective street blocks but for only about 12 days). Hence, another speculative interpretation is that the patrols diverted some troublesome people away from their usual hangouts to nearby locations where they caused more problems, whether real or perceived (with the latter possibly causing reporting effects). Short-term displacement of person offenses to nearby familiar locations may be more likely, especially in response to brief police interventions, given that violent offenders tend to commit crimes closer to home (e.g., see reviews in Eck and Weisburd 1995; Gabor and Gottheil 1984; also see Taylor et al. 2011b). Following

[33] The magnitudes of the estimated displacement effects were smaller than those of the estimated reductions in the hot routes, suggesting that the former did not completely offset the latter. However, given the imprecision of the displacement analyses, we have not tried to formally quantify these effects relative to one another.

the patrols, these actors may have returned to their usual locations but made greater efforts to stay out of trouble (at least for a time), or they may have temporarily avoided both the target and adjacent locations. This possibility could also potentially explain the post-intervention reduction in personal crimes in the manual check routes. Longer and/or repeated interventions are likely needed to have more lasting effects on the movements and behavior of people in hot spots and their surrounding areas.

Implications for police use of license plate readers

Despite these complexities, the findings would seem to have a number of tentative implications for police practice. To begin with, the findings suggest that the use of LPRs can reduce certain types of offenses at hot spots. Most notably, the use of LPR patrol was associated with reductions in drug crimes along hot spot road segments. Given the modest number of arrests made by the auto theft unit—and particularly the small effect that LPR use had on the unit's arrest activity—it would seem that the decline in drug calls at the LPR locations was due to the deterrent value of displaying the cameras rather than the identification and incapacitation of offenders. This implies that the visibility of the cameras may have also contributed to the brief reductions in personal crimes and auto theft that occurred in the manual check routes (where the auto theft unit patrolled with their LPRs visible but not activated). A caveat is that the study design does not enable us to differentiate the effects of the LPRs' visibility from those of the enhanced patrol presence. In other words, we cannot say conclusively that the LPRs caused deterrent effects beyond those of the additional officer presence and activities. However, it seems likely that the visibility of the cameras was an important factor in both groups of hot routes since three of the four police cars used by the auto theft unit were unmarked.

These results stand in contrast to those of the Virginia LPR study which found no changes in crime associated with LPR patrols (Lum et al. 2010; 2011a). Although the Virginia patrols were conducted for a longer period, the Mesa treatment was perhaps more intensive in that it was conducted by a special unit of four officers, rather than one, operating jointly in the locations on several consecutive days spread over 2 weeks. Our study also tested for effects over much smaller units of time than those used in the Virginia study and may have thus captured impacts of brief duration that were not apparent in that study. Of course, various contextual differences between the study locations could also account for the varying results. Clearly, further testing of LPR use in a wider variety of jurisdictions and with additional variation in tactics is warranted.

The varying results for the LPR and manual check locations also provide some insights into how different uses of LPR might affect different types of offenses. When the officers had the LPRs activated, they spent more time doing fixed surveillance in prominent locations along the hot routes. This may have driven away prospective drug buyers who were traveling to these locations for drug purchases. Despite the brief duration of the LPR operations, this effect appears to have been strong enough to reduce drug activity on these road segments for substantial lengths of time after the patrols ended.

In contrast, the vehicle theft unit spent more time roaming the streets, commercial parking lots, and apartment complexes of the hot routes—often at slow speeds and with frequent pauses—when they were doing manual license checks. This type of police presence, which was perhaps less visible along the major roadways but more obtrusive and threatening to potential offenders in specific high-activity locations along the routes, did not reduce drug offenses but does appear to have temporarily suppressed violent offenses and auto theft. As noted, the manual check patrols may have reduced auto theft in particular because officers were spending more time in parking lots that were targets for auto thieves and because it may have been more obvious to onlookers that the officers were searching for stolen vehicles.

These findings also highlight some of the ways in which the effects of police technology on crime are mediated by the complex interplay of technology and police behavior. In this case, the use of LPR devices led officers to adopt tactics that increased their effectiveness in reducing drug activity but reduced their effectiveness in reducing auto theft and personal offenses. If police managers are cognizant of these issues, they can perhaps guide LPR officers and teams on multiple ways of using LPRs that might create a wider range of effects. [34]

In short, these findings imply that one effective way for police to use small numbers of LPRs is to move them around hot spot locations for short-term operations. This may be especially useful at hot spots of drug crime, where use of LPRs may produce substantial residual deterrence. Although our analysis suggests that this could result in displacement of drug activity to other similar locations, continual and repeated rotation of LPRs across more of these sites (especially those exhibiting increases) might, in principle, produce further disruptions in these patterns and make displacement increasingly difficult for drug market participants. LPRs may also have utility in preventing auto theft, particularly if officers use them in a slow-moving fashion that conveys their focus on checking license plates. Impacts on auto theft were brief in this study, but perhaps longer and/or repeated applications of LPR patrol at hot spots will help police bring about longer-lasting reductions in this crime.[35] Such operations may also reduce violent offenses in targeted locations, though our results suggest that police will need to be sensitive to the possibility of displacement or reporting effects associated with these offenses, and perhaps others, in nearby places.

Study limitations

Our study had a number of limitations, however, that should qualify our conclusions and generalizations. For one, the productivity of the LPR unit (i.e., the rate at which

[34] More generally, there has been relatively little research on the impacts of technology in policing (beyond technical, efficiency, or process evaluations), and that which does exist suggests that technology does not necessarily bring about desired crime reduction benefits (Koper et al. 2009; Lum 2010; Manning 1992). Accordingly, there is a need to better understand both how technology affects various organizational and behavioral aspects of policing and how, in turn, these and other factors shape the uses and effectiveness of policing technology (e.g., see Chan 2001, 2003; Koper and Lum 2010).

[35] This conclusion may also be contingent on the types of locations where police focus their efforts. In another phase of this study, Mesa officers conducted similar LPR patrols over larger areas, averaging about one square mile in size. Neither LPR nor manual check patrols produced measurable declines in auto theft in these areas (impacts on other crimes were not examined) (Taylor et al. 2011a). Consistent with research on hot spots policing more generally, this supports the merits of focusing LPRs on well defined micro-places (in this case, high-risk road segments).

the officers detected stolen automobiles and other vehicles of interest) was constrained by both the experimental design and the limitations to the data fed into the LPR devices. The former required the officers to work assigned locations at assigned times in order to assure comparability across the study groups. In practice, officers using LPRs will likely be more productive, and potentially more effective, if they target their operations based on current crime analysis and traffic patterns (Taylor et al. 2011a, 2012).[36] The ability of police to reduce crime with LPRs may also be considerably greater if they can employ larger numbers of the devices and integrate them with a wider range of data sources (e.g., data on warrants, parking violators, probationers, parolees, investigations, and other persons and vehicles of interest). Indeed, the findings presented here may have limited applicability to places that have extensive networks of LPR devices (like the United Kingdom and New York City), particularly with respect to the value of LPRs for incapacitation (though it is possible that the findings regarding the varying effects of different uses of LPR can be more widely generalized).[37]

Another limitation to the study was that the experimental dosage was limited to a one-time treatment of 8 h (and a total of 32 officer-hours) at each route spread over 2 weeks. Further, we tested only one type of LPR deployment (use by a specialized unit focusing largely on auto theft). Police can use LPRs in a variety of ways, which necessitates further study and experimentation comparing different dosages and modes of LPR use (e.g., fixed, unmanned surveillance versus mobile surveillance by officers, and use by regular patrol versus use by special units). Uses of LPRs for investigative purposes, such as checking vehicles in the vicinity of crime scenes, also require further study. As the evidence base for this technology grows, police and analysts will also have a stronger basis for assessing its cost effectiveness.

Finally, we note that there are potentially significant ethical and privacy concerns surrounding LPR use that were not addressed in this study. In their study of LPR use in Virginia, Lum et al. (2010) surveyed community residents about LPR use and found that, while there was strong support for LPR use in general, this varied depending on the types of LPR applications under consideration (e.g., using the devices to detect stolen automobiles received much more community support than using them to detect parking violators). These attitudes may also differ across communities. At the time of this experiment, there was relatively little publicity about the LPR program in Mesa. However, there was considerable controversy and

[36] After the study, the auto theft unit conducted a number of multi-week "freestyle" operations in which their activities were guided by recent auto theft and traffic patterns. During these periods, they recovered stolen automobiles at a rate four to five times as high as during the experiment. This does not invalidate our findings, given that the LPR, non-LPR, and control routes and times were randomly selected for the experiment and that the unit faced the same constraints when working LPR and non-LPR locations (hence, our study provides valid estimates of the effects of the patrols). However, this does provide some evidence that police can potentially recover more vehicles and apprehend more persons of interest under normal operating conditions.

[37] In the United Kingdom, for example, police have a nationally integrated system for LPR data analysis and storage that receives up to 14 million reads per day from over 10,000 LPR devices (both fixed and mobile) throughout England and Wales and that matches them to a wide variety of data sources (Kable 2010; PA Consulting Group 2006). New York City, to provide another illustration, had 238 fixed and mobile LPRs linked to data on stolen automobiles, wanted persons, and unregistered vehicles as of April 2011 (Baker 2011).

public hostility in Arizona more generally regarding the use of speed cameras on major thoroughfares. Related issues of concern to the public and law enforcement executives are how and for how long LPR data should be maintained, as well as how and by whom they should be utilized (Lum et al. 2010; Police Executive Research Forum 2012).

Implications for crackdown theory and hot spots policing

This study also has more general implications for crackdown theory and hot spots policing. The Mesa operation provides one illustration of a short-term crackdown rotation strategy like that proposed by Sherman (1990), and it provides baseline data for comparative assessments of other similar efforts. In this case, the crackdown was brief and of fairly low intensity—a four-officer unit provided enhanced police presence and surveillance at hot spots for 1 h a day for 8 days spread over 2 weeks. These operations produced residual deterrence at the hot spots that lasted from a few to several weeks, which suggests that they were an improvement over standard patrolling and triage approaches at these locations. It appears to have taken several days of repeated treatment to change perceptions of risk and reduce offending at the locations, as evidenced by the fact that the reductions in crime were observed during the post-intervention weeks but not during the 2-week intervention period. Hence, 2 weeks may serve as a lower bound of recommended dosage for operations of this type.

These insights may be useful in refining the use of directed patrol crackdowns at crime hot spots. While a number of studies have shown that intensive directed patrol and fixed police presence reduce crime at hot spots (e.g., DiTella and Schargrodsky 2004; Lawton et al. 2005; Taylor et al. 2011b; Sherman and Weisburd 1995), such operations can be difficult and prohibitively expensive to maintain at large numbers of hot spots over long periods. Thus, questions still remain about optimal dosages and patrol strategies for hot spots policing (e.g., see discussions in Braga 2007; Taylor et al. 2011b). One common strategy used by police to optimize resource allocation across hot spots is the formation of special mobile units (often called saturation or suppression units) that are deployed to hot spots for temporary assignments as needed (Police Executive Research Forum 2008). This study provides some indirect support for the efficacy of this approach and also suggests, in concurrence with Sherman's (1990) recommendation, that police might use such units in a more proactive and preventive way by rotating them across hot spots either randomly or based on historical patterns and future forecasts. More specifically, the findings of this experiment imply that one potentially effective way to use such a unit is to deploy its members to designated hot spots for short daily patrol operations for at least a 2-week period. Assigning LPRs to these units, where available, might further enhance their effectiveness.

However, there are at least two important qualifications to these conclusions. One is that the results of such operations could vary from those observed here depending on a number of factors including dosage levels (e.g., officers deployed and hours applied per day), whether the police units are marked and/or equipped with LPRs, types of activities conducted by the officers, how often the operations are repeated at a location, and other important contextual variables. The second consideration is that police will need to be cognizant of possible displacement (or reporting effects) stemming from such operations and develop strategies to preempt or address these patterns as needed. Further study may help to clarify these issues and produce further improvements in the calibration of directed patrol operations.

This study also yields additional insights into how specific patrol activities can affect crime at hot spots. Slow roaming, extensive checking of license plates (whether done manually or with an LPR), and prominent overt surveillance could perhaps be incorporated into everyday routine patrol activity at hot spots—and tailored to the particular problems of those locations—in order to maximize the effectiveness of standard patrol. Further tests of such tactics with and without LPRs would also provide further insight into the added deterrent value of LPRs.

In conclusion, police and researchers should build on these results in a number of ways that help to optimize both technology utilization and geographically-focused strategies in policing.

Acknowledgments This project was supported by grant 2007-IJ-CX-0023 awarded by the National Institute of Justice (Office of Justice Programs, U.S. Department of Justice). The authors thank the Mesa, AZ Police Department (MPD) for its strong commitment to the project. We especially thank the auto theft unit officers, (Officers James Baxter, Joel Calkins, Stan Wilbur, and Brandon Hathcock), supervisory officer Cory Cover, Deputy Chief John Meza, and other MPD commanders. Also, the authors are very appreciative of Dr. Yongmei Lu for her work conducting geographic analyses. Finally, the authors thank David Weisburd and other anonymous peer reviewers for their helpful comments on an earlier version of this paper. The views expressed here are those of the authors and should not be attributed to the U.S. Department of Justice, the Mesa, AZ Police Department, the authors' respective institutions, or any of the aforementioned individuals.

References

Allison, P. D. (2005). *Fixed effects regression models for longitudinal data using SAS*. Cary: SAS Institute.

Armitage, P. (1996). The design and analysis for clinical trials. In S. Ghosh & C. R. Rao (Eds.), *Design and analysis of experiments. Handbook of statistics, Vol. 13* (pp. 1–25). Elsevier: Amsterdam.

Baker, A. (2011). Camera scans of car plates are reshaping police inquiries. *The New York Times*, April 11. Downloaded August 30, 2012 from: http://www.nytimes.com/2011/04/12/nyregion/12plates.html?pagewanted=all&_r=0.

Barclay, P., Buckley, J., Brantingham, P. L., Brantingham, P. J., & Whinn-Yates, T. (1995). Preventing auto theft in suburban Vancouver commuter lots: Effects of a bike patrol. In R. Clarke (Ed.), *Crime prevention studies* (Vol. 6, pp. 133–162). Monsey: Criminal Justice Press.

Berk, R. A., Boruch, R. F., Chambers, D. L., Rossi, P. H., & Witte, A. D. (1985). Social policy experimentation: a position paper. *Evaluation Review, 9*(4), 387–429.

Boruch, R. F. (1997). *Randomized experiments for planning and evaluation: A practical guide*. Thousand Oaks: Sage.

Boruch, R. F., McSweeny, A. J., & Soderstrom, E. J. (1978). Randomized field experiments for program planning, development, and evaluation. *Evaluation Quarterly, 2*(4), 655–695.

Bowers, K., Johnson, S., Guerette, R. T., Summers, L., & Poynton, S. (2011). Spatial displacement and diffusion of benefits among geographically-focused policing initiatives. *Campbell Systematic Reviews, 3*. doi:10.4073/csr.2011.3.

Braga, A. A. (2007). The effects of hot spots policing on crime. *Campbell Systematic Reviews, 2007*, 1. doi:10.4073/csr.2007.1.

Braga, A. A., & Bond, B. J. (2008). Policing crime and disorder hot spots: a randomized controlled trial. *Criminology, 46*(3), 577–607.

Braga, A. A., Weisburd, D., Waring, E., Green Mazerolle, L., Spelman, W., & Gajewski, F. (1999). Problem-oriented policing in violent crime places: a randomized controlled experiment. *Criminology, 37*(3), 541–580.

Braga, A. A., Papachristos, A. V., & Hureau, D. M. (2010). The concentration and stability of gun violence at micro places in Boston, 1980–2008. *Journal of Quantitative Criminology, 26*(1), 33–53.
Braga, A., Papachristos, A., & Hureau, D. (2012). Hot spots policing effects on crime. *Campbell Systematic Reviews, 8*. doi:10.4073/csr.2012.8.
Camerer, C. F., & Weber, M. (1992). Recent developments in modeling preferences: uncertainty and ambiguity. *Journal of Risk and Uncertainty, 5*(4), 325–370.
Cameron, A. C., & Triverdi, P. K. (1986). Econometric models based on count data: comparisons and applications of some estimators and tests. *Journal of Applied Econometrics, 1*(1), 29–53.
Campbell, D. T. (1969). Reforms as experiments. *American Psychologist, 24*(4), 409–429.
Campbell, D. T., & Stanley, J. S. (1963). *Experimental and quasi-experimental designs for research*. Boston: Houghton Mifflin.
Chan, J. (2001). The technological game: how information technology is transforming police practice. *Criminal Justice, 1*(2), 139–159.
Chan, J. (2003). Police and new technologies. In T. Newburn (Ed.), *Handbook of policing* (pp. 655–679). Portland: Willan.
Cohen, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). Hillsdale: Lawrence Erlbaum.
Cohen, L. E., & Felson, M. (1979). Social change and crime-rate trends: a routine activity approach. *American Sociological Review, 44*(4), 588–608.
Cohen, I. M., Plecas, D., & McCormack, A. V. (2007). *A report on the utility of the automated license plate recognition system in British Columbia*. Abbotsford, British Columbia: School of Criminology and Criminal Justice, University College of the Fraser Valley.
Dennis, M. L., & Boruch, R. F. (1989). Randomized experiments for planning and testing projects in developing countries: threshold conditions. *Evaluation Review, 13*(3), 292–309.
DiTella, R., & Schargrodsky, E. (2004). Do police reduce crime? Estimates using the allocation of police forces after a terrorist attack. *American Economic Review, 94*(1), 115–133.
Eck, J. E. (1993). The threat of crime displacement. *Criminal Justice Abstracts, 25*, 527–546.
Eck, J., & Weisburd, D. (1995). Crime places in crime theory. In J. Eck & D. Weisburd (Eds.), *Crime and place. Crime prevention studies, Vol. 4* (pp. 1–33). Monsey: Criminal Justice Press.
Faul, F., Erdfelder, E., Lang, A. G., & Buchner, A. (2007). G*Power 3: a flexible statistical power analysis program for the social, behavioral, and biomedical sciences. *Behavior Research Methods, 39*(2), 175–191.
Fleis, J. L. (1986). *The design and analysis of clinical experiments*. New York: Wiley.
Fleming, Z., Brantingham, P. L., & Brantingham, P. J. (1994). Exploring vehicle theft in British Columbia. In R. V. Clarke (Ed.), *Crime prevention studies* (Vol. 6, pp. 47–90). Monsey: Criminal Justice Press.
Gabor, T., & Gottheil, E. (1984). Offender characteristics and spatial mobility: an empirical study and some policy implications. *Canadian Journal of Criminology, 26*(3), 267–281.
Gelber, R. D., & Zelen, M. (1986). Planning and reporting of clinical trials. In P. Calabresi, P. S. Schein, & S. A. Rosenberg (Eds.), *Medical oncology* (pp. 406–425). New York: Macmillian.
Gordon, K. (2006). Automatic license plate recognition. *Law & Order, 545*, 10–13.
Henry, L., & Bryan, B. (2000). *Visualising the spatio-temporal patterns of motor vehicle theft in Adelaide, South Australia*. Adelaide: National Key Centre for Social Applications of GIS.
Holland, P. W. (1986). Statistics and causal inference. *Journal of the American Statistical Association, 81* (396), 945–970.
Kable. (2010, February 3). Police have more than 10,000 ANPR cameras: ACPO data centre processing up to 14 million images per day. *The Register*. Retrieved August 30, 2012 from http://www.theregister.]co.uk/2010/02/03/police_anpr/.
Katz, C., Webb, V. J., & Schaefer, D. (2001). An assessment of the impact of quality-of-life policing on crime and disorder. *Justice Quarterly, 18*(4), 825–876.
Kennedy, L. W., Poulsen, E. & Hodgson, J. (n.d.). *Problem solving using crime mapping: concentration and context*. Newark, NJ: Rutgers University.
King, J., Mulligan, D. K., & Raphael, S. (2008). *CITRIS report: The San Francisco community safety camera program.* Berkeley, CA: Center for Information Technology Research in the Interest of Society, University of California, Berkeley.
Klinger, D. A., & Bridges, G. S. (1997). Measurement error in calls-for-service as an indicator of crime. *Criminology, 35*(4), 705–726.
Koper, C. S. & Lum, C. (2010). *Realizing the potential of technology for policing: A multi-site study of the social, organizational, and behavioral aspects of implementing policing technologies*. Proposal funded by the National Institute of Justice, U.S. Department of Justice. Washington, DC: Police Executive

Research Forum and Center for Evidence-Based Crime Policy, George Mason University (Fairfax, VA).
Koper, C. S., Taylor, B. G., & Kubu, B. (2009). Technology and law enforcement: Future technologies to address the operational needs of law enforcement. Washington, DC: Police Executive Research Forum and the Lockheed Martin Corporation.
LaVigne, N. G., & Lowry, S. S. (2011). *Evaluation of camera use to prevent crime in commuter parking facilities: A randomized controlled trial*. Washington, DC: The Urban Institute.
LaVigne, N. G., Lowry, S. S., Markman, J. A., & Dwyer, A. M. (2011). *Evaluating the use of public surveillance cameras for crime control and prevention.* Washington, DC: The Urban Institute and the Office of Community Oriented Policing Services, U.S. Department of Justice.
Lawton, B. A., Taylor, R. B., & Luongo, A. J. (2005). Police officers on drug corners in Philadelphia, drug crime, and violent crime: Intended, diffusion, and displacement impacts. *Justice Quarterly, 22*(4), 427–451.
Lipsey, M. W. (1990). *Design sensitivity: Statistical power for experimental research*. Newbury Park: Sage.
Lu, Y. (2003). Getting away with the stolen vehicle: an investigation of journey-after-crime. *The Professional Geographer, 55*(4), 422–433.
Lum, C. (2010). Technology and the mythology of progress in American law enforcement. *Science Progress* (Center for American Progress), February 11. http://www.scienceprogress.org/2010/02/police-technology.
Lum, C. Merola, L., Willis, J., & Cave, B. (2010). *License plate recognition technologies for law enforcement: An outcome and legitimacy evaluation*. Final report to SPAWAR and the National Institute of Justice. Fairfax, VA: Center for Evidence-Based Crime Policy, George Mason University. http://gemini.gmu.edu/cebcp/LPR_FINAL.pdf.
Lum, C., Hibdon, J., Cave, B., Koper, C. S., & Merola, L. (2011). License plate reader (LPR) police patrols in crime hot spots: an experimental evaluation in two adjacent jurisdictions. *Journal of Experimental Criminology, 7*(4), 321–345.
Lum, C., Koper, C. S., & Telep, C. W. (2011). The evidence-based policing matrix. *Journal of Experimental Criminology, 7*(1), 3–26.
Manning, P. K. (1992). Information technologies and the police. In M. Tonry & N. Morris (Eds.), *Modern policing. Crime and justice: A review of research, Vol. 15* (pp. 349–398). Chicago: University of Chicago Press.
Maryland State Highway Authority. (2005). *Evaluation of the License Plate Recognition System*. Available from the American Association of State Highway and Transportation Officials at http://ssom.transportation.org/Documents/LPR_report_part3.pdf.
Mastrofski, S. D., Weisburd, D., & Braga, A. A. (2009). Rethinking policing: The policy implications of hot spots of crime. In N. A. Frost, J. D. Freilich, & T. R. Clear (Eds.), *Contemporary issues in criminal justice policy: Policy proposals from the American Society of Criminology conference* (pp. 251–264). Belmont: Wadsworth and Cengage Learning.
Mazerolle, L., Hurley, D., & Chamlin, M. (2002). Social behavior in public space: an analysis of behavioral adaptations to CCTV. *Security Journal, 15*, 59–75.
Mazerolle, L., Soole, D., & Rombouts, S. (2007). Drug law enforcement: a review of the evaluation literature. *Police Quarterly, 10*(2), 115–153.
Mazerolle-Green, L., Price, J., & Roehl, F. (2000). Civil remedies and drug control: a randomized field trial in Oakland, California. *Evaluation Review, 24*(2), 212–232.
McFadden, T. (2005). Automatic license plate recognition systems emerge as a law enforcement tool. *OLETC Today.*, April 2005
Musheno, M. C., Levine, J. P., & Palumbo, D. J. (1978). Television surveillance and crime prevention: evaluating an attempt to create defensible space in public housing. *Social Science Quarterly, 58*(4), 647–656.
Nagin, D. S. (1998). Criminal deterrence research at the outset of the twenty-first century. In M. Tonry (Ed.), *Crime and Justice* (Vol. 23, pp. 1–42). Chicago: University of Chicago Press.
National Research Council. (2004). In W. Skogan & K. Frydl (Eds.), *Fairness and effectiveness in policing: The evidence*. Washington, D.C: The National Academies Press.
Ohio State Highway Patrol. (2005). *Automatic plate reader technology.* OH: Planning Services Section, Research and Development Unit.
PA Consulting Group. (2003). *Engaging criminality—denying criminals use of the roads*. London: Author.
PA Consulting Group. (2006). *Police standards unit: Thematic review of the use of automatic number plate recognition within police forces*. London: Author.
Patch, D. (2005, January 1). License plate scanners lead to recovery of stolen vehicles. *Toledoblade.com*. Retrieved January 11, 2007 from: http://toledoblade.com/apps/pbcs.dll/article?AID=/20050105/NEWS11/501050412.

Pierce, G., Spaar, S., & Briggs, L. (1988). *The character of police work: Strategic and tactical implications*. Boston: Center for Applied Social Research.
Plouffe, N., & Sampson, R. (2004). Vehicle theft and theft from autos in parking lots in Chula Vista, CA. In M. G. Maxfield & R. V. Clarke (Eds.), *Understanding and preventing car theft. Crime prevention studies, Vol. 17* (pp. 147–171). Monsey: Criminal Justice Press.
Police Executive Research Forum. (2008). *Violent crime in America: What we know about hot spots enforcement*. Washington, D.C.: Author.
Police Executive Research Forum. (2012). *How are innovations in technology transforming policing?* Washington, DC: Author.
Potchak, M., McCloin, J., & Zgoba, K. (2002). A spatial analysis of criminal effort: Vehicle theft in Newark, New Jersey. *Criminal Justice Policy Review, 13*(3), 257–285.
Ratcliffe, J. H., Taniguchi, T., Groff, E. R., & Wood, J. D. (2011). The Philadelphia foot patrol experiment: a randomized controlled trial of police patrol effectiveness in violent crime hot spots. *Criminology, 49* (3), 795–831.
Rengert, G. (1996). Vehicle theft in Central Philadelphia. In R. Homel (Ed.), *Policing For prevention: Reducing crime, public intoxication and injury. Crime prevention studies, Vol. 7* (pp. 199–219). Monsey: Criminal Justice Press.
Rice, K. J., & Smith, W. R. (2002). Socioecological models of automotive theft: integrating routine activity and social disorganization approaches. *Journal of Research in Crime and Delinquency, 39*(3), 304–336.
Riecken, H. W., Boruch, R. F., Campbell, D. T., Caplan, N., Glennan, T. K., Pratt, J. W., et al. (1974). *Social experimentation: A method for planning and evaluating social programs*. New York: Academic.
Rubin, D. B. (1974). Estimating causal effects of treatments in randomized and nonrandomized studies. *Journal of Educational Psychology, 66*(5), 688–701.
Scott, M. S. (2003). *The benefits and consequences of police crackdowns*. Washington, DC: Office of Community Oriented Policing Services, U.S. Department of Justice.
Shadish, W. R., Cook, T. D., & Campbell, D. T. (2002). *Experimental and quasi-experimental designs for generalized causal inference*. New York: Houghton Mifflin.
Sherman, L. (1990). Police crackdowns: Initial and residual deterrence. In M. Tonry & N. Morris (Eds.), *Crime and justice: An annual review of research* (Vol. 12, pp. 1–48). Chicago: University of Chicago Press.
Sherman, L. W., & Rogan, D. P. (1995). Deterrent effects of police raids on crack houses: a randomized, controlled experiment. *Justice Quarterly, 12*(4), 755–781.
Sherman, L., & Weisburd, D. (1995). General deterrent effects of police patrol in crime hot spots: a randomized, controlled trial. *Justice Quarterly, 12*(4), 625–648.
Sherman, L., Gartin, P., & Buerger, M. (1989). Hot spots of predatory crime: Routine activities and the criminology of place. *Criminology, 27*(1), 27–56.
Taylor, B., Koper, C. S. & Woods, D. J. (2011a). *Combating auto theft in Arizona: A randomized experiment with license plate recognition technology.* Final report to the National Institute of Justice, U.S. Department of Justice. Washington, DC: Police Executive Research Forum. http://www.policeforum.org/library/technology/FinalreportPERFLPRstudy12-7-11submittedtoNIJ.PDF.
Taylor, B., Koper, C. S., & Woods, D. J. (2011b). A randomized control trial of different policing strategies at hot spots of violent crime. *Journal of Experimental Criminology, 7*(2), 149–181.
Taylor, B., Koper, C. S., & Woods, D. J. (2012). Combating auto theft in Arizona: a randomized experiment with license plate recognition technology. *Criminal Justice Review, 37*(1), 24–50.
Telep, C. T., Mitchell, R. J., & Weisburd, D. L. (2012). How much time should the police spend at crime hot spots? Answers from a police agency directed randomized field trial in Sacramento, California. *Justice Quarterly.* doi:10.1080/07418825.2012.710645.
Warner, B. D., & Pierce, G. L. (1993). Reexamining social disorganization theory using calls to the police as a measure of crime. *Criminology, 31*(4), 493–517.
Weisburd, D. (1993). Design sensitivity in criminal justice experiments. In M. Tonry (Ed.), *Crime and justice: An annual review of research* (Vol. 17, pp. 337–379). Chicago: University of Chicago Press.
Weisburd, D. (2008). *Place-based policing*. Washington, DC: Police Foundation.
Weisburd, D., & Eck, J. E. (2004). What can police do to reduce crime, disorder, and fear? *Annals of the Academy of Political and Social Sciences, 593*(1), 42–65.
Weisburd, D., & Green, L. (1995). Policing drug hot spots: the Jersey City drug market analysis experiment. *Justice Quarterly, 12*(4), 711–735.
Weisburd, D., Lum, C., & Petrosino, A. (2001). Does research design affect study outcomes in criminal justice? *Annals of the American Academy of Political and Social Sciences, 578*(1), 50–70.

Weisburd, D., Bushway, S., Lum, C., & Yang, S. M. (2004). Trajectories of crime at places: a longitudinal study of street segments in the city of Seattle. *Criminology, 42*(2), 283–321.

Weisburd, D., Wycoff, J. A., Ready, J., Eck, J. E., Hinkle, J. C., & Gajewski, F. (2006). Does crime just move around the corner? A controlled study of spatial displacement and diffusion of crime control benefits. *Criminology, 44*(3), 549–592.

Weisburd, D., Morris, N., & Groff, E. R. (2009). Hot spots of juvenile crime: a longitudinal study of arrest incidents at street segments in Seattle, Washington. *Journal of Quantitative Criminology, 25*(4), 443–467.

Weisburd, D., Groff, E. R., & Yang, S. M. (2012). *The criminology of place: Street segments and our understanding of the crime problem*. New York: Oxford University Press.

Welsh, B. P., & Farrington, D. (2008). Effects of closed circuit television surveillance on crime. *Campbell Systematic Reviews, 2008*, 17. doi:10.4073/csr.2008.17.

Christopher S. Koper is an Associate Professor in the Department of Criminology, Law and Society at George Mason University. He is also a senior fellow in George Mason's Center for Evidence-Based Crime Policy and co-director of the Center's evidence-based policing research program. Dr. Koper holds a Ph.D. in criminology and criminal justice from the University of Maryland and has worked for organizations including the Police Executive Research Forum, the University of Pennsylvania, the Urban Institute, the Rand Corporation, and the Police Foundation. He specializes in issues related to policing and firearms policy.

Bruce G. Taylor Ph.D., is a Senior Fellow for NORC at the University of Chicago. He has conducted studies in policing, school violence prevention, treatment for violent offenders, victimization, dynamics of drug markets, and hardcore drug use. His current research includes randomized experiments on school violence prevention, measuring the prevalence and risk factors for teen dating violence, conducting a national survey on services for crime victims and studying ways to improve police and correctional officer health and safety. He has published numerous peer reviewed publications, government reports, and guidebooks and led over 50 government and privately funded studies. He received his Doctorate from Rutgers University in 1996.

Daniel J. Woods is a Research Associate at the Police Executive Research Forum (PERF). While at PERF he has worked on survey construction, data management and analysis in different areas of law enforcement. Prior to joining PERF, he worked as a Research Specialist with the Jerry Lee Center of Criminology at the University of Pennsylvania. He was primarily responsible for analyzing data from the Reintegrative Shaming Experiments (RISE), conducted in Canberra, Australia. He received a M.A. in Criminology & Criminal Justice from the University of Maryland and his PhD. in Criminology from the University of Pennsylvania.