Journal of Experimental Criminology (2023) 19:213–235
https://doi.org/10.1007/s11292-021-09485-8

Hot spots policing in Las Vegas: results from a blocked randomized controlled trial in chronic violent crime locations

Nicholas Corsaro[1] · Robin S. Engel[1] · Tamara D. Herold[2] · Murat Yildirim[1] · Ryan T. Motz[1]

Accepted: 17 August 2021 / Published online: 5 October 2021

Abstract

Objectives This randomized experiment explored the impact of hot spots policing (HSP) strategies on criminal offenses and calls for service within chronic, persistent violent crime clusters in Las Vegas, NV.

Methods Forty-four street segments were randomized into treatment ($N=22$) and control ($N=22$) conditions across nine chronic, persistent violent crime areas. The conditions (foot patrols, stationary patrol vehicles, and business-as-usual) were active for 6 months.

Results Over 90% of the hot spots experienced an average of 1.5 h or greater of patrol dosage per day. In terms of impact, the mixed effects negative binomial regression results showed that the addition of HSP had a marginally significant reduction on overall crime (– 21%), a statistically significant reduction on overall calls for service (– 25.7%), and a statistically significant reduction of 34% on violent calls for service.

Conclusions Reductions in crime and calls for service occurred in settings where enhanced patrol resources were already deployed. The findings did not demonstrate any evidence of a ceiling effect for HSP to impact crime and violence, even where additional patrol resources were already higher than normal.

Keyword Hot spots policing; Randomized controlled trial; Ceiling effects; Crime reduction; Las Vegas

Hot spots policing (HSP) is a proactive crime prevention approach that concentrates police deployment in small geographic locations with disproportionate numbers of

✉ Nicholas Corsaro
nicholas.corsaro@uc.edu

1 University of Cincinnati, Cincinnati, USA

2 University of Nevada – Las Vegas, Las Vegas, USA

crimes and citizen-generated requests for police assistance (Braga, 2001; Weisburd & Braga, 2003). Today there is little disagreement that when correctly implemented, HSP strategies have a significant and moderate impact on crime in targeted locations (Braga & Weisburd, 2020; Weisburd & Majmundar, 2018). The establishment of comprehensive crime-and-place research, coupled with a series of rigorous systematic reviews of HSP via quasi-experiments and randomized controlled trials (RCT), has demonstrated a consistent crime prevention influence. This evidence has resulted in a shift from questioning whether effects are likely, to designing research studies that can better assess the differential types of HSP effects in divergent settings.

Within the backdrop of this robust body of evidence, the purpose of this study is to extend this line of inquiry to examine whether targeted crime outcomes can be influenced by (1) scale attenuation, specifically "ceiling effects" within controlled settings, and (2) whether a relatively under-tested patrol allocation (stationary vehicle with emergency lights flashing) can have substantive impact within crime hot spots given its widespread and previously established influence on reducing speed and vehicle accidents in construction zones (see Richards et al., 1985).

Using data gathered in partnership with the Las Vegas Metropolitan Police Department (LVMPD), the current study enhances the literature in two important ways. First, ceiling effects are a common issue in case–control RCTs—a common staple of HSP evaluations. Although statistical correction techniques are frequently employed to address ceiling effects (e.g., Tobit regression estimation; see McBee, 2010), research designs in many public health studies focus only on samples of subjects who are less likely to exhibit floor/ceiling effects on targeted outcomes (Golla et al., 2018). Conducting research within more inclusive samples that are at risk for floor and ceiling effects provides an opportunity to examine the potential limiting impacts of strategic interventions. This is also relevant for police executives as they determine how to best concentrate limited resources to gain even greater reductions in crime.

Second, as seen in an updated systematic review on HSP (Braga et al., 2019), increased foot and vehicle patrols are commonly combined into an overall patrol metric when examining differential types of HSP initiatives (e.g., increased patrols, drug enforcement operations, offender focused apprehension programs, and actively monitored CCTV with directed patrols). Yet, it remains unknown which of these tactics within the larger HSP inventions is the most effective. Therefore, it is important to assess, within the patrol archetype, whether one intervention approach or the other (e.g., foot patrol versus stationary patrol with emergency lights activated) leads to greater reductions in crime. While police agencies have become more efficacious in identifying and deploying officers to crime hot spots, what officers actually do at these locations has remained within the proverbial "black box" of interventions.

To address these limitations, we examine a 6-month HSP case-control RCT in Las Vegas, NV. The uniqueness of the current study is that the experimental design occurred within broader "chronic violent crime clusters" previously identified by the LVMPD for additional deployment. Within these clusters, the LVMPD had—prior to the experiment—devoted a special violent crime prevention task force to operate in a proactive manner by relieving officers from responding for calls for service and focusing on emergent violence problems. For the HSP experiment, uniform police deployment was added to the concentrated deployment efforts already in place. In short, this study examined the impact of traditional HSP dosage (i.e., two total hours of additional patrol allocation per day, delivered in 15-min intervals every 2 h during peak periods of activity) in locations where additional patrol resources were already allocated to reduce crime and violence. Within these hot spots, the police tactics were also randomized during the experimental period, with officers assigned to one of two conditions: stationary in-vehicle with emergency lights activated or foot-based proactive patrol.

Current study findings suggest that (1) even where police are specifically devoting enhanced resources to focus on violence, the addition of HSP policing presence had a clear and statistically significant impact on both overall crime and calls for service during the experimental period in all bivariate and multivariate estimates; and (2) the bivariate results showed that a stationary vehicle with emergency lights flashing potentially reduced crime at a higher rate than foot-based patrol; however, the multivariate estimates demonstrated that differential patrol allocation (stationary vehicle with emergency lights flashing versus foot-based patrol) did not have significant divergent impacts on targeted crime outcomes. At a minimum, the current study shows a stationary vehicle with emergency lights flashing is at least as effective as walking patrols, and that future research should unravel potential impact where statistical power is more sufficient to detect possible divergent effects. Finally, sensitivity and supplemental analyses also indicated no evidence of displacement to immediately adjacent street segments, adding to the growing body of literature examining displacement and diffusion of benefits. The implications of the current study results are considered in light of recent research designs that demonstrate the growing confidence in the external validity of HSP, and the policy implications for police agencies tasked with doing more to reduce harm in high-crime neighborhoods.

Hot spots policing framework

The theoretical foundation of HSP can be directly linked to the development of situational crime prevention (Clarke, 1980), which focused on understanding the opportunities that emerge at high-risk geographic contexts. Foundational crime-and-place research demonstrated that significant clustering occurs at a small number of places. For example, a highly influential study conducted in Minneapolis by Sherman and colleagues (1989) found that only 3% of addresses in the city were responsible for 50% of all emergency calls to police in a single year. Near identical levels of concentration at addresses for emergency calls to the police were also found in Boston (Pierce et al., 1988). Extending the intersection between crimes and geographic context further, it has also been demonstrated that, while most locations have none to very few crimes, certain locations consistently experience crime-clustering with great stability. For instance, using street segments in Seattle, Weisburd and colleagues (2004) showed that less than 5% of the city's street segments produced 50% of all crime incidents over a 14 years. In addition to consistent findings of high concentrations for all police calls for service or all crime incidents, some studies have reported high-crime concentrations for specific crime types as well (e.g., Braga et al., 2011; Sherman et al., 1989; Townsley et al., 2003; Weisburd et al., 2009). As a result of the collection of consistent findings of crime concentration across studies, Weisburd (2015) proposed the *law of crime concentration* in which he declares, "This law states that *for a defined measure of crime at a specific microgeographic unit, the concentration of crime will fall within a narrow bandwidth of percentages for a defined cumulative proportion of crime*" (p. 138) (emphasis in original). In all, this narrow bandwidth of percentages for a defined cumulative proportion of crime is approximately 25% and 50% of crime concentrated in just 1% and 3% of the microgeographic units within a city (Weisburd, 2015).

Early studies documenting "hot spot" locations led Sherman and Weisburd (1995) to conduct a randomized experimental study with cooperation from the Minneapolis Police Department to test the impact of proactive preventative police patrol within targeted hot spot areas. Due to the conclusions drawn by Kelling and colleagues (1974) in their Kansas City Preventative Patrol Experiment, the prevailing wisdom was that preventative patrol by police had no direct impact on preventing crime. Yet Sherman and Weisburd (1995) noted an important limitation to the Kelling and colleagues (1974) study: the unit of analysis should not be neighborhoods or patrol beats but should instead be hot spots. Therefore, in Minneapolis, 110 hot spots were split into two equal treatment and control groups where the treated hot spots received two to three more times police patrol presence than the control hot spots. The conclusions of the study contradicted the findings from Kelling and colleagues (1974) from two decades earlier. Specifically, the treatment hot spots experienced 6% and 13% reductions in calls for service relative to the calls for service in the control hot spots. Such findings led Sherman and Weisburd (1995) to conclude, "…it is time for criminologists to stop saying 'there is no evidence' that police patrol can affect crime" (p. 647).

Based on the promise of the Minneapolis Hot Spots Experiment, a series of rigorous evaluations further illustrated that additional police presence has the capacity to reduce gun violence (Sherman & Rogan, 1995), violent crime (Ratcliffe et al., 2011; Taylor et al., 2011), and street drug markets (Weisburd & Green, 1995) in areas with concentrated crime incidents. In 2004, the National Academy of Sciences concluded that HSP is one of the most effective crime prevention strategies to reduce crime and violence (National Research Council, 2004).

The proliferation of the HSP movement within policing as well as academic circles have been driven largely by the continued advancements in both research and practice. Rather than resting on the laurels of prior findings, HSP implementation and evaluation research continues to ask key questions that are critical to establishing deployment practices that are effective, efficient, and equitable. For example, researchers and practitioners continue to focus on HSP's unintended consequences, impact across various settings, and the potential for residential perceptions of backfire effects in targeted neighborhoods (e.g., Kochel & Weisburd, 2017). Likewise, research also continues to assess and demonstrate a lack of displacement but rather a consistent pattern of diffusion of crime benefits (Braga & Weisburd, 2020; Braga et al., 2019).

Although the internal validity of HSP is largely unquestioned, its generalizability remains open for further examination. Broadly speaking to this topic, Peters and colleagues (2018) specifically noted that external validity is more difficult to establish for RCTs occurring in a development context, implemented at a smaller scale and in specific localities, which is the very nature of HSP initiatives. Likewise, Nezu and Nezu (2008) illustrate the large number of threats to the validity of RCTs, which often go neglected and understated in strongly controlled, real-world applications. Thus, while RCTs have been widely adopted in HSP evaluations, the need for studies to investigate external validity continues to be important.

Speaking to hot spots research, Taylor and Ratcliffe (2020) clearly demonstrate that spatial scaling is multidimensional and must be weighed heavily in this line of field research. Specifically, Taylor and Ratcliffe demonstrated that a policing intervention at a small scale (e.g., a 500 foot by 500 foot radius) can have a major proportional pre/post-intervention change but still lead to problems in terms of detecting impact via conventional statistical analysis due to the bias toward the null hypothesis (extremely low power). Conversely, “scaling up” an intervention into higher geographic units can dramatically reduce the required impact of an intervention to detect an effect from a statistical power perspective. However, the literature shows that focusing on small crime spots (e.g., intersections, addresses, and street segments) offers considerable pragmatic advantages—thus creating a need to balance the size of the underlying problem relative to the size of detecting and being able to measure effects. The current Las Vegas study had to balance these same issues when designating the units of focus for the intervention.

Another under-tested inquiry is whether HSP can impact crime in divergent areas, given that the majority of studies have taken place in large urban contexts. Only recently did Koper and colleagues (2021) demonstrate that HSP can indeed impact crime in smaller urban contexts. In addition, more recent advancements that attempt to build the evidence of the generalizability of HSP show that the strategy is effective beyond the USA and Australia, such as in Bogota, Columbia (Mejia et al. 2015) and Montevideo, Uruguay (Chainey et al., 2021), and lesser so in Medellin, Columbia (Collazos et al., 2020). Thus, the strengths and limitations of HSP, as well as the contexts in which it seems to operate best, continue to be a focal point in field settings and among academic circles. The Las Vegas experiment provided an opportunity to address many of these ancillary lines of inquiry.

Study setting, design, and research questions

The City of Las Vegas is located within Clark County, Nevada, which has a county population of approximately 2.2 million people and a city proper population of roughly 600,000 residents. The county grew by approximately 14% from 2010 to 2018 (the study period), with an estimated median household income of $56,000. In terms of racial/ethnic composition of the population, approximately 69.9% are White, 12.8% are Black, 10.4% are Asian, and 6.9% are two or more races or of other races (US Census Bureau, 2021). In addition, approximately 31.4% of the population is of Hispanic or Latino descent. Overall, the City of Las Vegas and the larger surrounding Clark County have fairly similar demographics.

The LVMPD provides all policing services for the City of Las Vegas and Clark County, Nevada (excluding the cities of Henderson, North Las Vegas, Boulder City, and Mesquite). In total, the LVMPD serves a geographic jurisdiction of 7500 square miles, with a population of approximately 1.6 million—more than half of the population of the state of Nevada. The LVMPD is the largest police department in the State of Nevada, with 3200 sworn police officers and 1300 civilian employees, and is divided into nine urban Area Commands.[1] In 2017, the LVMPD recentralized gang intelligence, investigations, and enforcement actions into a new bureau, the LVMPD Gang/Vice Bureau. The hot spots deployment experiment was specially overseen by the Law Enforcement Operations Group of the LVMPD.

Hot spot randomization selection

In spring 2018, the research team began working with LVMPD police and dispatch executives, crime analysts, and patrol supervisors to develop a rigorous RCT to implement and assess the impact of directed patrol allocation to its highest risk locations within the city. At the onset of the design phase, the research team identified the top street segments within the city that had the largest volume of violent citizen-generated calls for police assistance. This study follows the first hot spots policing experiment, the Minneapolis hot spots experiment (see Sherman & Weisburd, 1995), by operationalizing high-crime concentration within street segments as our operationalization for hot spots randomization. It was imperative for the research team to suggest to the LVMPD police department that their hot spots policing initiative should have intensive dosage in highly concentrated microgeographic places (Wilcox & Eck, 2011: p. 476). Street segments fall within the broader "microgeographic units" within the criminology of place foundation (e.g., addresses, facilities, street segments, and clusters of street segments) show the incredible stability and persistence of crime (for a detailed analysis of the stability of crime at street segments, see Weisburd et al., 2004). The intent at this stage was to randomize half of the segments into treatment and half into control conditions; however, departmental logistics also became a focal point in the RCT design.

The LVMPD were concerned their resources would be too constrained to allocate the recommended hot spot patrol assignments due to staffing levels. The increased citizen-request to officer availability volume concerned district commanders that too few officers would be available to support citizen requests for assistance in a timely fashion. Indeed, LVMPD had roughly 17% fewer uniformed officers beginning in 2018–2019 relative to 2015–2017 (see UCR 2021).[2] LVMPD were willing to engage in an implementation design as long as the hot spots were assigned to the chronic, persistent violent crime areas that housed specialized officers who were

[1] LVMPD Area Commands include Bolden, Convention Center (which includes the Las Vegas Strip), Downtown, Northeast, Northwest, Southeast, Spring Valley, Enterprise, and South Central.

[2] Empirically, the FBI UCR data supported this LVMPD concern by demonstrating that the officer-to-citizen ratio was at a 5-year low in 2018 (1.8 officers per citizen) relative to 2015–2017 (2.2 officers per citizen).

released from responding to calls for service to focus on emergent violent crime problems. Once the research team assessed whether there would be enough treatment (and control hot spots) in these areas, the experimental framework was derived from these chronic, persistent violent crime areas.

Chronic, persistent violent crime areas

In January 2018, the LVMPD developed boundaries for chronically persistent high-violence areas across its various area commands. A team of officers, including one Sergeant and nine officers, conducted proactive patrols within the persistent hot spot boundaries to address violent crime when and where it emerged. To provide context, the chronically persistent high-crime areas that accounted for roughly 13% of the space within the city identified by the LVMPD comprised 23.6% of all citizen-generated calls for service and 20.3% of all crime reports in 2018. Officers assigned to the specialized violent crime task force do not respond to calls for service but rather engage in proactive work in the persistent crime areas. These activities include weekly deployment meetings to focus on specific areas in the violent crime areas, a focus on places and people, discussions regarding deployment strategies with Area Command Captains, and daily deployment meetings where they focus on any active crime in the areas, as well as a focus on violent crime that occurred within the past 24 h.

The boundaries for hot spot (treatment and control) selection were within these LVMPD-identified chronic, persistent violent crime clusters. It was determined that the number of officers needed to commit the standard 15 min of dosage (see Koper, 1995), randomly, every 2 h across multiple shifts (equating to roughly 2 h of total dosage per day) for 6 months would have overwhelmed the agency's patrol capacity. Therefore, when assessing the volume of call for service and reported crimes at the street segment level (see Weisburd et al., 2012) within the chronic violent crime areas, as well as the resources available to devote sufficient officer time to crime hot spots, the collaborative team elected to implement the strategy in all but one of its area commands.[3]

Another major concern for the experiment (and the subsequent evaluation) was that designating the treatment and controls allocated within specific macro-geographic areas with additional police allocation (to address violence) may bias the results of the study. A series of supplemental time series tests (presented in the Appendix) indicate that the onset (or duration) of the intensive policing within the chronic, persistent violent crime areas did not impact crime in any meaningful way, which is consistent with the prior literature that patrol allocation in large geographic units will have limited impact on crime (see Kelling et al., 1974).[4]

[3] Convention Center Area Command (CCAC) did not have enough high-risk segments (for both treatment and control) to include in the study.

[4] We also included time-series analyses where the post-intervention period ended prior to the start of the experiment (i.e., October 2018), and the results were virtually identical to those in the Appendix. There was no evidence of a significant change in crime in the chronic high-crime areas relative to the rest of the city in these results—suggesting that additional police presence in these larger geographic areas alone did not yield significant changes to crime.

Study design

A randomized experimental design was employed where treatment group street segments received additional patrol allocation (i.e., hot spots policing), and control group street segments received policing as usual. All treatment and control street segments in the randomized framework were within the chronic, persistent high-violence areas. For the purpose of this experiment, each LVMPD Area Command was willing to designate their pre-existing violent crime units (VCU) within the chronic, persistent high-crime areas to provide additional patrol saturation at the identified high-crime hot spots (i.e., street segments). The research team paired treatment and control street segments so that treatment and control hot spots were relatively similar, prior to randomization across the various districts. Upon block pairing, a computerized random number generator assigned treatment and control allocation. The control street segment locations remained unknown to LVMPD officials until after the experiment concluded.

Specific experimental parameters were developed. First, the identified hot spot street segments assigned to both treatment and control conditions had to be located within the chronically persistent high-crime areas to ensure sufficient patrol dosage during the experiment. This led to a higher likelihood that the hot spot street segments within the chronically persistent high-crime areas would have strong dosage compliance because the VCU officers devoted their energies to fulfilling the requirements of the experimental conditions. In short, intensive patrol allocation to these areas was consistent with the specialized unit officers' overall mission and prior practices to reduce violence within these predetermined areas.

A naïve randomization approach would likely have yielded unequal groups for comparative purposes. Thus, for the nine chronic violent crime areas included in the study, we followed Weisburd and Gill (2014) by conducting a blocked randomized controlled trial to maximize efficiency, produce an equivalent treatment to control segments on pre-intervention outcomes as possible and to enhance statistical power with a limited experimental size. In terms of ensuring equality within blocks (i.e., block assignment within chronic violent crime areas), we assessed pre-intervention outcomes (total offenses and calls for service) for each of the segments eligible for the experiment. We conducted a 1–1 case-control fully blocked match in chronic crime areas where only one hot spot was the focus of the intervention (1/9 areas) and partially blocked randomization at the highest, medium, and lowest levels in each chronic crime areas where two or more hot spots were included in the treatment assignment (8/9 areas) to avoid the loss of statistical power.

Second, prior research guided street segment inclusion criteria, particularly given our attempt to follow the initial Minneapolis hot spots experimental framework (Sherman & Weisburd, 1995). Specifically, no hot spot was larger than one standard linear street block (see Anderson & Malleson, 2011), did not extend for more than one-half block from either side of an intersection, and was not within one standard

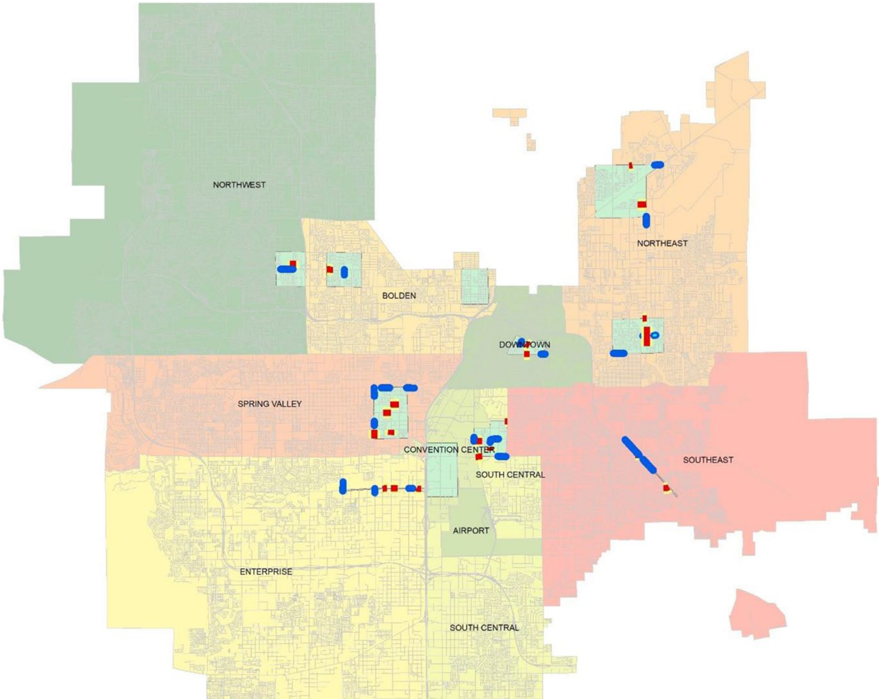

Fig. 1 LVMPD treatment and control segment allocation. Blue = control. Red = treatment. Though difficult to visualize in this graphic, each treatment and control segment is separated by at least one standard street segment

linear block of another hot spot.[5] Third, these factors resulted in a designation of 44 hot spots (i.e., street segments) that were allocated to treatment (*N* = 22) and control (*N* = 22) conditions.[6] These street segments are visually displayed in Fig. 1.

The treatment delivery plan followed Koper's (1995) seminal findings that patrol allocation should range between 12 and 15 min of patrol allocation per treatment received at each hot spot (see also Telep et al., 2014).[7] To allocate patrol resources to hot spot treatment locations, the LVMPD utilized radio control dispatchers to produce police-generated calls for service (referred to as dispatch code "469-T") to the midpoint of the treatment street segment at randomly designated time periods throughout the day. This deployment method ensured that the number of dispatch requests at each hot spot (1) had no more than 2 h between treatment dispatches,

[5] In circumstances where two standard linear street segments abutted one another, we randomly removed one of them from the potential pool prior to randomizing to ensure this criterion was met.

[6] Of the 44 hot spot segments that comprised the treatment and control conditions, the distribution was as follows: 8 in Bolden; 6 in Downtown; 8 in Northeast; 8 in Southeast; 6 in Spring Valley, and 6 in the remaining Area Commands.

[7] The Koper Curve, as it has come to be known within policing research circles, has been assessed and supported more directly by Telep et al. (2014) in their Sacramento, CA study.

(2) had two total hours of patrol intended dosage each day calibrated with the highest risk time periods of calls for assistance and criminal offenses, and (3) instructed the officers which deployment method (stationary with emergency lighting or foot patrol) was to be conducted.[8] The daily experimental schedule included sixteen consecutive hours during the highest risk times for crime and citizen-generated calls for service (3:00 pm through 7:00 am the following day).

The implementation plan provided 132 monthly treatment units (i.e., 22 treatment areas over 6 months of hot spots intervention). Given that the evaluation literature on hot spots initiatives indicates an expected small-to-moderate impact on crime and calls for service (Braga et al., 2019), the research team conducted power analyses on the planned experimental design. The results of these analyses indicated that it would be possible to detect a moderate effect on our targeted outcomes during the experimental period.[9] Commands of the various VCU teams in these chronically, persistent high-crime areas committed to providing 6 months of additional dosage as directed by the dispatch-generated deployment plan. Given the research team's confidence in LVMPD compliance with the experimental conditions during the study period (since we developed a collaborative experimental framework that was in line with their desire to rely on the VCU officers in each district to handle the dosage of patrol), a second dimension to the hot spots experiment was introduced: randomization of what activities officers performed during hot spots policing deployments.

Randomly assigned treatment to hot spots and dosage compliance

The research team balanced a desire to work with LVMPD to reduce targeted violence by relying on a strongly established intervention (HSP) while also seeking an opportunity to assess the potential impact of lesser-tested, though popular approaches of police activity during HSP deployment. This resulted in two approaches to treatment: (1) walking patrols at the hot spot street segments when deployed, to promote proactive engagement between the assigned officer and residents, business owners/patrons, and others at the designated locations and (2) stationary patrol cars with emergency lights flashing while parked at the designated street segments, to enhance citizen awareness of the additional patrols that were taking place.

Prior research clearly indicates that problem-solving policing approaches hold the most promise to reduce crime and calls for service among hot spots policing approaches (Braga et al., 2014). Within this area of inquiry, many studies have found that foot patrol can lead to a significant reduction in crime at hot spot locations

[8] For dispatched "calls to hot spots" that went unanswered, the dispatchers maintained the call as open until it was answered, or dropped by the beginning of the 7am shift the next day.

[9] The combined calls for service average in the treatment segments were 5.2 total calls for service per unit per month, with a standard deviation of 2.2 calls per month. Using a power calculator (one-tailed test, and a desired power of .70), we found that a 33% reduction in calls for service would be detectable with this design (from 5.2 calls per month to 4.1 per month). While more units of hot spots observation were desirable for a more precise test, a reliable design was possible under these conditions.

within a city (Haberman & Stiver, 2019; Novak et al., 2016; Piza & O'Hara, 2014; Ratcliffe et al., 2011). The combined results from these studies suggest that walking police officers in the hot spots should engage community stakeholders and business officials and that such an approach would be well grounded within the evidence-based literature of hot spots policing effectiveness.

The second approach, the use of flashing emergency lights during the treatment period, is based on a widely adopted risk-reduction practice in construction work zones—the use of police presence with emergency lighting to deter traffic speed violations and reduce accidents. As noted previously, Richards et al. (1985) found that visible police presence via flashing lights reduced speeds by roughly 18% in work zones. The use of police presence via flashing lights has been widely adopted as evidence-based practice in construction zones for the past 30 years; however, the adaptability of emergency lights to criminal justice initiatives (i.e., crime prevention as opposed to accident risk reduction) is unclear. To date, the research is less extensive regarding the impact of illuminated emergency lighting on crime in high-crime patrol hot spots.[10]

In summary, the LVMPD hot spots experiment was designed to address the following research questions:

1. Scale Attenuation-Threshold Effects. What was the impact of hot spots policing on crime within street segments that are contained within chronically persistent high-crime areas? In short, what was the impact of additional patrol allocation above and beyond the already higher-than-normal patrol allocation experienced within the chronic, persistent violent crime clusters?
2. Impact of Differential Patrol Allocation in Hot spots. What was the impact of different types of police patrol dosage treatments, and do different treatment types correlate with differential changes in crime reduction? In short, what is the most promising activity for police to perform during additional patrols? Patrol officers have long used police vehicle lights to reduce speeding (e.g., in work zones), as well as proactive patrols (e.g., walking foot patrols) to reduce crime risk in high-crime areas. By randomly assigning different types of treatment to the different hot spots, we assess whether patrol treatment type corresponds with differential changes in crime outcomes.

[10] Johnson (2019) describes a study in which night patrol shifts were randomized to either be completed with static red or blue cruise lights activated or no lights in three low-crime residential towns and one small city along the Connecticut shoreline. 61 "lights-on" nights were compared to 61 "lights-off" nights. With property crime as an outcome, the study found that auto burglary and auto theft were reduced by 16% and 44%, respectively. These reductions, however, were not statistically significant.

Hot spots implementation compliance

Of the 22 treatment segments, researchers used computer randomization to assign one of two treatment types—walking or stationary with lights—to each hot spot. This assignment lasted 1 month and then re-random assignment to the treatment locations occurred. The second randomization was based on the proportion of total hot spots that would equate to a 1:1 treatment type match over a 2 months. This iterative approach ensured that the 132 monthly treatment units in the intervention period had an even distribution of 66 treatments of walking patrols and 66 treatments of stationary with lights. During the transmission of the 469-T calls for police presence at the hot spots, dispatchers would communicate to the officer the pre-programmed and random assignment of the action to be taken at the hot spots.

An examination of dosage compliance, based on cleared calls to the dispatch center, indicates that 100% of the hot spots experienced at least 1.25 h of patrol dosage per day over the 6-month HSP experimental period. Indeed, over 90% of the hot spots ($n=19$) experienced an average of 1.5 h or greater of patrol dosage per day over the 6-month intervention period. And roughly 55% ($n=12$) of the hot spots averaged the intended 2 h of patrol dosage per day over 6 months.

The high level of patrol allocation compliance observed can be attributed primarily to the collaborative implementation design between the research team and feedback from the LVMPD Area Commanders prior to programmatic onset (to ensure compliance was feasible and attainable with current resources). In addition, the VCU officers committed and responded to deployment for multiple dosages per day to the hot spot locations. Finally, dedicated and immediate oversight by the Area Commanders, including prompt response to initial reports from the research team of low compliance,[11] served to convey to officers the importance of meeting the study requirements and proper dosage in the hot spots.[12]

[11] At the beginning of the experiment, the research team collected "weekly" dosage/compliance data from the dispatcher (i.e., percent of calls "responded to" that designated hot spots assignment and activity to the various VCU officers. There were some units that had lower than 50% compliance in the first week. The research team shared those findings with the area commanders, where compliance was both high (to reinforce success) and low (to alter low response rates to the hot spots). In the second week, the rates of compliance were considerably higher and remained stable for the experiment. This is not to suggest each area command was perfect at all times. Rather, the research team noticed an average of roughly 80% compliance rates or higher for a given month. Ideally, we would have included a verifiable manner to measure compliance (e.g., vehicle tracking analyses) but these technological advancements were not available. The "visits" to hot spots by the research team and district commanders were anecdotal.

[12] Logistically, our research team was provided anecdotal evidence of Area Commander oversight on compliance rates (including self-reported "occasional" site visits). We believe the primary motivating factor was that the upper administration of LVMPD wanted high compliance and District Commanders who had VCUs with lower response rates would be responsible for answering for low compliance. However, our research team did not conduct site visits to ensure fidelity, nor had access to any type of tracking technology which is a limitation of this design.

Outcome measures and bivariate analyses

To evaluate the impact of hot spots patrols, primary outcome measures from the LVMPD official reported data were collected. Specifically, the research team provided access to LVMPD calls for service data, which included all citizen-generated calls to 911 and patrol dispatch. For calls for services, the research team focused exclusively on calls for assistance for street level crime (i.e., assaults, fighting, shooting, robberies, auto burglaries, burglaries, grand larcenies, stolen motor vehicle, stolen property, larcenies, and property stolen from motor vehicles).[13] In addition, we examined LVMPD-reported crime incidents, which included all Part I crime incidents based on the UCR classification of serious crimes (i.e., homicide, robbery, burglary, auto theft, and aggravated assault, and larceny), and Part II crimes (less serious criminal incidents that are usually related to disorder—e.g., public drunkenness, trespassing, and vandalism), as well as misdemeanor assault. Reported crime incidents were further divided into violent (Part I assaults, robberies, rapes, and murders and Part II assaults) and property crime (burglaries, auto thefts, larcenies, and Part II thefts) categories for analysis. In sum, the following dependent variables were examined: Total crime incidents (Part I and Part II offenses), total calls for service, violent crime incidents, violent calls for service, property crime incidents, and property calls for service.

The primary outcome of interest was the change between the experimental period (Nov 1, 2018–Apr 30, 2019) and the same time period in the treatment versus the control hot spots the year prior (Nov 1, 2017–Apr 30, 2018).[14] We present results below for each of the outcomes listed previously in the treatment and comparison hot spots during the 6-month experimental period (November 2018–April 2019), as well as the same period the previous year (November 2017–April 2018). This approach allowed the estimation of mean differences-in-differences in each of the outcomes to assess whether treatment hot spots experience statistically significant changes in event counts relative to the control hot spots. As a consequence of the blocked RCT design, which has been shown by Weisburd and Gill (2014) to minimize the concern that fewer than 50 treatment sites are needed to accurately estimate programmatic effects (see Farrington & Welsh, 2005), we have heightened confidence that observable differences between the treatment and control groups are likely driven by patrol allocation to the treatment hot spots, particularly due to the high degree of implementation fidelity that occurred during the experimental period.

[13] While focusing on these specific calls for service potentially impacted statistical power, we were also confident in their consistent measurement by the dispatch center. For minor calls for service, we were aware of potential of measurement and reporting inconsistencies and thus we used a more conservative and reliable measure of calls for service related to street crime.

[14] Ideally, we would have compared several pre-intervention periods (e.g., Nov 2016–Apr 2017) with the experimental period to obtain smoothed or average rates of change between the pre-experimental period and the treatment period. However, LVMPD went through a major data systems change in 2017, which made the use of multiple pre-intervention periods impossible. LVMPD actually delayed the onset of their experiment in order for our research team to gather a suitable and matched pre-experimental period for the purpose of strengthening the evaluation.

Table 1 Treatment and control got spots offenses and calls for service reported between the pre-experimental period (11/01/2017–04/30/2018) and the experimental period (11/01/2018–04/30/2019)

Outcome	Treatment hot spots			Control hot spots		
	Pre-experimental period	Experimental period	% Change	Pre-experimental period	Experimental period	% Change
Total offenses	666	594	−10.8%	746	863	+15.6%
Violent offenses	304	290	−4.6%	413	499	+20.8%
Property offenses	362	304	−16.0%	333	364	+9.3%
Total CFS	661	565	−14.5%	671	797	+18.7%
Violent CFS	417	299	−28.3%	445	503	+13.0%
Property CFS	244	266	+9.0%	226	294	+30.0%

The differences between the treatment and control hot spots during the 6-month experiment are presented below. Table 1 shows that during the intervention period, there were a total of 594 total criminal offenses in the treatment hot spots, compared to 666 during the same period 1 year prior (a reduction of 10.8%). Comparatively, there were 863 offenses in the control area during the experiment compared to 746 offenses 1 year prior (an increase of 15.6%). A similar pattern emerged within the treatment hot spots for violent offenses (–4.6%) and property offenses (–16.0%), while the control hot spots experienced increases in violent crimes (+20.8%) and property crimes (+9.3%). For citizen-generated calls for police service, treatment hot spots experienced a 14.5% decline during the experimental period when calls were reduced to 565 from 661 (in the pre-experimental period). Comparatively, total calls for service increased by 18.7% in the control hot spots and, when comparing event counts, they increased from 671 in the pre-experimental period to 797 in the experimental period. A similar difference-in-difference pattern emerged for violent and property calls for service. In total, the relative rate of change ranged from 26% (in total criminal offenses) to 33.2% (in total calls for service) in the treatment hot spots compared to the control hot spots.

Analytic strategy and multivariate results (multilevel mixed effects repeated measures regression)

Following Gelman et al. (2007), we draw upon a hierarchical mixed effects Poisson model to estimate the impact of the Las Vegas experiment on crime and calls for service outcomes.[15] As noted in the "methods" section, the pairing of treatment and control segments that followed a partially blocked randomization process within each chronic violent crime area (which were policed by the localized violent crime units) was likely to share many unmeasured commonalities (e.g., structural dimensions, police presence, underlying factors that lead to street crime). Therefore, to adjust for the place-based randomization process used in this study, which would

[15] The authors' would like to thank this suggestion by one of the reviewers to draw upon this analytical technique.

lead to correlated unobserved errors common to all the street segments—regardless of treatment/control condition—within the nine chronic violent crime areas where randomization took place, we used mixed Poisson regression models to analyze the difference-in-difference predictors on the monthly crime outcomes of interest. The Poisson distribution, controlling for overdispersion, is appropriate for count variables (Greene, 1994). Thus, we relied on hierarchical Poisson regression estimation, correcting for overdispersion (i.e., negative binomial) mixed effects regression estimation for the analyses herein. Overdispersion was estimated using a chi-squared statistic, with standard errors inflated by the square root of the estimated overdispersion. In all analyses, the difference-in-difference estimates (i.e., the 0/1 control/treatment indicator variable, the 0/1 pre-intervention/post-intervention variable, and the treatment*post-intervention interaction term) were the level-1 units and chronic high-crime area where the hot spots were randomized was the level-2 unit. In the analysis, the intercept was modeled as a random effect. Thus, chronic high-crime area intercepts were allowed to vary randomly around the overall intercept. The difference-in-difference predictors were modeled as fixed effects.

Consistent with Telep and colleagues (2014), all regression models presented in the tables rely on a one-tailed distribution given that the body of evidence for hot spots policing clearly indicates that backfire effects for hot spots policing are highly unlikely. Additionally, our analysis estimates models with the same overall hypothesis using six interconnected and correlated outcome variables. Therefore, it may be necessary to adjust the *p*-value for each test upward to counteract the effect of multiple tests and to correct for experiment-wise error rate. The utility of *p*-value adjustments for multiple testing has been heavily debated (see, e.g., Aickin, 1999; Aickin & Gensler, 1996; Feise, 2002; Perneger, 1998, 1999; Rothman, 1990). One limitation being that *p*-value adjustment reduces the chance of making a type I error at the cost of increasing the chance of making a type II error. Nonetheless, given the lack of consensus on whether *p*-value adjustments should be used, we have opted to provide both adjusted (presented in bold in Table 2) and unadjusted *p*-values for our results.[16]

Given that the primary purpose of this evaluation was to assess whether HSP within these heavily and proactively policed areas experienced any kind of significant shift in overall criminal activity, we first examined the change in total criminal offenses and total citizen-generated calls for service. Table 2 shows the results of the difference-in-difference mixed effects negative binomial panel regression estimates for the outcomes of interest to this experiment. The total number of criminal offenses within the treatment hot spots experienced a marginally statistically significant decline of -21.3% (IRR = 0.78, SE = 0.169, $p < 0.10$) during the experimental period relative to the control hot spots, and accounting for prior seasonal trends in

[16] While the Bonferroni correction (Dunn 1961) is likely the most common method for *p*-value adjustment, it has been criticized for being overly conservative—especially when sample sizes are small. Other approaches, however, exist that are uniformly more powerful than the Bonferroni correction. As such, we have chosen to use the Holm procedure to adjust our *p*-values and control for experiment-wise error rate (Holm 1979).

Table 2 Repeated measures mixed negative binomial regression on difference-in-difference crime and calls for service counts

	Total offenses		Total CFS		Violent offenses		Violent CFS		Property offenses		Property CFS	
Fixed effects	B (S.E.)	*Z*-value	B (S.E.)	*Z*-value	B (S.E.)	*Z*-value	B (S.E.)	*Z*-value	B (S.E.)	*Z*-value	B (S.E.)	*Z*-value
Intercept	1.52* (.174)	8.72	1.50* (.135)	11.15	.863* (.214)	4.03	1.04* (.167)	6.19	.768* (.174)	4.41	.475* (.147)	3.23
Treatment	.052 (.124)	0.42	.041 (.104)	0.39	−.123 (.129)	-0.95	.001 (.104)	0.98	.163 (.142)	0.25	.065 (.149)	0.44
Experimental effect	.118 (.118)	1.00	.143 (.101)	1.41	.160 (.121)	1.31	.081 (.102)	0.79	.073 (.137)	0.53	.255 (.144)	1.77
DID estimate	−.239+ (.169)	−1.38	−.297* (.145)	−2.04	−.214 (.179)	−1.19	**−.417* (.149)**	**−2.79**	−.256+ (.194)	−1.32	−.157 (.205)	−0.76
Random effects												
Variance (intercept)	.201		.111		.331		.199		.174		.086	
Model fit												
Number of observations	528		528		528		528		528		528	
Number of groups	9		9		9		9		9		9	
Log-likelihood	−1434.5		−1390.81		−1106.53		−1147.77		−1101.21		−985.94	
Wald *X*^2 (*P*-value)	2.40 (.493)		6.21 (.101)		8.17 (.042)		16.73 (<.01)		2.17 (.538)		3.61 (.306)	

+$p < .10$ (one-tailed uncorrected); *$p < .05$ (one-tailed uncorrected)

Bold = two-tailed with Holm Correction ($N = 6$ tests) crime counts in both treatment and control areas. Similarly, the total number of citizen-generated calls for service experienced a 25.7% statistically significant reduction that corresponded with increased patrol treatment in the randomized hot spots (IRR = 0.74, SE = 0.145, $p < 0.10$).

Table 2 also shows that among criminal offense outcomes, both violent and property offenses decreased, though for violent crimes, the reduction did not reach any statistically significant threshold.[17] However, property crimes experienced a marginally significant decline by roughly 22.6% (IRR = 0.774, SE = 0.194, $p < 0.10$). Thus, the results for the overall crime, property crime, and overall calls for service are suggestive of impact, on the most liberal of scientific assessment tests (one-tailed significance thresholds).

The results in Table 2 also show that the primary driving force behind the overall reduction in calls for service was specifically for violence-related calls for police assistance, which declined by roughly 34.0% (IRR = 0.659, SE = 0.149, $p < 0.05$). Indeed, the violent calls for service decline reached the statistically significant threshold of reduction even relying on a conservative Holm *p*-value correction test (to control for the number of models estimated, relying on a two-tailed distribution). Thus, we have heightened confidence the reduction in the observed reduction in violent calls for service that was associated with timing of the experiment in the hot spot segments relative to the control segments.

Property calls for service also declined; however, the results were not statistically significant. Combined, these results show that overall criminal activity declined in the treatment hot spots, and that where there was a difference in calls for service by type of call, the most stable observed reduction was in reported violent activity.

To address the research question as to which patrol type (walking versus stationary with emergency lights) has the most promise in terms of impacting targeted crime rates, we examined the difference-in-difference-in-difference (DDD) estimator for patrol walking relative to stationary with lights, relative to changes that occurred in the control areas, and controlling for prior seasonal trends. The results in Table 3 demonstrate that despite the initial bivariate findings suggesting greater impact for stationary emergency lighting, more precise analyses indicate that the type of randomly assigned patrol at the different hot spots did not have a differential impact on the overall crime and calls for service reductions associated with the treatment conditions. The same non-significant findings were observed for violence and property-based call and offense-specific outcomes (additional results available from the authors upon request). It should be noted that the differential impact analyses presented in Table 4 are limited in statistical power and are potentially biased toward the null hypothesis, which as noted by Taylor & Ratcliffe (2020) should temper conclusions of these findings.

[17] It is also worth noting that the "treatment" indicator variable is not statistically significant in any of the models presented in Table 2. This suggests that our blocked approach to pairing treatment to control sites yielded treatment and control segments with extremely similar criminal offenses and calls for service—particularly when including the random intercept model in the mixed regression analysis.

Table 3 Difference-in-difference-in-difference count regressions measuring changes between walking and stationary lights on patrol in treatment hot spots relative to control hot spots

	Total offenses			Total CFS		
Fixed effects	B	S.E	*Z*-Value	B	S.E	*Z*-value
Intercept	1.52*	.174	8.72	1.50*	.134	11.15
Treatment	.052	.124	0.42	.041	.104	0.39
Experimental effect	.118	.118	1.00	.143+	.101	1.41
DID estimate	−.226	.189	−1.19	−.285+	.163	−1.74
DDD estimate	−.012	.173	−0.07	−.024	.150	−0.16
Random effects						
Variance (intercept)	.201			.110		
Model fit						
Number of observations	528			528		
Number of groups	9			9		
Log-likelihood	−1434.5			−1390.8		
Wald X^2 (*P*-value)	2.41 (.661)			6.24 (.182)		

$^{+}p < .10$ (one-tailed unadjusted); $^{*}p < .05$ (one-tailed unadjusted)

Discussion

Study findings from the LVMPD experiment showed that HSP had a significant overall impact in the treatment hot spots on total calls for service and total criminal offenses, particularly when comparing the 6-month experiment in 2018–2019 to the same time period in 2017–2018. That overall crime and overall calls for service experienced statistically significant reductions is particularly important, as the vast majority of previous HSP studies finding impact focused on calls for service outcomes, but less on total criminal offenses (with exceptions to specific offense types, such a gun crimes). The context of the LVMPD experiment also has a number of important implications. The hot spots that were randomized in Las Vegas were located within larger, chronic, persistent violent crime areas. These areas were already receiving additional resources to counteract violence prior to the HSP experiment. This high-risk environment most likely minimized statistical power limitations on overall criminal offense outcomes and provided a distinct context to assess various components of the HSP initiative.

While this experiment makes several important contributions to the HSP literature, it is important to acknowledge the limitations of the current investigation. First, LVMPD did not have automatic vehicle location tracking devices at the time of the study to document and track patrol dosage processes (e.g., see Sorg et al., 2014; Weisburd et al., 2015).[18] To address this

[18] The fact that our research team did not have vehicle tracking devices and that the team was remote for the experiment raises concerns about boundary non-compliance during the experiment (see Sorg et al., 2014). One strength of the design however was that the “address” the officers were dispatched to during the experiment was the center of the segments which provides some additional buffer between the treatment/control locations (in particular for the walking officers).

limitation, the research team developed a random implementation dispatch protocol using police-generated calls for service that altered hot spots patrol by the type of treatment (walking vs. stationary vehicles with emergency lighting) and the times that patrol officers were to arrive and depart. This randomly assigned process varied bi-monthly during the 6-month experimental period. The study compliance indicators are based on designated officers responding to and “clearing” the police-generated calls for proactivity through recorded dispatch. LVMPD supervisors also randomly appeared at the hot spots to ensure dosage fidelity; there were no noteworthy concerns of compliance uncovered by the research team.

Second, while we relied upon multiple sources of outcome data (e.g., citizen-generated calls for service and offenses reported), we were unable to examine additional outcomes, such as changes in observed physical and social disorder. The use of ride-alongs and/or surveys of citizens and their perceptions of impact would have provided additional data sources to triangulate the reported findings. While the reliance on official data sources is not unique to the current study, methodological triangulation would have enhanced confidence in the findings.

Despite these limitations, the results of the current experiment specifically show that street segments with chronic, persistent violent crimes can also be impacted by concentrated HSP dosage. These findings also provide further support of Koper’s (1995) initial recommendation, supported by Telep and colleagues (2014), that an additional 15 min of patrol every 2 h can support an effective crime reduction initiative. Thus, intensive patrol time at high-crime hot spots is not needed to reduce crime, but rather short (< 15 min) and intermittent presence in high-crime hot spots can clearly reduce all criminal activity (i.e., both citizen requests for assistance and total criminal offenses). Combined with an updated systematic review which demonstrates the high likelihood of crime prevention impact (Braga et al., 2019), the LVMPD context adds further external validity regarding the promise of HSP in divergent contexts.

Most importantly, the setting for the Las Vegas experiment provided a direct test of the potential limiting factor (i.e., ceiling effects) of HSP initiatives because the street segments selected for treatment were situated in chronic, persistent high-violence clusters that experienced additional patrol and investigative measures on a routine basis. Patrol dosage in these areas was not “business as usual” but was akin to “additional patrol allocation in areas that had more attention paid than most of the rest of the city.” The fact that the HSP treatment significantly impacted calls for service and criminal offenses in the treatment segments within the chronic, persistent violent crime areas but not the control segments (also located within the chronic crime areas) suggests that whatever the threshold of impact is for HSP, it extends beyond moderate to extensive patrol dosage. This finding is particularly important to share with law enforcement officials within the field who question whether adding “additional patrol allocation” via HSP to places with an already greater-than-normal police presence would be beneficial or a waste of limited resources. Anecdotally speaking, this is a concern police officials commonly shared with our research team whenever we have suggested HSP as a way to reduce violence; in many police jurisdictions, high-risk locations already experience higher-than-normal patrol allocation in comparison with the rest of the city. The LVMPD findings suggest that moderately more time and intermittently dispersed patrol allocation (15 min every 2 h during peak periods within the day) within the targeted areas can generate even more substantive reductions in crime outcomes.

This study also has another important implication: the use of stationary patrol with lights flashing at the midpoint of each treatment segment—an approach that is widely employed in policing—transcends risk reduction in construction zones and can also impact crime rates in hot spots. While the bivariate results suggested a greater likelihood of programmatic impact for flashing lights versus walking patrols, the DDD estimates did not yield significant findings. The statistical power tests used in this study, however, were not designed to discern differential impact types but were rather designed to assess treatment versus control effects. The current study shows that patrol cars with emergency flashing lights have the potential for substantive effect, and at a minimum, likely impacts crime in high-crime contexts virtually identical to walking patrol hot spots via the estimated DDD intervention parameters. Walking patrols are more widely implemented and studied, and have been shown to be successful in HSP (Ratcliffe et al., 2011). We suggest considerable future sites with sufficient statistical power examine the impact of emergency flashing lights while stationary more thoroughly as well.

These findings combined with the previous research, which demonstrates that hot spots policing does not seem to undermine public confidence or negatively impact police legitimacy (see Kochel & Weisburd, 2017), suggest that hot spots policing continues to be an effective crime prevention tool with minimal consequences. However, it will be important to continue to unravel the public's perception of hot spots policing as well as potential unintended consequences moving forward to ensure the police are operating in an efficient, effective, as well as socially and procedurally just manner when funneling intensive resources in high-risk communities.

In conclusion, HSP studies continue to build an evidence base, strengthen external validity, and showcase impact in divergent high-risk settings. The Las Vegas experiment adds to this both established and continually evolving body of research by demonstrating observed changes in criminal behavior across multiple outcomes, relying on a relatively untested patrol types (stationary with emergency lights), and highlighting tangible changes in crime in already heavily proactively policed chronic, violent crime areas. Pragmatically speaking, these findings specifically show that concerns for threshold effects are no reason for law enforcement officials to avoid HSP when attempting to proactively reduce violence and property offending in crime hot spots.

Appendix

Table 4 Maximum likelihood interrupted time series analysis—monthly crime counts in Las Vegas (1/2013–6/2019) with January 2018 intervention period (pooled chronic high-crime areas and rest of the city)

	Violent CHCA	Violent rest of city	Property CHCA	Property rest of city	Other CHCA	Other rest of city	Total CHCA	Total rest of city
	B (SE)	B (SE)	B (SE)	B (SE)	B (SE)	B (SE)	B (SE)	B (SE)
Measure								
Post-January 2018	.017 (.025)	.040* (.017)	.033 (.027)	.013 (.019)	−.078* (.024)	−071* (.019)	−.007 (.019)	−.004 (.014)
Model fit statistics								
Log Pseudo likelihood	−509.8	−877.5	−585.4	−1207.8	−553.5	−992.5	−868.4	−2089.5
Wald X^2	206.4	732.5	71.7	98.3	155.8	661.1	195.5	656.2

$^{+}p<.10$; $^{*}p<.05$; all analyses include monthly dummy variables (January = reference category) and sequential trend measure

References

Aickin, M. (1999). Other methods for adjustment of multiple testing exists. *BMJ, 318*, 127.

Aickin, M., & Gensler, H. (1996). Adjusting for multiple testing when reporting research results: The Bonferroni vs Holm methods. *American Journal of Public Health, 86*, 726–728.

Anderson, M. A., & Malleson, N. (2011). Testing the stability of crime patterns: Implications for theory and policy. *Journal of Research in Crime and Delinquency, 48*, 58–82.

Braga, A. A. (2001). The effects of hot spots policing on crime. *The Annals of American Political and Social Science, 578*, 104–125.

Braga, A. A., Hureau, D. M., & Papachristos, A. V. (2011). An ex post facto evaluation framework for place-based police interventions. *Evaluation Review, 35*(6), 592–626.

Braga, A. A., Papachristos, A. V., & Hureau, D. M. (2014). The effects of hot spots policing on crime: An updated systematic review and meta-analysis. *Justice Quarterly, 31*, 633–663.

Braga, A.A., Turchan, B., Papachristos, A.V. & Hureau, D.M. (2019). Hot spots policing of small geographic areas effects on crime. Campbell Systematic Reviews, https://www.campbellcollaboration.org/better-evidence/effects-of-hot-spots-policing-on-crime.html

Braga, A.A. & Weisburd, D.L. (2020). Does hot spots policing having meaningful impacts on crime? Findings from an alternative approach to estimating effect sizes from place-based program evaluations. Journal of Quantitative Criminology, 1–22.

Chainey, S. P., Serrano-Berthet, R., & Veneri, F. (2021). The impact of a hot spot policing program in Montevideo, Uruguay: An evaluation using a quasi-experimental difference-in-difference negative binomial approach. *Police Practice and Research, 22*, 1541–1556.

Clarke, R. V. (1980). Situational crime prevention theory and practice. *British Journal of Criminology, 20*, 136–147.

Collazos, D., García, E., Mejía, D., Ortega, D., & Tobón, S. (2020). Hot spots policing in a high-crime environment: An experimental evaluation in Medellín. *Journal of Experimental Criminology*. https://doi.org/10.1007/s11292-019-09390-1

Dunn, O. J. (1961). Multiple comparisons among means. *Journal of the American Statistical Association, 56*(293), 52–64.

Farrington, D. P., & Welsh, B. C. (2005). Randomized experiments in criminology: What have we learned in the last two decades? *Journal of Experimental Criminology, 1*, 9–38.

Feise, R. J. (2002). Do multiple outcome measures require p-value adjustment? BMC Medical Research Methodology, 2 (8).

Gelman, A., Fagan, J., & Kiss, A. (2007). An analysis of the New York City Police Department's "Stop-and-Frisk" Policy in the context of claims of racial bias. *Journal of the American Statistical Association, 102*(479), 813–823.

Golla, A., Muller, T., Wohlfarth, K., Patrick, J., Mattukat, K., & Mau, W. (2018). Home-based balance training using Wii Fit: A pilot randomized controlled trial with mobile older stroke survivors. *Pilot and Feasibility Studies, 4*, 143–153.

Greene, W. H. (1994). Accounting for excess zeroes and sample selection in Poisson and negative binomial regression models. Stern School of Business working paper no. 94–10 (March 1994).

Haberman, C. P., & Stiver, W. H. (2019). The Dayton foot patrol program: An evaluation of hot spots foot patrols in a central business district. *Police Quarterly, 22*(3), 247–277.

Holm, S. (1979). A simple sequentially rejective multiple test procedure. *Scandinavian Journal of Statistics, 6*(2), 65–70.

Johnson, J. P. (2019). Testing cruise lights on patrol: An illuminating experience. *Translational Criminology, Fall, 2019*, 20–21.

Kelling, G., Pare, A., Dickman, D., & Brown, C. (1974). *The Kansas City preventative patrol experiment: A technical report*. Police Foundation.

Kochel, T. R., & Weisburd, D. (2017). Assessing community consequences of implementing hot spots policing in residential areas: Findings from a randomized field trial. *Journal of Experimental Criminology, 13*, 143–170.

Koper, C. S. (1995). Just enough police presence: Reducing crime and disorderly behavior by optimizing patrol time in crime hot spots. *Justice Quarterly, 12*, 649–672.

Koper, C.S., Lum, C., Wu, X. & Hegarty, T. (2021). The long-term and system-level impacts of institutionalizing hot spot policing in a small city. Policing: A Journal of Policy and Practice, paaa096, https://doi.org/10.1093/police/paaa096

McBee, M. (2010). Modeling outcomes with floor or ceiling effects: An introduction to the Tobit Model. *Gift Child Quarterly, 54*, 314–320.

Mejía, D., Ortega, D., & Ortiz, K. (2015). Un análisis de la criminalidad urbana en Colombia. Technical report, CAF - Banco de Desarrollo de America Latina.

National Research Council (NRC). (2004). The effectiveness of police activity in reducing crime, disorder and fear. In W. Skogan & K. Frydl (Eds.), *Fairness and effectiveness in policing: The evidence* (pp. 217–251). The National Academies Press.

Nezu, A. M. & Nezu, C. M. (Eds.). (2008). Evidence-based outcome research: A practical guide to conducting randomized controlled trials for psychosocial interventions. Oxford University Press.

Novak, K. J., Fox, A. M., Carr, C. M., & Spade, D. A. (2016). The efficacy of foot patrol in violent places. *Journal of Experimental Criminology, 12*(3), 465–475.

Pierce, G., Spaar, S., & Briggs, L. R. (1988). *The character of police work: Strategic and tactical implications*. Center for Applied Social Research, Northeastern University.

Perneger, T. V. (1998). What's wrong with Bonferroni adjustments. *BMJ, 316*, 1236–1238.

Perneger, T. V. (1999). Adjusting for multiple testing in studies is less important than other concerns. *BMJ, 318*, 1288.

Piza, E. L., & O'Hara, B. A. (2014). Saturation foot-patrol in a high-violence area: A quasi-experimental evaluation. *Justice Quarterly, 31*(4), 693–718.

Peters, J., Langbein, J., & Roberts, G. (2018). Generalization in the tropic – development policy, randomized controlled trials, and external validity. *World Bank Research Observer, 33*, 34–64.

Ratcliffe, J. H., Taniguchi, T., Groff, E. R., & Wood, J. R. (2011). The Philadelphia foot patrol experiment: A randomized controlled trial of police patrol effectiveness in violent crime hotspots. *Criminology, 49*, 795–831.

Richards, S.H., Wunderlich, R.C., Dudek, C.L. & Brackett, R.Q. 1985. Improvements and new concepts for traffic control in work zones: Volume 4 – Speed control in work zones. Springfield, VA:

United States Federal highway Administration: Office of Safety and Traffic Operations Research and Development.
Rothman, K. J. (1990). No adjustments are needed for multiple comparisons. *Epidemiology, 1*(1), 4346.
Sherman, L., Gartin, P., & Buerger, M. (1989). Hot spots of predatory crime: Routine activities and the criminology of place. *Criminology, 27*, 27–56.
Sherman, L. W., & Rogan, D. P. (1995). Effects of gun seizures on gun violence: "Hot spots" patrol in Kansas City. *Justice Quarterly, 12*(4), 673–693.
Sherman, L. W., & Weisburd, D. L. (1995). General deterrent effects of police patrol in crime hot spots: A randomized controlled trial. *Justice Quarterly, 12*, 625–648.
Sorg, E. T., Wood, J. D., Groff, E. R., & Ratcliffe, J. H. (2014). Boundary adherence during place-based policing evaluations: A research note. *Journal of Research in Crime and Delinquency, 51*(3), 377–393.
Taylor, B. G., Koper, C. S., & Woods, D. (2011). A randomized controlled trial of different policing strategies at hot spots of violent crime. *Journal of Experimental Criminology, 7*, 149–181.
Taylor, R. B., & Ratcliffe, J. H. (2020). Was the pope to blame? Statistical powerlessness and the predictive policing of micro-scale randomized control trials. *Criminology & Public Policy, 19*, 965–995.
Telep, C. W., Mitchell, R. J., & Weisburd, D. (2014). How much time should the police spend in crime hot spots? Answers from a police agency directed randomized field trial in Sacramento, California. *Justice Quarterly, 31*, 905–933.
Townsley, M., Homel, R., & Chaseling, J. (2003). Infectious burglaries: A test of the near repeat hypothesis. *British Journal of Criminology, 43*, 615–633.
U.S. Census Bureau (2021). *Las Vegas, NV.* Retrieved from *Census Reporter Profile page for Las Vegas, NV* https://censusreporter.org/profiles/16000US3240000-las-vegas-nv/
Weisburd, D. (2015). The law of crime concentration and the criminology of place. *Criminology, 53*, 133–157.
Weisburd, D. L. & Anthony A. Braga. (2003). Hot spots policing. In Crime prevention: New approaches, eds. Helmut Kury and Joachim Obergfell-Fuchs. Mainz, Germany: Weisner Ring.
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), 283–322.
Weisburd, D., & Gill, C. (2014). Block randomized trials at places: Rethinking the limitations of small n experiments. *Journal of Quantitative Criminology, 30*(1), 97–112.
Weisburd, D., Groff, E. R., Jones, G., Cave, B., Amendola, K. L., Yang, S.-M., & Emison, R. F. (2015). The Dallas patrol management experiment: Can AVL technologies be used to harness unallocated patrol time for crime prevention? *Journal of Experimental Criminology, 11*(3), 367–391.
Weisburd, D., Groff, E. R., & Yang, S.-M. (2012). *The criminology of place: Street segments and our understanding of the crime problem*. Oxford University Press.
Weisburd, D. L., & Green, L. (1995). Policing drug hot spots: The Jersey City drug market analysis experiment. *Justice Quarterly, 12*, 711–735.
Weisburd, D. & Majmundar, M. K. (Eds.). (2018). Proactive policing: Effects on crime and communities. The National Academies Press. https://doi.org/10.17226/24928
Weisburd, D., Morris, N., & Groff, E. (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.
Wilcox, P., & Eck, J. E. (2011). Criminology of the unpopular. Implications for policy aimed at payday lending facilities. *Criminology & Public Policy, 10*, 473–482.