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THE EFFECTS OF BODY-WORN CAMERAS ON POLICE ACTIVITY AND POLICE-CITIZEN ENCOUNTERS: A RANDOMIZED CONTROLLED TRIAL*

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Many have suggested that placing body-worn cameras (BWCs) on police officers improves the civility of police-citizen encounters and enhances citizen perceptions of police transparency and legitimacy. In response, many police departments have adopted this technology to address public concerns over the quality of policing in their communities. The existing program evaluation evidence on the intended and unintended consequences of outfitting police officers with BWCs is still developing, however. This study reports the findings of a randomized controlled trial involving more than 400 police officers in Las Vegas, Nevada. We find that officers equipped with

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body-worn cameras generated fewer complaints and use of force reports relative to officers without cameras. BWC officers also made more arrests and issued more citations than their non-BWC counterparts. The findings of this randomized controlled trial raise the possibility that planning for the placement of BWCs on officers should consider the competing effects of improvement in civilian perceptions of police generated by reductions in complaints and use of force incidents and of public concerns about increased enforcement activity.

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INTRODUCTION

Recent deadly police officer-involved shooting events in Baltimore, Charlotte, Chicago, Ferguson, and elsewhere in the United States have exposed very concerning rifts in the relationships between the police and the communities they protect and serve.[1] Placing body-worn cameras (BWCs) on police officers has been suggested as one potentially powerful response to

[1] *See, e.g.*, Larry Buchanan et al., *What Happened in Ferguson?*, N.Y. TIMES (Aug. 10, 2015), https://www.nytimes.com/interactive/2014/08/13/us/ferguson-missouri-town-under-siege-after-police-shooting.html (describing conflict between the community of Ferguson, Missouri and its police force over the controversial death of Michael Brown).

the current police legitimacy crisis in many U.S. cities.[2] Advocates suggest there are many benefits associated with placing BWCs on police officers.[3] BWCs are suggested to increase transparency and citizen views of police legitimacy, improve police and citizen behaviors during encounters, enhance evidence collected for the resolution of complaints against the police and the arrest and prosecution of offenders, and provide improved opportunities for police training.[4] The Obama Administration proposed that Congress provide the U.S. Department of Justice with $75 million to fund the purchase of and technical assistance for BWCs.[5] In 2013, the U.S. Bureau of Justice Statistics estimated that over one-quarter of the approximately 18,000 U.S. police departments had adopted the BWC technology, and the number of police departments with BWC programs has undoubtedly increased since then.[6]

Like other police technologies, the rapid adoption of BWCs has occurred within a low-information environment. As Professor Cynthia Lum cautions, the rapid adoption of police technologies in the absence of rigorous empirical evidence about their impact can lead to unanticipated and unintended consequences that may harm both police and public interests.[7] Social scientists are only beginning to develop scientific knowledge about the effects, expected and unexpected, of the BWC technology.[8] For instance, the evidence on the effects of BWCs on the civility of police-citizen encounters is still somewhat unclear. Several studies find that BWCs reduce complaints against police officers and officer use of force reports,[9] while

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[2] *See* JAY STANLEY, ACLU, POLICE BODY-MOUNTED CAMERAS: WITH RIGHT POLICIES IN PLACE, A WIN FOR ALL 1–2 (2015), *available at* https://www.aclu.org/other/police-body-mounted-cameras-right-policies-place-win-all.

[3] *Id.* at 2.

[4] MICHAEL D. WHITE, OFF. OF JUST. PROGRAMS, U.S. DEP'T OF JUST., POLICE OFFICER BODY-WORN CAMERAS: ASSESSING THE EVIDENCE 6–7 (2014).

[5] FACT SHEET: STRENGTHENING COMMUNITY POLICING, WHITE HOUSE (Dec. 1, 2014), http://www.whitehouse.gov/the-press-office/2014/12/01/fact-sheet-strengthening-community-policing.

[6] U.S. BUREAU OF JUST. STAT., U.S. DEP'T OF JUSTICE, LAW ENFORCEMENT MANAGEMENT AND ADMINISTRATIVE STATISTICS, 2013 200 (2015).

[7] Cynthia Lum, *Body Worn Cameras—Rapid Adoption in a Low Information Environment?*, TRANSLATIONAL CRIMINOLOGY 6, 6–7 (2015).

[8] CYNTHIA LUM ET AL., EXISTING AND ONGOING BODY WORN CAMERA RESEARCH: KNOWLEDGE GAPS AND OPPORTUNITIES 3 (2015).

[9] *See* Barak Ariel et al., *The Effect of Police Body-Worn Cameras on Use of Force and Citizens' Complaints Against the Police: A Randomized Controlled Trial*, 31 J. QUANTITATIVE CRIMINOLOGY 509, 524–25 (2015); E.C. Hedberg et al., *Body-Worn Cameras and Citizen Interactions with Police Officers: Estimating Plausible Effects Given Varying Compliance Levels*, 34 JUST. Q. 627, 642 (2016); Wesley G. Jennings et al., *Evaluating the Impact of Police Officer Body-Worn Cameras (BWCs) on Response-to-Resistance and Serious External Complaints: Evidence from the Orlando Police Department (OPD) Experience Utilizing a*

other studies find no statistically significant reductions in complaints against BWC officers[10] and a concerning increase in assaults on officers with BWCs.[11]

There is also growing evidence that suggests BWCs may result in increased enforcement activity by police officers. Controlled evaluations reveal that BWC officers make more arrests and citations relative to their non-BWC counterparts.[12] These unexpected outcomes could undermine improvements in police-citizen encounters associated with adoption of the technology in urban environments. To some observers, too many police departments engage in excessive surveillance and enforcement practices in urban neighborhoods.[13]

We examine the effects of BWCs on police activity and police-citizen encounters in Las Vegas, Nevada. A randomized controlled trial design involving more than 400 Las Vegas Metropolitan Police Department (LVMPD) patrol officers tested the impacts of randomly-allocated BWCs on complaints, use of force reports, and officer activity outcomes for treatment officers relative to control officers.[14] Part I reviews the existing literature on the impact of BWCs on the civility of police-citizen encounters and police officer work activities. Parts II and III describe the Las Vegas randomized experiment and the statistical models used to analyze the outcome data. Part IV reveals that our experimental analyses found statistically significant reductions in complaints and officer use of force reports for treatment officers relative to control officers. However, we also find statistically significant

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Randomized Controlled Experiment, 43 J. CRIM. JUST. 480, 485 (2015).

[10] LYNNE GROSSMITH ET AL., POLICE, CAMERA, EVIDENCE: LONDON'S CLUSTER RANDOMISED CONTROLLED TRIAL OF BODY WORN VIDEO 15 (2015).

[11] Barak Ariel et al., *Wearing Body-Cameras Increases Assaults Against Officers and Do Not Reduce Police-Use of Force: Results from a Global Multisite Experiment*, 13 EUR. J. CRIMINOLOGY 744, 750 (2016).

[12] CHARLES KATZ ET AL., EVALUATING THE IMPACT OF OFFICER WORN BODY CAMERAS IN THE PHOENIX POLICE DEPARTMENT 31 (2014); CATHERINE OWENS ET AL., THE ESSEX BWV TRIAL: THE IMPACT OF BWV ON CRIMINAL JUSTICE OUTCOMES OF DOMESTIC ABUSE INCIDENTS 14–15 (2014); Justin Ready & Jacob Young, *The Impact of On-Officer Video Cameras on Police-Citizen Contacts: Findings from a Controlled Experiment in Mesa, AZ*, 11 J. EXPERIMENTAL CRIMINOLOGY 445, 452 (2015).

[13] CHARLES EPP ET AL., PULLED OVER: HOW POLICE STOPS DEFINE RACE AND CITIZENSHIP 139–45 (2014); VICTOR M. RIOS, PUNISHED: POLICING THE LIVES OF BLACK AND LATINO BOYS 43–73 (2011); Kathryne Young & Joan Petersilia, *Keeping Track: Surveillance, Control, and the Expansion of the Carceral State*, 129 HARV. L. REV. 1318, 1322 (2016).

[14] As will be detailed later in this article, participating LVMPD officers were randomly allocated to a treatment group that wore the BWCs during the experiment or to a control group that did not wear the BWCs during the experiment. The control officers served as the counterfactual condition to evaluate the effects of BWCs on police officer work activities and the civility of police-citizen encounters.

increases in arrests and citations made by treatment officers relative to control officers. We discuss implications of these findings for BWC policy and practices in the conclusion.

I. LITERATURE REVIEW

A. INFLUENCE ON THE CIVILITY OF POLICE-CITIZEN ENCOUNTERS

Two theoretical perspectives, deterrence and self-awareness, are commonly applied to support the position that placing BWCs on officers will improve the civility of police-citizen interactions by deterring undesirable behaviors (i.e., not wanting to be recorded on video doing something inappropriate or illegal) and stimulating desirable behaviors (i.e., remembering to treat others with respect).[15] Deterrence theory suggests that crimes can be prevented when the costs of committing the crime are perceived by the offender to outweigh the benefits.[16] Much of the literature evaluating deterrence focuses on the effect of changing certainty, swiftness, and severity of punishment associated with certain acts on the prevalence of those crimes.[17] The available research suggests that deterrent effects are ultimately determined by offender perceptions of sanction risk and certainty.[18]

BWCs have been suggested as a deterrent to noncompliance with the rules of proper behavioral conduct in police-citizen encounters.[19] In his discussion of the influence of cameras on behavior, Professor Nick Tilley argued that deterrence is one prominent prevention mechanism triggered by the technology: the presence of a camera "reduces . . . [noncompliance] by deterring potential offenders who will not wish to risk apprehension and conviction by the evidence captured on videotape or observed by an operator on a screen on which their behavior is shown."[20] For officers and citizens

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[15] Ariel et al., *supra* note 9, at 516.

[16] FRANKLIN ZIMRING & GORDON HAWKINS, DETERRENCE: THE LEGAL THREAT IN CRIME CONTROL 3 (1973).

[17] *See, e.g.*, PANEL ON RES. ON DETERRENT AND INCAPACITATIVE EFFECTS, DETERRENCE AND INCAPACITATION: ESTIMATING THE EFFECTS OF CRIMINAL SANCTIONS ON CRIME RATES 96–97 (Alfred Blumstein et al. eds., 1978); Philip Cook, *Research in Criminal Deterrence: Laying the Groundwork for the Second Decade*, 2 CRIME & JUST. 211, 230–33 (1980); Raymond Paternoster, *The Deterrent Effect of the Perceived Certainty and Severity of Punishment: A Review of the Evidence and Issues*, 4 JUST. Q. 173, 173–75 (1987).

[18] Daniel Nagin, *Deterrence in the Twenty-First Century*, 42 CRIME & JUST. 199, 201 (2013).

[19] *See* Ariel et al., *supra* note 9, at 526.

[20] NICK TILLEY, UNDERSTANDING CAR PARKS, CRIME, AND CCTV: EVALUATION LESSONS FROM SAFER CITIES 3 (1993).

alike, the presence of a camera during encounters increases the likelihood that any misconduct and illegal behaviors will be captured on video and, as such, generates a deterrent effect by increasing their perceptions of the likelihood of apprehension and celerity of punishment.[21]

Self-awareness theory states that when we focus our attention on ourselves, we evaluate and compare our current behavior to our internal standards and values.[22] This theory further suggests that when human beings are under observation, they modify their behavior, exhibit more socially acceptable behavior, adhere to social norms, and cooperate more fully with the rules.[23] People are more likely to align their behavior with personal standards when made self-aware and believe that they will be negatively affected if they do not live up to these standards.[24] Various environmental cues and situations induce awareness of the self, such as mirrors, an audience, or being videotaped or recorded.[25] A well-developed line of research suggests that people do alter their behavior once they know that they are being observed.[26]

The presence of BWCs during police-citizen encounters is suggested to stimulate self-awareness by making these individuals conscious that they are being watched and their actions are being recorded.[27] As a result, police and citizens alike become self-aware and compare their behavior in the encounters with objective standards, which are socially desirable behaviors.[28] If encounter participants notice a discrepancy between their behavior and what is socially desirable, then they will alter their behavior.[29] As will be discussed further below, these socially desirable behaviors include

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[21] *See generally* Barak Ariel, *Increasing Cooperation With the Police Using Body Worn Cameras,* 19 POLICE Q. 331 (2016).

[22] T. SHELLEY DUVAL & ROBERT WICKLUND, A THEORY OF OBJECTIVE SELF-AWARENESS 10 (1st ed. 1972).

[23] *Id*. at 4.

[24] *Id.* at 10.

[25] *Id.* at 42–51, 100, 101; Paul Silvia & T. Shelley Duval, *Objective Self-Awareness Theory: Recent Progress and Enduring Problems*, 5 PERSONALITY & SOC. PSYCHOL. REV. 230, 234–35 (2001).

[26] *See, e.g.*, Kristen Munger & Shelby Harris, *Effects of an Observer on Hand Washing in a Public Restroom*, 69 PERCEPTUAL & MOTOR SKILLS 733, 734 (1989); Delroy Paulhus, *Two-Component Models of Socially Desirable Responding*, 46 J. PERSONALITY & SOC. PSYCH. 598, 606 (1984).

[27] Tony Farrar et al., Self-Awareness to Being Watched and Socially Desirable Behavior: A Field Experiment on the Effect of Body-Worn Cameras and Police Use of Force 9 (2013) (unpublished manuscript), *available at* https://www.policefoundation.org/wp-content/uploads/2015/06/The-Effect-of-Body-Worn-Cameras-on-Police-Use-of-Force.pdf.

[28] *Id.* at 2–3.

[29] *Id.* at 3.

procedurally just treatment of citizens by police officers.[30] In summary, there is solid theoretical support for the use of BWCs as a prevention mechanism to influence the behaviors of those who are under observation.[31] BWCs are suggested to have both an intrinsic effect (self-awareness theory) and an extrinsic effect (deterrence theory) on those being watched such that police and citizens will exhibit socially desirable behavior in their interactions.[32]

While it remains unclear whether deterrence, self-awareness, or both are generating the observed effects, several recently completed randomized controlled trials and quasi-experiments suggest that BWCs improve the civility of police-citizen civilian encounters by reducing complaints against officers and officer use of force (both excessive and non-excessive). In the Rialto, California randomized experiment, officers wearing BWCs during treatment shifts generated a 90% reduction in complaints and a 50% reduction in use of force reports relative to officers not wearing cameras during comparison shifts.[33] In Arizona, the Mesa Police Department's quasi-experimental evaluation of BWCs revealed a 40% reduction in citizen complaints against treatment officers for misconduct during the study period, and a 75% decline in use of force complaints.[34] In the Orlando, Florida randomized experiment, BWC officers had a significantly lower prevalence of response-to-resistance incidents (involving electronic control devices, chemical agents, impact weapons, and other non-lethal implements) and lower prevalence of serious external complaints relative to control officers without BWCs.[35] A quasi-experimental evaluation in Phoenix reported a 62% reduction in complaints lodged against treatment officers relative to control officers.[36] In the Mesa, Phoenix, and Rialto studies, many complaints were resolved quickly due to the accessibility of video evidence.[37]

While there is some promising evidence that BWCs de-escalate confrontation and aggression in police-citizen encounters, not all evaluations support this position. A randomized experimental design was used to evaluate the effects of BWCs on complaints against officers in the London

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[30] *Id.* at 9.

[31] *Id.* at 2–3.

[32] *Id.* at 9–10; LUM ET AL., *supra* note 8, at 19; WHITE, *supra* note 4, at 20–23.

[33] Ariel et al., *supra* note 9, at 523–524.

[34] MESA POLICE DEP'T, ON-OFFICER BODY CAMERA SYSTEM: PROGRAM EVALUATION AND RECOMMENDATIONS 11 (2013).

[35] Jennings et al., *supra* note 9, at 485.

[36] Hedberg et al., *supra* note 9, at 644.

[37] KATZ ET AL., *supra* note 12, at 41; Ian Lovett, *In California, a Champion for Police Cameras*, N.Y. TIMES (Aug. 21, 2013), http://www.nytimes.com/2013/08/22/us/in-california-a-champion-for-police-cameras.html?mcubz=3.

Metropolitan Police Service (United Kingdom).[38] The study did not reveal any statistically significant differences in overall complaints made against officers with BWCs relative to officers not wearing BWCs.[39] There were also no statistically significant differences in self-reported assaults on officers or injuries for BWC officers relative to control officers.[40] A multi-site randomized experiment involving 2,122 officers in eight police departments reported no overall reduction in officer use of force and an increase in assaults on officers wearing BWCs during treatment shifts relative to officers not wearing BWCs during control shifts.[41] In a re-analysis of the multisite randomized experiment data, Professor Barak Ariel and his colleagues show that use of force by treatment officers decreased by 37% in three sites with high compliance to a BWC policy that required officers to notify citizens that they were being recorded at the beginning of the encounter.[42] Ariel and his colleagues also reported a 71% increase in officer use of force in sites with low compliance to the BWC policy.[43] Based on these findings, the authors hypothesized that unchecked BWC discretion may increase use of force as camera activation during situations with escalating aggression may further increase aggression during these volatile situations.[44] The authors also suggested that verbal notification of video recording by officers at the commencement of encounters may be helpful in deterring aggressive behavior and stimulating civil behavior before police-citizen interactions escalate in a negative direction.[45]

B. INFLUENCE ON POLICE OFFICER WORK ACTIVITIES

A very small number of studies have examined the effects of BWCs on police officer work activities, such as their willingness to be proactive, problem solve, and their discretion in making arrests and citations in discretionary incidents.[46] Survey research suggests that police officers generally view the technology as facilitating the arrest and prosecution of criminal offenders by improving the quality of evidence via the creation of a

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[38] GROSSMITH ET AL., *supra* note 10, at 29–33.

[39] *Id*. at 15.

[40] *Id*. at 25–26.

[41] Ariel et al., *supra* note 11, at 750.

[42] Barak Ariel et al., *Increases in Police Use of Force in the Presence of Body-Worn Cameras are Driven by Officer Discretion: A Protocol-Based Subgroup Analysis of Ten Randomized Experiments*, 12 EUR. J. CRIMINOLOGY 453, 459 (2016).

[43] *Id.*

[44] *Id.* at 461.

[45] *Id.*

[46] *See, e.g.*, LUM ET AL., *supra* note 8, at 19; WHITE, *supra* note 4, at 13.

permanent record of the events that transpired.[47] In agencies considering the adoption of BWCs, police officers have been noted to express concern over how camera footage will be used to monitor officer performance.[48] Indeed, officers may fear being reprimanded for not issuing a citation or making an arrest when a video clearly shows that a citizen has violated the law.[49] Both orientations towards the placement of BWCs on officers—that is, the belief that offenders are more likely to be held accountable for their transgressions via the availability of video evidence and the a priori knowledge that supervisors may scrutinize officer discretion in resolving incidents—seem likely to influence officer work activities.[50]

Two controlled studies suggest that officers do increase their law enforcement activities when outfitted with BWCs. The Phoenix, Arizona quasi-experimental evaluation concluded that BWCs increased officer productivity when measured by the number of arrests.[51] The evaluators reported that the number of arrests increased by about 17% among officers in the BWC treatment group compared to 9% among officers in the comparison group.[52] In the Essex, United Kingdom randomized controlled trial, Owens and her colleagues found that incidents attended by BWC officers were more likely to result in criminal charges as compared to incidents attended by control officers.[53]

Researchers Ready and Young used a quasi-experimental analysis of field contact reports to examine whether BWCs influenced Mesa, Arizona Police Department officer behavior during police-citizen encounters over a ten-month period.[54] The analysis suggested that BWC officers were less likely to perform stop-and-frisks and make arrests, but were more likely to give citations and initiate encounters.[55] Ready and Young suggested that

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[47] MARTIN GOODALL, GUIDANCE FOR THE POLICE USE OF BODY-WORN VIDEO DEVICES 87 (2007); ODS CONSULTING, BODY WORN VIDEO PROJECTS IN PAISLEY AND ABERDEEN, SELF-EVALUATION 24–28 (2011).

[48] POLICE EXECUTIVE RES. F., IMPLEMENTING A BODY-WORN CAMERA PROGRAM: RECOMMENDATIONS AND LESSONS LEARNED 24–28 (2014).

[49] Ready & Young, *supra* note 12, at 454.

[50] BWC advocates suggest that the video evidence will facilitate the arrest and prosecution of offenders, as it offers a real-time, permanent record of the events that transpired. *See, e.g.*, WHITE, *supra* note 4, at 24. In police departments considering the adoption of BWC, some officers expressed concerns that the cameras were a signal that their supervisors and managers did not trust them and that the technology would be used to track and scrutinize their every move while on duty. *See, e.g.*, POLICE EXECUTIVE RES. F., *supra* note 48, at 24–25.

[51] KATZ ET AL., *supra* note 12, at 31.

[52] *Id.*

[53] OWENS ET AL., *supra* note 12, at 14–15.

[54] Ready & Young, *supra* note 12, at 448–49.

[55] *Id.* at 454.

Mesa police officers were more proactive with the BWC technology without increasing their use of invasive strategies that may threaten the legitimacy of the organization.[56] However, the authors did not assess how initiating additional encounters with citizens and issuing more citations might impact police relationships with the communities they serve.[57]

II. RANDOMIZED CONTROLLED TRIAL DESIGN AND IMPLEMENTATION

A. RESEARCH SETTING

The LVMPD provides policing services to some 1.5 million residents of the Las Vegas metropolitan area.[58] In fiscal year 2014–2015, the LVMPD had roughly 2,600 sworn police officers with nearly 1,400 officers assigned to the patrol division.[59] At the time of the study, the patrol division was divided geographically into eight "area commands."[60] The area commands, each headed by a captain, have primary responsibility for preventive patrol, responding to calls for service, and other proactive activities.[61] In 2014, the Las Vegas metropolitan area had a total index crime rate of 3,839.5 and a violent index crime rate (or crimes against persons rate) of 532.0 per 100,000 residents.[62] LVMPD officers responded to 1,139,777 emergency 911 citizen calls for service in 2014.[63]

LVMPD began pilot testing body-worn cameras with a small group of officers in 2011, around the time that the agency was under intense public criticism and scrutiny for its use of force policies that ultimately resulted in a collaborative reform process with the U.S. Department of Justice, Office of Community Oriented Policing Services.[64] The pilot testing period served as an opportunity for the agency to experiment with different BWC vendors, see how officers responded to the technology, and draft the Department's initial body-worn camera policy.[65] By 2013, LVMPD selected Taser International

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[56] *Id.*

[57] *Id.* at 454–55.

[58] The basic statistical information presented here was gleaned from the LVMPD 2014 annual report. LAS VEGAS METRO. POLICE DEP'T, 2014 ANNUAL REPORT, https://www.lvmpd.com/en-us/Documents/2014AnnualReport.pdf (last visited Feb. 7, 2018).

[59] *Id.* at 13.

[60] *Id.* at 7, 11.

[61] *Id.* at 7.

[62] *Id.* at 9.

[63] *Id.* at 10.

[64] JAMES K. STEWART ET AL., COLLABORATIVE REFORM PROCESS: A REVIEW OF OFFICER-INVOLVED SHOOTINGS IN THE LAS VEGAS METROPOLITAN POLICE DEPARTMENT 6–8 (2012).

[65] *Id.* The American Civil Liberties Union endorsed the LVMPD policy and suggested it was balanced in terms of transparency and privacy concerns. *See* Colton Lochhead, *ACLU*

and its Axon Flex as the vendor and camera to be worn by officers, and an official body-worn camera policy was developed.[66] A modest implementation of body-worn cameras on 200 officers was planned to test the impacts of the technology on a range of outcome measures and to guide a larger deployment of body-worn cameras on patrol officers.[67] In 2014, docking stations that recharged body-worn camera batteries and uploaded acquired videos to cloud memory storage were installed in four area commands.[68]

The LVMPD's experience with body-worn cameras and willingness to implement and evaluate them set the stage for a rigorous program evaluation. With support from the U.S. Department of Justice, National Institute of Justice, the LVMPD partnered with an external research team to develop and execute a randomized controlled trial.[69] The research team, Sousa and colleagues, documented the implementation of the LVMPD body-worn camera experiment and, despite some operational challenges, concluded that the intervention was implemented with integrity as body-worn camera officers generally complied with the policy.[70] However, one implementation challenge involved the recruitment of officers into the randomized controlled trial. Due to the provisions of the police union contract in place at the time of the experiment, the LVMPD could not mandate its officers to wear the body cameras.[71] As such, participants in the randomized controlled trials had to be volunteers who were willing to wear the cameras for at least one year.[72] The implications of this design challenge are explored further below.

B. RESEARCH DESIGN

Randomized experimental designs allow researchers to assume that the only systematic difference between the control and treatment groups is the

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Praises Las Vegas Police Body Camera Policy, LAS VEGAS REV.-J. (2015), https://www.reviewjournal.com/local/local-nevada/aclu-praises-las-vegas-police-body-camera-policy/ (last visited Jan. 26, 2018). The LVMPD BWC policy requires that "whenever possible, safe and practical, officers should inform individuals that they are being recorded." LAS VEGAS METRO. POLICE DEP'T, 5/210.01 BODY WORN CAMERAS, *available at* http://ipicd.com/ceer/files/LVMPD%20BWC%20Policy.pdf (last visited Feb. 15, 2017).

[66] STEWART ET AL., *supra* note 64, at 6–8.

[67] *Id.*

[68] *Id.*

[69] William Sousa et al., *Research on Body Worn Cameras: Meeting the Challenges of Police Operations, Program Implementation, and Randomized Controlled Trial Designs*, 19 POLICE Q. 364, 370 (2016).

[70] *Id.*

[71] *Id.*

[72] *Id.*

presence of the intervention; this permits a clear assessment of causes and effects.[73] Randomized experiments are valued for their strong internal validity—that is, the extent to which a research design can eliminate competing explanations of an observed correlation.[74] The randomization procedure allows the assumption to be made that there are no systematic differences between officers in the treatment and control groups prior to the experiment. Since randomized experiments control for confounding factors by design, analyses of experimental data do not require extensive statistical modeling to ensure rival causal influences are identified and controlled.[75]

This randomized controlled trial tested the impact of body-worn cameras on citizen complaint reports, police use of force incidents, and police activity measures for treatment officers as compared to control officers over pre-intervention and intervention time periods. Treatment officers were requested to wear the body cameras for at least a twelve-month intervention period. N=416 volunteer patrol officers were identified and randomized to treatment and control groups beginning in February 2014 and continuing through September 2014. This extended time period was needed to recruit eligible patrol officers through informational sessions held in each of the area commands, randomize volunteer officers from each area command into treatment and control groups, equip the treatment officers, and train them on BWC operations and policy. In anticipation of higher levels of attrition in the treatment group, the randomization procedure was weighted so that 10% more officers would be allocated to wear body cameras. The randomization process resulted in the assignment of N=218 officers to the treatment group and N=198 officers to the control group.

The LVMPD provided the evaluation team with detailed information on the patrol officers who did participate (N=416) in the randomized controlled trial as well as the remaining patrol officers (N=955) who did not participate in the randomized controlled trial. This information included age, race, sex, rank, time on the job, current assignment, and their unique identification number. All officers in the randomized controlled trial were closely monitored over the course of the March 1, 2014–September 30, 2015 intervention period. The data on the officers not participating in the randomized controlled trial represented a "snapshot" of nonexperimental officers as of the start of the experiment on March 1, 2014.

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[73] WILLIAM SHADISH ET AL., EXPERIMENTAL AND QUASI-EXPERIMENTAL DESIGNS FOR GENERALIZED CAUSAL INFERENCE 13, 246–56 (2002).

[74] *Id.* at 54–55.

[75] David Weisburd, *Justifying the Use of Non-Experimental Methods and Disqualifying the Use of Randomized Controlled Trials*, 6 J. EXPERIMENTAL CRIMINOLOGY 209, 220–21 (2010).

Official data on complaint reports and police officer use of force reports were acquired from the LVMPD Professional Standards Division between March 1, 2011 and September 30, 2015. The evaluation team matched the unique officer identification numbers for randomized controlled trial participants and non-participants to officer identification numbers in the complaint and use of force report data. Complaints were investigated by the Internal Affairs Bureau and originate externally from citizens who filed reports and internally from LVMPD personnel. The complaint report data included the date and time of the alleged misconduct, the types of allegations made against the officers, the unique identification number of the officer(s) alleged to be involved in the misconduct, and disposition information.

According to LVMPD policy, officers are not required to submit reports on low-level use of force incidents (such as empty hand tactics not involving strikes, use of baton as an escort tool, handcuffs, other restraints, and minimum lateral vascular neck restraint), unless the subject is injured or complains of injury.[76] Police officers are required to submit reports on intermediate use of force incidents (such as use of electronic discharge devices, empty hand strikes, low-lethality shotguns, baton use with impact, and pepper spray) and deadly force incidents.[77] Police officer use of force data included the date and time of the incident, the unique identification number of the officer(s) involved in the incident, and the types of force used in the incident. It is important to note here that these data do not distinguish between excessive and non-excessive force applied by LVMPD officers.

C. ASSESSING EXPERIMENTAL GROUP BALANCE AND GENERALIZABILITY

Randomization provides a simple and convincing method for achieving comparability in the treatment and control groups.[78] If randomization is done correctly, the only systematic difference between treatment and control groups should be the presence or absence of the treatment.[79] To test the balance between the treatment and control groups on key officer variables, we used independent samples (*t* tests) and standardized mean differences, known as Cohen's *d*.[80] Table 1 presents basic descriptive information on officers participating in the experiment and the results of these tests; for binary variables, means are expressed as percentages. A positive *t* test

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[76] LVMPD Directive GO-021-12, *Use of Force*, effective June 22, 2012.

[77] *Id.*

[78] SHADISH ET AL., *supra* note 73, at 248.

[79] *Id.*

[80] JACOB COHEN, STATISTICAL POWER ANALYSIS FOR THE BEHAVIORAL SCIENCES 15–20 (1988).

indicates that the treatment group has a higher mean than the control group. Covariate imbalance would be exhibited by Cohen's |*d*| in excess of .20 and a |*t*| in excess of 1.96. The equality of variances was tested and confirmed for all variables. This reveals that the randomization created balanced treatment and control groups.

Table 1. Summary Characteristics

	Treatment v. Control Groups, N=416			Participants v. Non-Participants, N=1,371		
		Balance Diagnostics			Balance Diagnostics	
Officer Characteristics	Mean (SD)	*t*	*d*	Mean (SD)	*t*	*d*
Experimental Group	52.4%	--	--	30.3%	--	--
Male	91.6%	.12	.006	90.6%	.83	.023
White	72.4%	.72	.035	71.4%	.51	.014
Hispanic	13.2%	-.53	.026	14.1%	-.90	.024
Black	8.9%	-.82	.041	7.4%	1.40	.038
Asian/Other	5.5%	.41	.019	7.1%	-1.47	.039
Mean Age	36.77 (7.89)	-1.40	.068	36.40 (7.68)	1.17	.031
Mean Years on Job	9.15 (5.21)	-1.56	.076	8.91 (5.65)	1.07	.028
Patrol Officer II	76.4%	1.01	.049	79.7%	-1.99*	.054
Patrol Officer I	8.7%	-.30	.015	11.0%	-1.84	.049
Sergeant	14.9%	-.96	.047	9.3%	4.79**	.128
Yearly Complaints 2012-2014	.837 (1.137)	1.51	.078	.891 (1.51)	-1.21	.032
Yearly Use of Force 2012-2014	.643 (.877)	1.03	.050	.657 (.881)	-.91	.025
Bolden	13.5%	1.05	.051	13.1%	.24	.006
Convention Center	7.2%	.86	.042	11.7%	-3.45**	.093
Downtown	9.6%	-1.01	.049	11.3%	-1.30	.035
Enterprise	19.5%	-.85	.042	14.6%	3.39**	.091
Northeast	19.7%	-.49	.023	13.9%	4.16**	.111
Northwest	17.3%	.85	.042	11.1%	4.99**	.131
South Central	7.5%	1.02	.050	11.1%	-2.84**	.076
Southeast	5,7%	.28	.014	13.1%	-5.42**	.145

Note: Experimental Group in the Treatment v. Control Group columns designates officers in the treatment group.

Experimental Group in the Participant v. Non-Participant columns designates officers in the randomized controlled trial. Binary variable means are expressed as percentages.
* = p < .05
** = p < .01

We also tested whether there were any systematic differences between patrol officers who participated in the experiment (N=416) and patrol officers who did not participate in the experiment (N=955) using the same approach (Table 1). There were no statistically significant differences in sex, race, age, years on the job, mean yearly complaints, and mean yearly use of force reports noted between the patrol officers who volunteered to participate in the randomized controlled trial and those who did not. These data suggest that officers with higher numbers of complaints and use of force reports did not seem to avoid participating in the body-worn camera pilot program. Indeed, on most observable characteristics, the volunteer officers seemed no different than those officers who chose not to volunteer for the program.

When compared to their non-volunteer counterparts, volunteer officers were somewhat more likely to be sergeants and to be assigned to the Enterprise, Northeast, and Northwest area commands and somewhat less likely to be patrol officers and to be assigned to the Convention Center, Southeast, and South Central area commands.[81] These observed differences were largely driven by implementation decisions. During the pre-implementation recruitment period, LVMPD commanders highly encouraged sergeants to "lead by example" by volunteering for the body-worn camera program. These data suggest that many sergeants responded to this call. The LVMPD located the body-worn camera docking stations in four area commands: Bolden, Enterprise, Northeast, and Northwest. Participating officers were required to place their cameras in the docking stations at the end of the shift so that acquired videos could be uploaded to cloud memory storage. Patrol officers not assigned to an area command with a docking station could still participate through an alternative mechanism that LVMPD established for uploading videos. However, the lack of docking station infrastructure at those area commands limited the number of officers who volunteered from them.

The balanced treatment and control groups supports the internal validity of the design and suggests that the randomized controlled trial was well-positioned to isolate the impact of body-worn cameras on the study outcome measures. External validity, however, gauges the extent to which the

[81] While the *t*-tests revealed that the observed differences were statistically significant at the α=.05 level, the Cohen's *d* standardized mean difference metric suggested that these differences were small (*ES*<.20). *See* MARK W. LIPSEY, DESIGN SENSITIVITY: STATISTICAL POWER FOR EXPERIMENTAL RESEARCH 55 (1990).

findings of a study can be generalized to the population of interest.[82] A study can have very high internal validity but be relevant only to a very limited number of contexts or problems. Inferences about cause-effect relationships based on a specific scientific study are said to possess external validity if they may be generalized from the unique and idiosyncratic experimental settings, procedures, and participants to other populations and conditions.[83] The available data presented here suggest that the findings of this study can be generalized to other LVMPD officers with the caveat that there are some small differences in rank and command area.

D. OFFICER ATTRITION

Attrition represents a threat to the internal validity of randomized experiments as it introduces bias into the analysis of experimental data.[84] In general, attrition from the randomized controlled trial was low: only 10.1% (42 of 416) of the officers left their assignments during their twelve-month intervention periods. However, differential attrition was noted for the treatment officers (N=26, 11.9% of 218) when compared to the control officers (N=16, 8.1% of 198). In the treatment group, fourteen officers changed assignments from the patrol division and did not continue wearing body cameras, seven officers withdrew from the program but stayed in their current assignment, two officers retired, two officers resigned from the LVMPD, and one person took a medical leave for a surgical procedure. In the control group, thirteen officers changed assignments from the patrol division, two officers resigned from the LVMPD, and one officer retired.

To address the observed attrition issue, we used intention-to-treat (ITT) analyses based on the initial random assignment to treatment rather than analyses of the treatment as actually received. ITT analyses provide fair comparisons between treatment and control groups because it avoids the bias associated with the non-random loss of study participants.[85] As such, all N=218 treatment officers and N=198 control officers were included in our analyses.

E. CONTAMINATION OF CONTROL CONDITIONS

One possible threat to the internal validity of any randomized

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[82] SHADISH ET AL., *supra* note 73, at 83.

[83] *Id.*

[84] *Id.* at 323.

[85] Sally Hollis & Fiona Campbell, *What is Meant by Intention to Treat Analysis? Survey of Published Randomized Controlled Trials,* 319 BRIT. MED. J. 670, 670–71 (1999).

experiment is the diffusion of the treatment into the control group.[86] Put simply, contaminated control conditions undermine the counterfactual contrast between subjects that receive the treatment and subjects that do not receive the treatment.[87] The stable unit treatment value assumption (SUTVA) assumes that the effect of some intervention on a given individual is not related to the treatment assignments of other people (or observational units).[88] In the context of the LVMPD experiment, this could include effects of treatment officers responding to the same dispatched calls for service as control officers.

The well-known Rialto, California BWC randomized experiment experienced possible diffusion of treatment effects, but this was due to the randomization of BWCs by shift rather than by individual officer.[89] In the Rialto experiment, the same officers participated in treatment (BWC on during shift) and control conditions (no BWC during shift).[90] As such, it was possible that participating officers carried over the treatment effect into control shifts. While the evaluation did still find significant reductions in citizen complaints and use of force incidents during treatment shifts relative to control shifts, Ariel and colleagues also observed reductions in these outcome measures during the control shifts, which suggests possible contamination.[91]

The LVMPD randomized controlled trial attempted to minimize these kinds of contamination effects by using different officers in control and treatment groups. Because LVMPD normally operates with one-officer patrol units, interaction between officers—and thus the potential for contamination—is infrequent during a typical shift but does occur when one officer backs up another on particular calls. Ideally, our randomized controlled trial would have also separated treatment and control officers into different policing areas to minimize interactions further. Unfortunately, this was not possible due to our reliance on volunteer officers to comprise treatment and control groups. Our research design was not able to prevent contamination of control conditions, and, as such, our estimates of BWC impacts on outcomes measures are biased towards the null hypothesis of "no difference" between treatment and control groups.

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[86] THOMAS COOK & DONALD CAMPBELL, QUASI-EXPERIMENTATION: DESIGN AND ANALYSIS ISSUES FOR FIELD SETTINGS 54 (1979).

[87] *Id.*

[88] *See generally* Donald Rubin, *Randomization Analysis of Experimental Data: The Fisher Randomization Test*, 75 J. AM. STAT. ASS'N 591 (1980).

[89] Ariel et al., *supra* note 9, at 527.

[90] *Id.* at 519–20.

[91] *Id.* at 527.

We were, however, able to utilize data from LVMPD's computer-aided dispatch (CAD) system to monitor and assess the extent of possible contamination in the experiment. The LVMPD also provided the evaluation team with CAD data recording citizen calls for service and officer-initiated calls made between March 1, 2011 and September 30, 2015. In this study, the analyzed CAD data represented unique call events where duplicate citizen calls for service for the same event were removed. For instance, in 2014, LVMPD officers were dispatched to 1,139,777 emergency 911 calls for service that represented 485,419 unique call events.

The CAD data included the event date and time, call event type, and the officer(s) responding to the call event; these data also included basic disposition information that indicated whether the call event generated a crime incident report, whether responding officer(s) issued citation(s), and whether responding officer(s) made arrest(s). The evaluation team matched the unique officer identification numbers for officers in the randomized controlled trial to officer identification numbers in the CAD data. During the fifty-five-month observation period, the N=416 officers who participated in the randomized experiment made N=694,408 responses to dispatched call events and N=225,368 self-initiated call events. Figure 1 presents the mean monthly counts of responses to dispatched call events for treatment and control officers between March 1, 2011 and September 30, 2015. This figure suggests similar monthly dispatched call response activity levels for both groups of officers before and during the intervention period.

These data allowed us to determine which officers responded to each call during the intervention period. As such, we were able to estimate the percentage of calls for service that involved one or more treatment and control group officers. Figure 2 reveals very modest contamination between treatment and control officers occurred each month (from March 2014 to September 2015); contamination ranged from a low of 15.3% in March 2014 to a high of 20.9% in August 2014, with an average of 19.1% per month.

Figure 1.

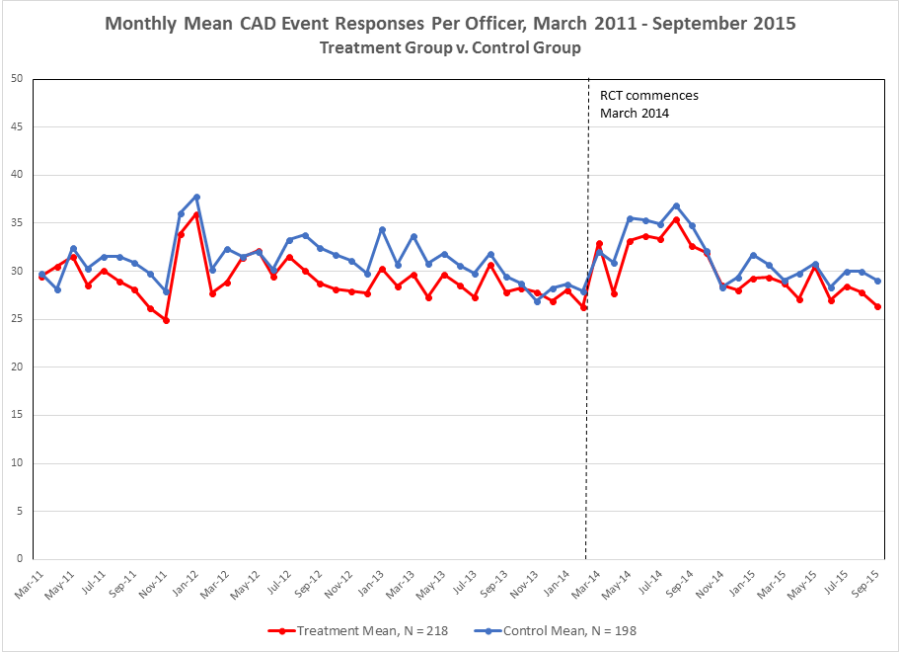
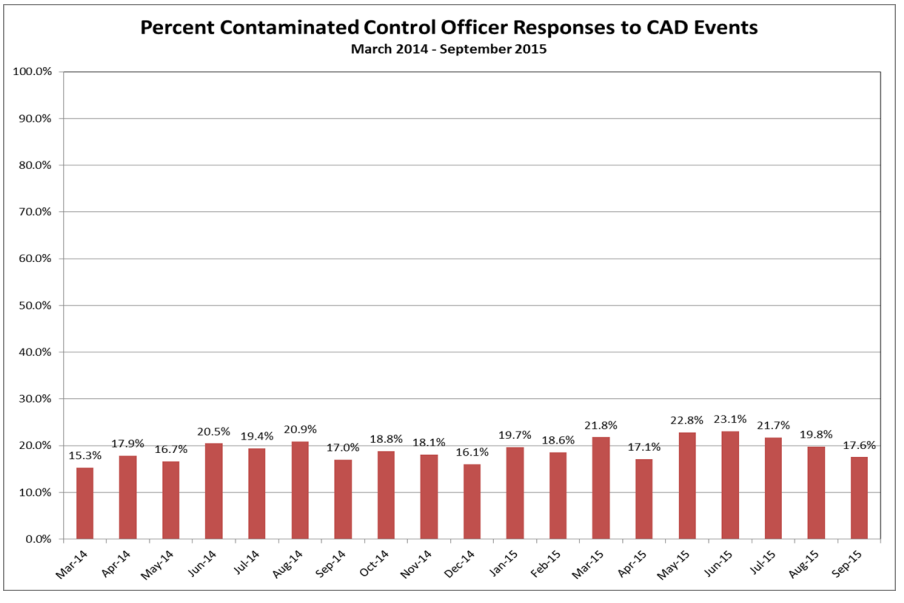


Figure 2.



III. ANALYTICAL APPROACH

A. OFFICER ACTIVITY OUTCOMES

The CAD data were used to develop key officer activity measures for the treatment and control officers in the randomized controlled trial during twelve-month pre-intervention and twelve-month intervention periods. Key officer activity measures included mean monthly responses to dispatched call events, mean monthly self-initiated call events, mean monthly call events that generated crime incident reports, mean monthly call events that resulted in citations, and mean monthly call events that resulted in arrests per month during the intervention and pre-intervention study periods. The impact of BWCs on treatment officer activity (N=218) relative to control officer activity (N=198) was estimated through the difference-in-differences (DID) estimator.[92] The DID estimates the difference in a treatment officer's post-intervention outcomes at time (*t*) compared with their pre-intervention outcomes, relative to the same difference for the control officers in the experiment.[93] The equation for our ordinary least squares panel regression model was:

$$(1) Y_{it} = \beta_0 + \beta_1 Group_i + \beta_2 Period_t + \beta_3 Group_i \times Period_t + u_i$$

In this model, the monthly mean number of dispatched call events per officer was our exemplar outcome measure (Y_{it}). The regressor $Group_i$ is a

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[92] *See, e.g.*, David Card & Alan Krueger, *Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania*, 84 AM. ECON. REV. 772, 778 (1994) (using the DID estimator to compare employment in the fast food sector in New Jersey and in Pennsylvania, in February 1992 and in November 1992, after New Jersey's minimum wage rose from $4.25 to $5.05 in April 1992).

[93] This randomized controlled trial had very modest statistical power to detect smaller program effects. With only N=426 officers, this randomized controlled trial had an estimated statistical power of .531 to detect a small standardized effect size of .20 for a two-tailed test with α =.05. The modest contamination of control conditions, described above, and the low prevalence of citizen complaints and officer use of force reports further undermined our ability to detect smaller program effects. The standardized mean difference (Cohen's *d*) between treatment (N = 218) and control groups (N = 198) during the intervention period revealed small effect sizes for the outcomes that were consisted with the results reported below. For instance, the BWC favored a treatment effect on complaints for treatment officers relative to control officers (*d* = -.074, SE = .048, *p* =.128) and on use of force reports for treatment officers relative to control officers (*d* = -.089, SE = .041, *p* =.069). However, the use of the DID estimator in a panel regression model with pre-intervention and intervention observations for each officer had the benefit of increasing the statistical power of the research design (416 officers * 2 observations each = 832 total observations) to detect these small effect sizes. *See, e.g.*, LIPSEY, *supra* note 81, at 91.

dummy variable identifying whether an individual officer (i) was in the treatment group (1) or not (0). The omitted group comprises control officers in the experiment. The regressor $Period_t$ is a dummy variable for whether monthly mean number of dispatched call events per officer was during the intervention period (1) or during the pre-intervention period (0). The coefficient β_3, conforming to the product of the group dummy with the period dummy, is the DID estimate of the effect of BWCs on the monthly count of officer-initiated call events. The XTREG command in Stata 14.1 was used to provide ordinary least squares estimates of the difference-in-differences described above. To ensure that the coefficient variances were robust to violations of the homoscedastic error assumption of linear regression models, robust standard errors clustered by officer were used.

B. COMPLAINT AND USE OF FORCE OUTCOMES

As noted in other BWC evaluations, citizen complaints against officers and use of force reports were rare events for LVMPD officers.[94] Indeed, during the one-year period preceding inclusion in the randomized controlled trial, 45.4% of treatment officers (99 of 218) and 52.0% of control officers (103 of 198) did not experience a single citizen complaint, and 68.8% of treatment officers (150 of 218) and 73.7% of control officers (146 of 198) did not generate a single use of force report. When these events occurred, a large majority of treatment officers and control officers generated only a single incident during the twelve months immediately preceding the experiment.[95] Given these rare event distributions, we collapsed the observed counts into binary outcomes (0 = no event, 1 = one or more events) for both citizen complaint events and use of force events outcomes during twelve-month pre-intervention and twelve-month intervention periods. DID of proportions *Z* tests were then used to estimate whether treatment officers were less likely to experience complaints and generate use of force reports relative to control officers between the pre-intervention and intervention periods.[96]

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[94] Ariel et al., *supra* note 9, at 509.

[95] For instance, for the sixty-eight treatment officers who experienced at least one use of force report during the pre-test time period: forty-eight had one incident (70.6%), fourteen had two incidents (20.6%), four had three incidents (5.9%), one had four incidents (1.5%) and one had five incidents (1.5%). For the fifty-two control officers who experienced at least one use of force report during the pre-test time period: thirty-seven had one incident (71.2%), ten had two incidents (19.2%), two had three incidents (3.8%), two had four incidents (3.8%), and one had seven incidents (1.9%).

[96] HUBERT BLALOCK, SOCIAL STATISTICS 235 (1979) (explaining the use of the difference-in-differences of proportions *Z* tests to evaluate policy interventions).

IV. RESULTS

A. OFFICER ACTIVITY OUTCOMES

Table 2 presents the DID estimator results of the panel regression models comparing pre-intervention and post-intervention monthly work activity levels for treatment officers to monthly work activity levels for control officers. It also presents the pre-intervention and intervention means and their percent differences for the various activity outcomes for the treatment and control officers. Holding group and period constant, the BWC intervention was not associated with any statistically significant changes in the monthly mean number of responses to dispatched call events, officer-initiated call events, and call events involving crime reports. However, controlling for group and period, the BWC intervention was associated with statistically significant increases in the monthly mean number of call events with citations issued and the monthly mean number of call events with arrests by the treatment officers relative to the control officers ($p<.01$ for both outcomes). Comparing monthly officer means over pre-intervention and intervention periods, treatment officers generated 5.2% more arrests and 6.8% more citations in their responses to call events relative to control officers. While these seem like very modest effects on event outcomes, these increases represent a noteworthy practical impact on the total number of police-citizen encounters with punitive outcomes. During the twelve-month treatment period, the officers with body-worn cameras generated an additional 471 call events involving crime incidents reports with arrests and an additional 1,125 call events with citations issued relative to their control officer counterparts.

Table 2. Impact of Body-Worn Cameras on Monthly Mean Officer Activity Outcomes

	Dispatched Calls	Officer-Initiated	Crime Incidents	Citations	Arrests
Impact (DID estimator)	.021 (.025)	.006 (.008)	-.002 (.009)	.685 (.036)**	.352 (.019)**
Treatment Officers					
Pre-Intervention monthly mean	29.85	9.44	11.00	10.27	6.87
Intervention monthly mean	30.24	9.56	11.13	11.08	7.30
Percent mean difference	+1.3%	+1.3%	+1.2%	+7.9%	+6.3%

Control Officers					
Pre-Intervention Monthly Mean	30.61	9.68	11.28	10.53	7.04
Intervention Monthly Mean	30.98	9.80	11.41	10.65	7.12
Percent mean difference	+1.2%	+1.2%	+1.2%	+1.1%	+1.1%
Percent Mean Difference-in-differences	+0.1%	+0.1%	0.0%	+6.8%	+5.2%
N=832 observations (416 officers * 2 observations)					

Note: Group and period covariates were included in the ordinary least squares panel regression models but not shown here. Robust standard errors clustered by officer are in parentheses. There were N=218 treatment officers and N=198 control officers included in this analysis.
* = $p < .05$
** = $p < .01$

B. CITIZEN COMPLAINTS AND USE OF FORCE OUTCOMES

Between the pre-intervention and intervention periods, the percentage of treatment officers that generated at least one complaint decreased by 16.5% from 54.6% to 38.1% (Table 3). By comparison, between the pre-intervention and intervention periods, the percentage of control officers that generated at least one complaint decreased by only 2.5% from 48.0% to 45.5%. The absolute differences in the share of officers with at least one complaint between the treatment and control groups over the pre-intervention and intervention periods represented a 14.0% reduction in favor of the treatment group ($Z = 2.035$, $p < .05$). The proportional difference between the two groups over time represented a larger 30.2% reduction in the percentage of treatment officers relative to control officers who generated at least one citizen complaint.

Similar significant reductions were noted in the likelihood that a treatment officer generated at least one use of force report during the intervention period. Between the pre-intervention and intervention periods, the percentage of treatment officers that generated at least one use of force report decreased by 11.5% from 31.2% to 19.7% (Table 4). By comparison, between the pre-intervention and intervention period, the percentage of control officers that generated at least one use of force report increased by 1.0% from 26.3% to 27.3%. The absolute differences in the share of officers with at least one use of force report between the treatment and control groups over the pre-intervention and intervention periods represented a 12.5% reduction in favor of the treatment group ($Z = 2.057$, $p < .05$). The proportional difference between the two groups over time represented a larger 40.7% reduction in the percentage of treatment officers relative to control officers who generated at least one use of force report.

Table 3. Impact of Body-Worn Cameras on Citizen Complaint Reports and Officer Use of Force Incidents

	Citizen Complaint Reports		Officer Use of Force Incidents	
	Treatment, N=218	Control, N=198	Treatment, N=218	Control, N=198
Pre-Intervention	54.6%	48.0%	31.2%	26.3%
Intervention	38.1%	45.5%	19.7%	27.3%
% Absolute Change	-16.5%	-2.5%	-11.5%	+1.0%
% Proportional Change	-30.2%	-5.2%	-36.9%	+3.8%
Difference-in-differences				
% Absolute Change	-14.0%		-12.5%	
% Proportional Difference	-25.0%		-40.7%	
Z-Test results	2.035*		2.057*	

* = $p < .05$
** = $p < .01$

These analyses also suggest that the modest contamination of control conditions noted by our analyses of call event responses did not result in diffusion of treatment effects or SUTVA violations in complaint and use of force incidents for control officers during the intervention period. Indeed, between the pre-intervention and intervention periods, the percentage of control officers with at least one complaint decreased by only 2.5% and the percentage of control officers with at least one use of force incident increased by 1.0%.[97] The presence of treatment officers with cameras at roughly one in five call events attended by control officers during the intervention period had no significant influences on how the control officers handled interactions with citizens.

We also conducted an exploratory analysis of the disposition of complaints against police officers during the intervention time period.[98]

[97] The difference of proportions test result for the percentage of control officers with at least one complaint between the pre-intervention and intervention periods was $Z = 0.4985$, $p = .6181$. The difference of proportions test result for the percentage of control officers with at least one use of force report between the pre-intervention and intervention periods was $Z = -0.2246$, $p = .8223$.

[98] A formal DID analysis was not possible due to 18.9% (76) of the 404 allegations against eighty-three treatment officers with at least one complaint and 18.7% (74) of the 396 allegations against eighty-nine control officers with at least one complaint incident not having dispositions at the time the data were provided to the evaluation team.

Complaints against officers can have multiple allegations of misconduct.[99] The eighty-three treatment officers with at least one complaint during the intervention time period had 328 allegations with formal dispositions completed while the eighty-nine control officers with at least one complaint during the intervention time period had 322 allegations with formal dispositions. BWC officers were modestly less likely to have the allegations in the complaints against them sustained and dispositions were made more quickly than their control officer counterparts. Some 14.9% of the 323 allegations against treatment officers and 19.9% of the 322 allegations against control officers were sustained against the officers. The 5.0% actual reduction difference (-25.1% proportional reduction difference) was statistically significant at a less restrictive $p < .10$ level ($Z = -1.682$). Body cameras also resulted in a statistically significant -15.5 day reduction ($t = -3.304$, $p < .01$) in the mean number of days between an allegation being made and disposed for officers in the treatment group (mean = 49.3, standard deviation = 57.2) relative to officers in the control group (mean = 64.8, standard deviation = 62.3).

CONCLUSION

The results of our randomized controlled trial suggest that the placement of BWCs on LVMPD officers reduced complaints and use of force reports for treatment officers relative to non-BWC comparison officers. These results support the position that BWCs may de-escalate aggression or have a civilizing effect on the nature of police-citizen encounters. Research suggests that police disproportionately use force when attempting to control or apprehend suspects in disadvantaged, minority neighborhoods.[100] Minority citizens are also more likely to feel that they experience disrespectful treatment at the hands of officers.[101] The complaint and use of force reductions associated with placing BWCs on police officers may be particularly important for improving police-community relations in impoverished, minority neighborhoods.

The presence of BWCs in police-citizen encounters also seemed to result in quicker resolutions of complaint allegations against treatment officers relative to control officers. The availability of video evidence on the

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[99] One control officer with 126 allegations stemming from a single complaint incident was excluded from this analysis as an outlier.

[100] Robert J. Kane, *The Social Ecology of Police Misconduct,* 40 CRIMINOLOGY 867, 887 (2002).

[101] RIOS, *supra* note 13, at 5; Rod Brunson, *'Police don't like black people': African American Young Men's Accumulated Police Experiences*, 6 CRIMINOLOGY & PUB. POL'Y 71, 85 (2007).

nature of the encounter seemed to allow internal affairs investigators to make quicker dispositions of complaints. However, it is important to note here that the reductions in citizen complaints may not reflect a substantive change in police-citizen behavior during interactions. The observed reductions could simply reflect a reduction in citizens filing frivolous complaints against BWC police officers.[102] Indeed, the modest reduction in complaint allegations sustained against BWC officers relative to control officers suggests that there were fewer legitimate complaints lodged against officers wearing the cameras.

Future research should attempt to interview citizens and police officers immediately following interactions to determine whether the BWCs do indeed have a calming effect. It would also be important to unravel the theoretical mechanisms associated with any behavioral changes influenced by the introduction of cameras to police-citizen encounters. From the standpoint of improving police-community relations, of course, the distinction between deterrence and self-awareness is irrelevant; that is, it only matters whether an intervention "works" by improving individual behavior.[103] However, determining whether deterrence, self-awareness, or some combination of the mechanisms associated with both theoretical perspectives would be important in developing further refinements to BWC deployments and policy.[104]

BWCs have been suggested to increase transparency in policing and, as a result, improve citizen views of police legitimacy.[105] Behavioral changes for BWC officers in police-citizen encounters represent key components of Professor Tom Tyler's process-based model of police legitimacy: public voice, neutrality in decision-making, trustworthiness, and treatment with dignity and respect.[106] Citizens are suggested to respond to BWC officers'

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[102] *See also* KATZ ET AL., *supra* note 12, at 41; WHITE, *supra* note 4, at 20–21.

[103] *See, e.g.*, Thomas J. Miles & Jens Ludwig, *The Silence of the Lambdas: Deterring Incapacitation Research,* 23 J. QUANTITATIVE CRIMINOLOGY 287, 288 (2007). Miles & Ludwig draw a similar substantive policy conclusion when assessing the relative impact of incarceration through on individual offending rates through incapacitation and deterrence crime control mechanisms.

[104] For instance, if encounter participants noted that they were deterred from aggressive behavior by an initial announcement that the interaction was being recorded, it might be important to make such announcements mandatory for all police-citizen encounters where a BWC is present. Further, if non-BWC officers report that responding to calls where BWC officers were present stimulated procedurally-just behavior because they were made more aware of their own decision-making processes, BWCs could be strategically allocated to maximize these kinds of second-order impacts.

[105] STANLEY, *supra* note 2, at 2.

[106] Tom R. Tyler, *Procedural Justice, Legitimacy, and the Effective Rule of Law*, 30 CRIME & JUST. 283, 284 (2003).

behavioral changes by viewing them as more legitimate and, in turn, become more willing to cooperate with the police.[107] Placing BWCs on police officers may also influence other dimensions of police legitimacy, such as police accountability, distributive justice, and crime control effectiveness. As suggested by others, police legitimacy is substantially more complex than ensuring procedurally-just encounters with citizens.[108]

The video recording of police-citizen encounters could enhance citizen perceptions of transparency and accountability in the day-to-day work of the officers that serve them. Lawfulness certainly matters to citizens when they appraise the legitimacy of the police; putting BWCs on officers may increase their adherence to the rule of law when stopping, searching, frisking, and/or arresting citizens.[109] BWC officers also might be less likely to allow extra-legal factors, such as the race of suspected offenders, to influence their decision-making, thereby improving distributive justice.[110] Finally, citizen perceptions of police effectiveness have been found to be related to assessments of police legitimacy.[111] Placing BWCs on officers could generate a deterrent effect on crime by increasing offenders' perceived risk of apprehension and punishment by capturing critical evidence on video that could be used against them in court proceedings. Unfortunately, there is little evidence available on the varied impacts that BWCs could have on the different aspects of police legitimacy.[112]

In this randomized controlled trial, we found that BWC officers generated more arrests and issued more citations than their control counterparts. Further research is needed to determine whether these increases in enforcement activity were driven by enhanced officer confidence that the video evidence collected would be used to hold offenders accountable for their transgressions, officers' concerns that supervisors who view videos of the interactions would hold them accountable for their discretionary actions, or both. Further research would help determine whether increased arrest and citation activity affected communities of color

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[107] Ariel, *supra* note 21, at 352–53.

[108] Anthony Bottoms & Justice Tankebe, *Beyond Procedural Justice: A Dialogic Approach to Legitimacy in Criminal Justice*, 102 J. CRIM. L. & CRIMINOLOGY 119, 168–70 (2012); Justice Tankebe, *Public Cooperation with the Police in Ghana: Does Procedural Fairness Matter?*, 47 CRIMINOLOGY 1265, 1265–67 (2012).

[109] Justice Tankebe, *Viewing Things Differently: The Dimensions of Public Perceptions of Police Legitimacy*, 51 CRIMINOLOGY 103, 108–09 (2013).

[110] Michael Reisig et al., *The Construct Validity and Refinement of Process-Based Policing Measures*, 34 CRIM. JUST. & BEHAV. 1005, 1024 (2007).

[111] Bottoms & Tankebe, *supra* note 108, at 146–47; Tankebe, *supra* note 108, at 1275–83.

[112] LUM ET AL., *supra* note 8, at 19; WHITE, *supra* note 4, at 19–20.

or other concerned communities disproportionately. It is also unknown how the observed increased enforcement activity of BWC officers might influence police legitimacy. It is possible that increased enforcement activity associated with BWCs might enhance legitimacy by improving police effectiveness in controlling crime and their capacity to hold offenders accountable.

Alternatively, increased enforcement activity could undermine police legitimacy if citizens view heightened arrests and citations as harmful to their communities. Citizens' appraisals of the police are influenced by the style of policing in their communities.[113] Policing strategies that emphasize increased investigative stops, criminal summonses, and misdemeanor arrests across jurisdictions have been shown to generate concern regarding racial disparities[114] and are suggested to contribute to the increased incarceration of young minority males.[115] The findings of this randomized controlled trial raise the possibility that, in our most vulnerable neighborhoods, increased enforcement activity associated with the placement of BWCs on officers could possibly undermine improvement in citizen perceptions of the police generated by reductions in complaints and use of force incidents.

Former President Obama's Task Force on Twenty-First Century Policing recommended that police departments need to include "an evaluation or assessment process to gauge the effectiveness of any new technology, soliciting input from all levels of the agency, from line officer to leadership, as well as assessment from members of the community."[116] Unfortunately, BWCs have been adopted by many urban police departments without much scientific evidence available to guide implementation.[117] To some observers, such as the American Civil Liberties Union, BWCs are "a [w]in [f]or [a]ll when implemented with the right policies in place."[118] However, without fuller understanding of the intended and unintended consequences of this new technology, it is difficult to know what the "right" policies are. The findings of this study suggest that as this body of scientific evidence continues to develop, jurisdictions should implement BWCs with eternal vigilance to both intended and unintended consequences.

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[113] *See, e.g.*, RONALD WEITZER & STEVEN A. TUCH, RACE AND POLICING IN AMERICA: CONFLICT AND REFORM 39–44 (2006).

[114] Jeffrey Fagan et al., *Street Stops and Broken Windows Revisited: The Demography and Logic of Proactive Policing in a Safe and Changing City*, *in* RACE, ETHNICITY AND POLICING 338–39 (Stephen K. Rice & Michael D. White eds., 1st ed. 2010).

[115] EPP ET AL., *supra* note 13, at 139–145, 155–60.

[116] PRESIDENT'S TASK FORCE ON 21ST CENTURY POLICING, FINAL REPORT OF THE PRESIDENT'S TASK FORCE ON 21ST CENTURY POLICING 35 (2015).

[117] LUM ET AL., *supra* note 8, at 3.

[118] STANLEY, *supra* note 2, at 1.