A randomized control trial evaluating the effects of police body-worn cameras

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Police body-worn cameras (BWCs) have been widely promoted as a technological mechanism to improve policing and the perceived legitimacy of police and legal institutions, yet evidence of their effectiveness is limited. To estimate the effects of BWCs, we conducted a randomized controlled trial involving 2,224 Metropolitan Police Department officers in Washington, DC. Here we show that BWCs have very small and statistically insignificant effects on police use of force and civilian complaints, as well as other policing activities and judicial outcomes. These results suggest we should recalibrate our expectations of BWCs' ability to induce large-scale behavioral changes in policing, particularly in contexts similar to Washington, DC.

body-worn cameras | field experiments | policing

SOCIAL SCIENCES

Police body-worn camera (BWC) programs are rapidly spreading across the United States. In 2015, the US Department of Justice awarded over $23 million in funding to support the implementation of BWC programs throughout the country (1), and a nationwide survey found that 95% of large police departments either have already implemented or intend to implement a BWC program (2). Much of the expansion has been motivated by a series of high-profile, officer-involved shootings, many of which were captured in bystander video and shared across social media. Stakeholders such as the American Civil Liberties Union, Campaign Zero, and Black Lives Matter have urged the police to equip BWCs as a technological solution to improve policing, or at least to document police practices and civilian behavior to resolve disputes (3, 4).

The widespread support for BWCs is due, in large part, to their anticipated effects on behavior. Both officers and civilians on the street may comport themselves differently if under the watchful lens of a camera. A wide range of research, dating back to the classic experiments at Hawthorne Works (5), has suggested that people act differently when they believe they are being watched, from increasing work productivity and charitable giving (6–9) to encouraging honesty (10), promoting adherence to recycling rules (11), stimulating voter turnout (12), and reducing theft (13). Across these settings, monitoring appears to shift behavior into alignment with socially acceptable conduct.

In the policing context, cameras are expected to encourage officer adherence to departmental protocols and deter police from engaging in unprofessional behavior or misconduct, especially unjustified use of force (14). Similarly, civilians interacting with a BWC-equipped officer may be less likely to engage in inappropriate or combative behavior. The underlying social or psychological mechanisms linking BWCs and behavior could include greater self-awareness, heightened threat of being caught, or a combination of the two. Whatever the exact mechanisms, commentators sometimes allude to a so-called "civilizing effect," wherein BWCs are predicted to calm all parties involved and reduce the likelihood that violence occurs (15). By capturing the police–civilian interaction, the cameras are also expected to have evidentiary value, both for internal affairs and criminal investigations (15, 16).

The existing evidence on whether BWCs have the anticipated effects on policing outcomes remains relatively limited (17–19). Several observational studies have evaluated BWCs by comparing the behavior of officers before and after the introduction of BWCs into the police department (20, 21). Other studies compared officers who happened to wear BWCs to those without (15, 22, 23). The causal inferences drawn in those studies depend on strong assumptions about whether, after statistical adjustments are made, the treatment is independent of potential outcomes. In particular, we would need to believe that, after conditioning on a set of pretreatment covariates, BWCs were as if randomly assigned.

A small number of randomized controlled trials (RCTs) of BWCs have been conducted, with mixed results. In a series of RCTs conducted across several sites in the United Kingdom and the United States, BWCs appeared to increase police use of force at some sites and decrease it at others (24, 25). Cameras appeared to decrease complaints in some experiments but not others (16, 25). Further trials found no detectable treatment versus control differences on measured outcomes (26). The extant set of RCTs has typically been limited by either small sample sizes or shift-level random assignments that introduce the potential for within-officer spillover (14, 27).

Methods

We collaborated with the Metropolitan Police Department of the District of Columbia (MPD) to design and implement an RCT to evaluate the effects of BWCs citywide. Specifically, as part of MPD's deployment of BWCs to its police force, approximately half of all full duty patrol and station

Significance

Police departments are adopting body-worn cameras in hopes of improving civilian–police interactions. In a large-scale field experiment (2,224 officers of the Metropolitan Police Department in Washington, DC), we randomly assigned officers to receive cameras or not. We tracked subsequent police behavior for a minimum of 7 mo using administrative data. Our results indicate that cameras did not meaningfully affect police behavior on a range of outcomes, including complaints and use of force. We conclude that the effects of cameras are likely smaller than many have hoped.

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Data deposition: The cleaned dataset sufficient for reproducing the difference-in-means estimates of the treatment effects have been deposited in the Open Science Framework, https://osf.io/p6vuh/.

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officers were randomly assigned to wear BWCs, while the other half remained without BWCs. With 2,224 MPD members participating in the trial, this study is the largest randomized evaluation of BWCs conducted to date. Our project was deemed "not human subjects research" by the Yale University IRB (protocol no. 2000020390), as all study activities were carried out by MPD.

The primary outcomes of interest were documented uses of force and civilian complaints, although we also measure a variety of additional policing activities and judicial outcomes. All outcomes were measured using administrative data. Before obtaining outcome data, we developed a detailed write-up of the methodology and planned statistical analyses (a preanalysis plan) and publicly shared it on the Open Science Framework. The preanalysis plan is included in *SI Appendix*.

Our study encompassed the entire department and included geographic coverage of the entire city. We identified eligible officers within each of the seven police districts (as well as several specialized units) based on the following criteria: The officer was on active, full duty administrative status and did not have a scheduled leave of absence during the study period, held a rank of sergeant or below, and was assigned to patrol duties in a patrol district or to a nonadministrative role at a police station. Eligible officers within each district or special unit were then randomly assigned to one of two groups: (*i*) no BWC (control) or (*ii*) with BWC (treatment). Specifically, treatment entails assignment of an eligible participant to wear and use a BWC in accordance with MPD policy. MPD General Order SPT-302.13 specifies that "[m]embers, including primary, secondary, and assisting members, shall start their BWC recordings as soon as a call is initiated via radio or communication from OUC [Office of Unified Communications] on their mobile data computer (MDC), or at the beginning of any self-initiated police action." The general order enumerates the range of events for which officers are required to activate their BWCs; this list is included in *SI Appendix*.

Randomization was implemented using a block-randomized assignment procedure. This approach, which uses pretreatment information to group officers into blocks before randomly assigning a fixed number of cameras to officers in each block, increases the statistical power of the experimental design and enforces treatment-versus-control balance on the covariates according to which blocking occurs. We applied a two-level blocking approach: The "major" blocks were the seven police districts and three special units, and the minor blocks were constructed using a clustering algorithm based on the background characteristics of the officers (28). Based on the eligibility requirements noted above, our sample consisted of 2,224 MPD members, with 1,035 members assigned to the control group and 1,189 members assigned to the treatment group.

As anticipated in our preanalysis plan, some officers who were assigned cameras did not install or use them, and some officers who were not assigned cameras nevertheless obtained them. We estimate two compliance measures: the number of videos uploaded to the video database by treatment officers and the average length of the videos in minutes, as compared with control officers. If officers complied with the randomization protocol, we would expect that officers assigned BWCs would make vastly more videos per year, as well as have a longer average length of videos, than their counterparts in the control group. On average, treatment officers uploaded about 665 videos annually (compared with 14 videos uploaded among control officers). The average video recorded by a treatment officer was over 11 min long, while the average video recorded by a control officer was just 0.8 min long. For both manipulation check measures, the treatment assignment is both substantively and statistically significant ($p < 0.001$). We conclude that compliance with the study protocol was high.

Following best practices in settings encountering two-sided noncompliance, we conducted all of our analyses according to the original random assignment (29). Our experiment thus recovers estimates of the effect of being assigned to a BWC on a variety of outcomes (the intention-to-treat effect).

Measurement Strategy. We assessed the effect of BWCs on four families of outcome measures: police use of force, civilian complaints, policing activity, and judicial outcomes.

i) Police use of force was based on officers' self-reported use of force (in accordance with MPD policy). It included a count of all use of force incidents as well as measures of serious uses of force (as defined by MPD policy), nonserious uses of force, and use of force incidents by the race of the subject of force.

ii) Civilians can file complaints in two ways: with MPD itself or with the independent Office of Police Complaints. Our measure was the total number of complaints associated with an officer from both sources. We also disaggregated the complaints by disposition: sustained, not sustained, or unresolved due to insufficient facts.

iii) The policing activity category included traffic tickets and warnings issued, reports taken from particular types of calls for service, arrests on specific charges (e.g., disorderly conduct, traffic violations, assaults against a police officer), and injuries sustained by officers in the line of duty. We used these measures to evaluate the effects of BWCs on officer discretion and activity, as well as on civilian behavior.

iv) Finally, we examined the effects of BWCs on judicial outcomes, measured by whether MPD arrest charges are prosecuted by the US Attorney's Office (USAO) or the Office of the Attorney General (OAG) and the disposition of those charges. Our examination of this set of outcomes was constrained by limitations in the available data. Namely, we did not have access to the full datasets managed by the USAO, OAG, and the courts. We instead had access to a subset of these data available to MPD, which captures only the initial charges on which an individual was arrested. A consequence is that we were unable to track court outcomes for any changes to those initial charges. As this limitation applies to both control and treatment groups, however, we were still able to conduct a preliminary analysis on the evidentiary value of BWCs.

Due to logistical constraints, MPD deployed cameras on a district-by-district basis over the course of 11 mo. Officers in two of the seven police districts received cameras in late June 2015, with the deployment to the remaining districts taking place from March to May 2016. By integrating randomization directly into the BWC deployment process, we were able to conduct this study at marginally low cost to MPD.

To address the staggered deployment process, the data collection period varies for each police district, based on the start date of BWC deployment in that district. All outcomes were obtained at the officer level and translated into yearly rates. These rates were calculated from the date that the cameras were first deployed in each district. We calculate these rates before and after the intervention based on a window of 212 d, because 212 is the number of days between deployment and the end of the study period for the district that was the last to receive cameras. The pretreatment and posttreatment periods are of the same length for all districts; the pretreatment measurements come from the same 212-d window (in the previous year) as the posttreatment measurements, to account for seasonality in policing and desensitization to the treatment over time.

Because all of our outcomes are unconditional event counts translated into yearly event rates per 1,000 offices, our measurement procedure avoids the posttreatment bias that would be associated with measuring various conditional quantities. For example, we might want to measure the fraction of an officer's civilian interactions that include use of force, but, since the officer–citizen interaction is posttreatment, we cannot condition on it without the risk of bias.

Estimation Strategy. We use two estimators of the average treatment effects: (*i*) difference-in-means with inverse probability weights to account for differential probabilities of assignment by block and (*ii*) regression of outcome on treatment assignment with controls for pretreatment characteristics and inverse probability weights. Specifically, we control for the pretreatment value of the outcome (e.g., past use of force), pretreatment covariates for the officer, and indicators for each major block. Eq. **1** provides the exact specification, as preregistered before the realization of outcomes.

$$Y_{POST} = \beta_0 + \beta_1 Z + \beta_2 Y_{PRE} + \beta_3 Block + \beta_4 X + \epsilon, \qquad [1]$$

where Z is the treatment indicator (officer assigned camera or not); Y_{PRE} is the pretreatment value of the outcome under study; *Block* is a vector of indicator variables for an officer's home district or special unit; X is a vector of pretreatment covariates that includes race, gender, and length of service; and ϵ is the error term. We estimate Eq. **1** using weighted least squares regression with inverse probability weights, which are calculated as the inverse of the probability of each unit being in its observed condition (29). We use HC2 robust standard errors for variance estimation (30). We conduct our primary analysis among officers in the seven districts of DC (n = 1,922). We exclude officers in special units from this analysis, as policing activities and camera use patterns may differ between these units and the district officers. We conduct this analysis at the officer level, and report results as a yearly rate per 1,000 officers. Our analyses were conducted by two independent statistical teams, to help avoid coding errors and as a check of convergence in results.

Data Availablity. The cleaned dataset sufficient for reproducing the difference-in-means estimates of the treatment effects will be made

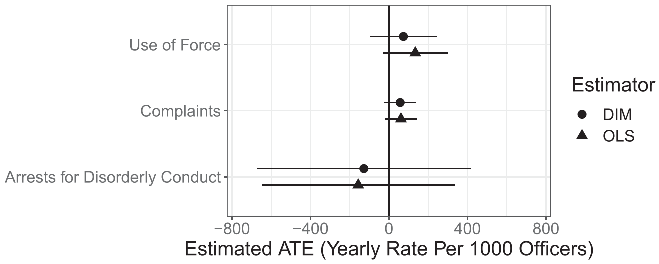

Fig. 1. Average difference (with 95% confidence interval) between BWC and non-BWC groups, per 1,000 officers over a year for police use of force, complaints filed against officers, and arrests for disorderly conduct. We show findings from both our difference-in-means (DIM) estimator and ordinary least-squares (OLS) regression including pretreatment covariates.

available at the Open Science Framework at https://osf.io/p6vuh/. We are unable to make public the raw data from which the cleaned dataset was produced, due to privacy concerns of both officers and civilians. We are also unable to release the officer-level covariate information that we use to estimate the covariate adjusted models, as these data would uniquely identify individual officers.

Results

Across each of the four outcome categories, our analyses consistently point to a null result: The average treatment effect estimate on all measured outcomes was very small, and no estimate rose to statistical significance at conventional levels. Because our study has a large enough sample size to detect small effect sizes, these failures to reject the null are unlikely to be due to insufficient statistical power. Fig. 1 plots the estimated average treatment effect (as a yearly rate per 1,000 officers) of BWCs on police use of force, civilian complaints, and officer discretion (as measured by arrests for disorderly conduct). Our best guess is that cameras caused an increase of 74 (SE = 87) uses of force per 1,000 officers, per year. This estimate is not statistically significantly different from zero. The effects on complaints (57 per 1,000 officers per year, SE = 41) and arrests for disorderly conduct (−128 per 1,000 officers per year, SE = 277) were also nonsignificant. Effect estimates on court appearances, judicial outcomes, domestic violence calls, and other measures of police behavior (all null) are included in *SI Appendix*.

Discussion

We consider here a few possible explanations for our null findings. First and most obviously, it is possible the null finding needs no explanation: The devices, in fact, have no effect on behavior. Perhaps neither the officer nor civilian involved in an interaction are actually aware of or affected by the camera, either due to attention being diverted elsewhere or desensitization over time to the presence of the cameras.

Second, Washington, DC may be different from other places in important ways. Perhaps BWCs have no effect in the nation's capital, but they do in other municipalities. We are sympathetic to this possibility, but we also note that, as BWCs were randomly assigned within each of the seven police districts, we conducted the equivalent of seven mini-experiments. Despite substantial district-to-district heterogeneity in baseline outcomes, we observe small, insignificant effects in all seven districts.

A third explanation for the null findings considers the possibility that other factors are masking the true effect of the BWCs: The cameras do affect the measured outcomes, but these effects are being hidden by interference across units, or spillovers from treated to control officers. Approximately one-third of calls were responded to by control officers only, one-third by treatment officers only, and the last third by a mix of treatment and control officers. This distribution of calls indicates that control officers were frequently performing their duties without cameras nearby. As a check of whether the introduction of cameras affected both treatment and control officers, we examined time trends for documented uses of force and civilian complaints before and after cameras were deployed (analysis presented in *SI Appendix*). We observed no differences in precamera versus postcamera outcomes for either group.

Finally, the true effect of BWCs may be masked by the widespread presence of nonpolice cameras (e.g., civilians' cell phones). Civilians regularly record encounters with MPD members with their own cameras, and closed caption television (CCTV) is widespread. Perhaps the BWCs do not change behavior at the margin, simply because there is no more room to have an effect. To explore this possibility (we note that this analysis was not preregistered), we examined the effect of treatment on use of force at night, when exposure to nonpolice cameras is lower. We also found no effect of cameras on this alternative dependent variable.

Other researchers have suggested that BWCs may fail to affect results because of nonadherence: Officers, for a variety of reasons, may not use their assigned cameras according to departmental policy (15, 22, 26). Officers may fail to activate the camera, for example. We have no indication that nonadherence was a widespread problem in our experiment. For 98% of the days in 2016, MPD averaged at least one video (and often many more) per call for service associated with a treatment officer. Further, even for the 2% of days in 2016 in which the number of videos uploaded was less than the number of incidents for which we would expect them, the difference is minimal, with 96% average adherence based on our measure. That said, effects may depend on the level of discretion officers are given to activate the cameras, although evaluation of that possibility will have to await further experiments.

We acknowledge that BWCs may have had effects that are not measurable with administrative data. For example, it may be the case that there were uses of force that were previously going unreported, and those have now dropped with the introduction of BWCs. However, because our data do not capture unreported uses of force, we are unable to detect this kind of change. As a matter of speculation, however, we find it implausible that we would measure very small effects on reported outcomes but that the true average effect on unreported outcomes is large.

In summary, we measured the average effects of BWCs on documented uses of force and civilian complaints as well as a variety of additional policing activities and judicial outcomes. Our sample size was unusually large, enhancing our ability to detect differences, should they exist. In addition, our comparison groups were constructed from an individual-level officer randomization scheme, which avoids several problems of inference present in other methodologies used to date. We are unable to detect any statistically significant effects. As such, our experiment suggests that we should recalibrate our expectations of BWCs as a technological solution to many policing difficulties.

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