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Body-Worn Cameras and Citizen Interactions with Police Officers: Estimating Plausible Effects Given Varying Compliance Levels

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Recent citizen deaths involving police use of force have increased discussion surrounding police accountability and community relations. One piece of this discussion is the use of body worn cameras (BWCs) by officers. Unfortunately, little rigorous research has been conducted to estimate the effectiveness of BWCs in reducing problematic police-citizen interactions. In this paper, we estimate two measures of effectiveness of BWCs by comparing incidents that occur in a squad assigned cameras to incidents that occur in a squad assigned control. First, we estimate the effect of being assigned a BWC (but not necessarily using the camera) on reducing complaints and resistance associated with incidents. Second, we employ data on BWC use to estimate the effect of cameras if they were used with full compliance. Together, these two estimates provide a plausible range of effectiveness that policymakers can expect from BWCs. We find that BWCs have no effect AQ3 on the rate of arrest or resistance, but can substantially reduce complaints.

Introduction

Over the past year the high profile deaths of Michael Brown in Ferguson, Eric Garner in New York City, Tamir Rice in Cleveland, Walter Scott in North

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Charleston, and Freddie Gray in Baltimore have resulted in mass protests against the police. A wide range of police accountability reforms in these agencies, as well as in police agencies across North America, have followed. One mechanism of enhanced police accountability that has gained substantial momentum has been the adoption and implementation of police officer body worn cameras (BWCs). Today, it is estimated that out of about 18,000 law enforcement agencies, about 4000–6000 have adopted BWCs.[1] It is anticipated that over the next three years a large number of agencies will adopt BWCs as a consequence of President Obama's $75 million program putting 50,000 BWCs on the street.[2]

Supporters of BWC's have claimed they are beneficial to the police and the communities they serve in several ways. First, they increase accountability and transparency, which will for example reduce complaints, help identify persistent problems within the agency, and lead to greater police legitimacy.[3] Second, BWCs have a civilizing effect on the police and those who they encounter, resulting in a de-escalation of force among all parties (White, 2014). Third, they have evidentiary benefits through their provision of an "objective account" of the event, which can result in higher arrest and prosecution rates (Katz, Choate, Ready, & Nuño, 2014). Some researchers, however, have questioned whether the occupational culture of the police presents a strong barrier to the implementation of BWC's (Young & Ready, 2015), and whether negative views of BWC could undermine implementation efforts (Jennings, Fridell, & Lynch, 2014). Unfortunately there is little empirical evidence that has examined the effectiveness of BWCs (for exception see Ariel, Farrar, & Sutherland, 2014), and the research that has been conducted has not examined the effect of implementation fidelity on desired outcomes.

The purpose of this paper is to evaluate the effects of BWCs on police interactions with the public by examining data from a large southwestern city police department collected as part of a Bureau of Justice Assistance (BJA) funded evaluation. First, we examine whether there is an "intent to treat" effect. That is, we examine whether the mere assignment of a BWC to an officer has an impact on police and citizen behavior through measures of citizen complaints, resisting arrest and the number of arrests made by an officer. Second, we examine the effectiveness of BWCs in the context of actual usage through the use of instrumental variable analysis, a common econometric technique to retrieve a "local" average treatment effect for those who activate the cameras. In the below sections we review the rationale for police BWCs and prior research examining their use.

1. http://www.wsj.com/articles/police-cameras-bring-problems-of-their-own-1428612804.
2. https://www.whitehouse.gov/the-press-office/2014/12/01/fact-sheet-strengthening-community-policing.
3. Miller, Toliver & Police Executive Research Forum (2014).

Rationale for Police Worn Body Cameras

One theoretical approach that is helpful in understanding the potential impact of BWCs on police and citizen behavior is deterrence theory. Deterrence theory posits that as the certainty, severity, and swiftness of punishment increases, the likelihood of misbehavior decreases (Beccaria, 1963; Stafford & Warr, 1993). A large body of literature relying on a variety of samples such as college students (Paternoster, Saltzman, Waldo, & Chiricos, 1983a), school attending youth (Paternoster, Saltzman, Waldo, & Chiricos, 1983b), prisoners (Block & Gerety, 1995), and active offenders (Piquero & Rengert, 1999) has confirmed the relationship between elements of deterrence theory and decisions regarding criminal behavior. More recent research has also found that this relationship holds regardless of whether the punishment is an extra-legal (e.g. shame) or legal (e.g. sentence length) sanction (Meier & Johnson, 1977). In fact, some empirical research suggests that it is a fairly powerful theoretical explanation for predicting criminal behavior (Weisburd & Piquero, 2008, p. 475).

Pogarsky and Piquero (2004) expanded the scope of deterrence theory by applying it to police misconduct. The authors examined traditional "deterrence considerations" such as certainty, severity, celerity, and impulsivity and intention to engage in police misconduct. Their sample relied on responses from about 200 police officers from a southwest police department who were asked about various hypothetical scenarios involving police misconduct. Pogarsky and Piquero (2004) concluded "deterrence considerations appear to figure prominently in police misconduct decisions. For example, although perceived sanction severity offered little deterrent threat, perceived sanction certainty and celerity were negatively associated with police misconduct" (p. 381).

A goal of many agencies that implement BWCs is to deter officers from engaging in misconduct. BWCs, if activated, are thought to increase the likelihood that misconduct will be observed. This, in turn, is thought to increase the possibility of an officer being disciplined for misconduct. Therefore, BWCs might decrease the benefit associated with misconduct because the potential cost increases (e.g. certainty of punishment).

More recently scholars have begun to explore the role of organizational justice as an alternative mechanism for understanding police misconduct. This body of literature began in socio-psychology and hypothesized that organizational justice influences organizational practices and outcomes. It is posited that the fairer organizations (or their managers) are to their employees, the more effective their employees will perform. Conversely, those organizations (or managers) who are not fair to their employees will suffer counterproductive employee behavior. Cohen-Charash and Spector (2001) examined the relationship between organizational justice and organizational practices and outcomes through their meta-analysis of 190 prior workforce studies. They reported that organizational justice exhibits three primary dimensions: distributive justice, procedural justice, and interactional justice. Cohen-Charash

and Spector (2001) discussed distributive justice as the perceived fairness of outcomes. They reported that there was a strong and robust relationship between how fair organizational outcomes were distributed (e.g. salary increases, discipline, promotions) and the quality and quantity of work performed by employees. Procedural justice, or the fairness of the process by which outcomes are distributed, was also found to be significantly associated with organizational performance. This finding held even when it resulted in a negative outcome. In other words, employees accepted a negative outcome as long as the process in which the outcome was determined was perceived to be administered fairly. Last, interactional justice was found to be related to organizational performance. Interactional justice pertains not to the actual outcome, but to how the outcome was delivered. Those interactions where the outcome is delivered impolitely, dishonestly, and/or disrespectfully result in employees who are less committed to organizational goals and more likely to be engaged in work-place misbehavior.

Wolfe and Piquero (2011) applied the theory of organizational justice to police misconduct in their analysis of data obtained from 499 Philadelphia Police Department officers. The authors argued that officers who believed that their department acted unfairly, distributed resources unfairly, or treated officers disrespectfully were more likely to be disobedient, violate departmental policy, and engage in employee deviance. They further posited that organizational injustice can result in some officers engaging in noble corruption. Officers, for example, might perceive unjust state laws and organizational policies as preventing them from achieving justice, and that it is necessary for them to engage in various forms of "street justice" to protect the public. Related, they argued that agencies that are perceived to engage in unfair and unjust investigations of police officer misconduct were more likely to have officers more strongly adhere to the code of silence, because of fear that an officer will be unfairly treated by the police organization during the course of its investigation. The authors examined these issues by linking data obtained from the officer's official personnel records with self-report data obtained from the officers. Analysis of the data suggested that the organizational justice framework might be useful for understanding police misconduct. Specifically, they reported that "perceptions of organizational justice were associated with lower likelihood of officers having citizen complaints filed, IAD investigations instigated, or disciplinary charges brought against them" (p. 346).

This same theoretical vehicle suggests that citizens might also alter their behavior if BWCs are worn by the police. Police officers who wear BWCs, as discussed above, might increase their performance (i.e. more respectful, polite, fair, transparent) for reasons related to increased police accountability and organizational justice. Citizens in turn should perceive the police as more legitimate through such mechanisms as distributive justice, procedural justice, and interactional justice, and will be more cooperative and more likely to trust the police during an encounter where a BWC is present. Support for police legitimacy and procedural justice being associated with increased levels of

public cooperation and trust of the police have been repeatedly demonstrated in the literature (Mazerolle, Bennett, Davis, Sargeant, & Manning, 2013; Sunshine & Tyler, 2003). It stands to reason that police worn body cameras might increase citizens' perceptions of police legitimacy and procedural justice, and result in fewer conflicts between the public and police.

Taken together these two perspectives (i.e. deterrence, procedural justice) suggest that officer worn body cameras might impact officer and citizen behavior in two important ways. One might be that it deters police officers and those who they encounter from acting inappropriately. If they do, they will be more likely to be caught and punished. Another mechanism through which BWCs might operate is through organizational or procedural justice. When police officers believe that they will be treated fairly, they will perform better and more fairly. Likewise, if the police perform in procedurally just ways and are perceived as legitimate, the public will trust the police more and will be less likely to behave poorly and more likely to cooperate and comply with the police.

Other mechanisms by which body cameras might reduce misconduct or complaints against the police, however, are unclear. One explanation is that BWCs might have a civilizing effect on both the police and public during an encounter. Members of the public who have contact with the police might recognize the presence of an officer worn body camera and restrain their behavior that might otherwise have escalated into conflict with the officer. Likewise, police officers who wear a BWC during an encounter with the public might restrain their behavior in situations that might otherwise have normally resulted in an escalation of force (i.e. verbal or physical). Additionally, police officers might be deterred from engaging in misconduct because the likelihood of the encounter being discovered by those who hold the police accountable (e.g. supervisors, media, civilian review boards) is heightened when a camera is present and activated. Ariel et al. (2014) notes that the underlying element to these reasons is that BWCs increase transparency of the encounter "and the curtain of silence that protects misconduct can be more easily unveiled, which makes misconduct less likely" (p. 10).

Prior Research on Police BWCs

The television show Community Oriented Policing Services (*Cops*) first aired in 1989, and continues as one of the longest running shows on television. The American public's familiarity with seeing police work on video is engrained, but this is still an external observer with a camera, recording events for the purpose of producing a television show. As video recording technology advanced sufficiently to allow for compact devices that could fit on a patrol car's dashboard without significantly interfering with ordinary responsibilities, police departments began to adopt dashboard cameras. Through the 1990s and early 2000s the U.S. Department of Justice's Office of COPS In-Car Camera

Program provided millions of dollars in grants to purchase and deploy dashboard cameras to law enforcement agencies across the country (Fiumara, 2012). Dashboard cameras have been demonstrated to improve officer safety and accountability. In part because of this, they have been widely adopted and accepted by law enforcement agencies and officers over the past few decades (Baker, 2004).

More recent technological developments in the portability of devices with video recording capability have renewed the discussion about cameras in policing. On-officer, BWC devices are an emerging technology, lauded for their contribution to police accountability and transparency, as well as their evidentiary value, an increasing number of police departments are deploying them, if not wholesale, in a limited capacity (White, 2014). Despite the exponential growth in the number of agencies purchasing and deploying BWCs, there is still little empirical evidence to support the claims of their supporters, or understand their unintended consequences. To date, only a few empirical studies have examined the impact of BWCs and the process of their implementation in the United States.

In the first peer reviewed study on the matter, the Rialto, California Police Department (RPD), deployed BWCs for 12 months beginning in February 2012. At the time of the study, the RPD identified 54 frontline officers (out of a total of about 110 sworn officers in the department) who would be eligible to wear the BWC. The officers were randomly assigned by shift to either wear (i.e. treatment) or not wear (i.e. control) the BWC on a weekly basis. Over the course of the study period, this method yielded 489 treatment shifts and 499 control shifts being observed. The results were favorable for the use of BWCs. The study found that citizen complaints dropped by 88%, from 28 complaints in the year prior to just three complaints during implementation. There were 61 use-of-force incidents before implementation, which declined by 60% to 25 incidents during the implementation period. Similar results were observed in a follow up study conducted by Jennings, Lynch and Fridell (2015) who used a pre-post test randomized control trial design to examine the impact of 46 BWCs in the Orlando Police Department. The study examined police misconduct through two primary outcome measures including the number of resisting arrest incidents and the number of serious external complaints against the police. The authors reported that among the treatment group resisting arrest incidents declined by 53.4% and serious external complaints declined by 65.4%.

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Unique compared to the above, Ready and Young (2015) examined the relationship between BWC policy and police initiated stops and arrests among 50 officers over a 10 month period in one southwestern city. Half way through the study period the agency's BWC policy changed from "mandatory," which required the 50 officers to activate their BWC when they "approached the scene of the call or at the point of initiation." (p.450) to "discretionary," which allowed the 50 officers to activate their BWC at their own discretion. The authors reported that while the BWC activation policy had no significant

impact on the self-reported number of proactive (e.g. stop and frisk) or reactive (e.g. CFS) contacts with the public, officer self-reports of the number of arrests were more likely to occur among officers who wore the BWC during the discretionary use policy period.

There were, however, some of limitations to the above studies. First, and perhaps most related, none of the above studies took into account officer compliance with BWC policy. That is prior research did not examine whether observed changes were the result of BWC assignment or their actual use in the field. Second, and related, Jennings et al. and Ready and Young's research largely relied on volunteer samples.[4] The primary issue here being whether officers who volunteered to wear BWCs behave differently than officers who do not volunteer to wear BWCs. Compared to non-volunteers, for example, BWC volunteers might be more likely to comply with BWC policy, might be more predisposed to positively change their behavior, or might exhibit some other behavioral trait that differentiates those who would self-select into a BWC program vs. those who would not. The purpose of the present study is to address these gaps in research by examining the impact of BWC compliance in the context of actual usage through the use of instrumental variable analysis.

4. Jennings et al. sample was comprised of 100% volunteers (n = 46) and Ready and Young's sample was comprised of a treatment group of 25 volunteers and 25 selected officers, which were matched to a control group of another 50 officers (total N = 100). Ready and Young's model controlled for volunteer status and thus their main treatment effect reflects the difference between the 25 selected officers and the 50 matched officers. The effects of being a volunteer must be combined with the treatment effect to estimate the treatment effect for the volunteers who wore cameras. For example, looking at Table 1 in Ready and Young (2015), the effect for the 25 selected treatment officers on the odds of citation was an odds ratio of 1.85 (SE = .5), and the effect of volunteers on citations was an odds ratio of about 4 (SE = 2.4). We can estimate the true estimated treatment effect for the volunteers by multiplying these odds ratios together (i.e. there is an effect on the log odds for the non-volunteers, X, and an effect of being a volunteer that always received a camera on the log odds, Y, and so we add these effects together for an effect on the log odds of $X + Y$, which when we convert to an odds ratio is exp $(X + Y)$ = exp $(x) \times$ exp $\times$ (Y)). Combining these effects (by multiplying them) yields an odds ratio comparing the 25 volunteers to the 50 matched officers of $1.85 \times 4 = 7.4$. Since these odds ratios have sampling variances, the best approximate guess as to the sampling error of this effect is, $V\ (X \times Y) = X\text{^}2 \times V(Y) + Y\text{^}2 \times V\ (X) + 2 \times X \times Y \times$ COV (X, Y), but since the covariance is not reported, we use $V\ (X \times Y) = X\text{^}2 \times V\ (Y) + Y\text{^}2 \times V\ (X)$ (See Goodman, 1960), so the standard error is SE = sqrt $(1.85\text{^}2 \times 2.4\text{^}2 + 4.0\text{^}2 \times .5\text{^}2) = 4.9$. Again, without knowing the covariance, this may be an over estimate of the variance if the sampling covariance of these estimates is negative, which simulations suggest. If it is close, however, it means that the z-score for the volunteer effect is 7.4/4.9 = 1.5, which is not statistically significant. Thus, it is plausible that while the cameras have an effect for the selected officers with regards to citations, there may not be an effect for the volunteers. Moreover, their appendices estimate the differences between the 50 officers in the treatment group (those who volunteered and those that did not) and the 50 matched officers. If there are differences between the volunteers and non-volunteers, then the paper only plausibly reflects the impact of the non-volunteers.

The Present Study

BJA awarded funding to the Phoenix Police Department (PPD) to purchase, deploy and evaluate on-person video cameras that record the interactions between community members (e.g. the public, suspects and victims) and officers. The camera is worn on the officer's uniform, placed optionally on the shoulder lapel or upper placket, with a forward-facing viewable area. The camera captures events and interactions that take place between suspects, victims, witnesses and the officer. The video recordings can also be used by police to document statements, observations, behaviors, and other evidence.

The purpose of the present study is to determine two plausible estimates of the effectiveness of BWCs on several outcomes. The first estimate examines the effect of assigning the BWCs to officers, which is akin to (but not equivalent to) "intent to treat" analyses. This effect will offer an estimate of the utility of BWCs given rates of non-compliance. The second effect is (to the authors' knowledge) unique to this study, in that we estimate the effectiveness of BWCs on those who actually use them, which is akin to (but also not equivalent to) the "treatment on the treated" estimator employed in causal analysis. This offers an estimate of the upper-bound of effectiveness under conditions of near perfect compliance. The estimate is possible through data we collected related to incidents associated with officers assigned to wear cameras as well as incidents associated with activated cameras.

Data Collection

PPD is a large municipal police agency, with more than 3000 authorized sworn personnel, and serves a community of more than 1.5 million people; making it the sixth largest city in the US. The PPD is organizationally divided into precincts and beat areas for principal patrol services. At the time of the study, the PPD's patrol division was divided into eight precincts. The Maryvale Precinct, where the present study took place, is approximately 15 square miles, and is operationally and geographically divided into two similarly sized patrol areas. Each of the two areas is assigned six patrol squads to provide first response coverage to calls for service on a twenty-four hour basis, seven days a week. While small changes in staffing occurred throughout the study, generally there were between 100 and 110 patrol officers equally divided between Areas 81 (control) and 82 (treatment).

The community characteristics of the study setting were important to the site selection for the study. The community served by the precinct has a population of about 105,000 residents, and is primarily comprised of Hispanic residents who are poorer and more likely to be unemployed than residents living in other areas in the city. Maryvale has historically been and continues to be a location for a high-volume of police activity, calls for service, and elevated crime rates, particularly for violent crime, relative to other areas in the city.

In 2010, the UCR violent crime rate for Maryvale was approximately 85 crimes per 10,000 residents, compared to 55 per 10,000 for the rest of the city. The organizational structure, combined with the historically higher than citywide average crime rates lead to the Maryvale Precinct being selected as the site for the proposed quasi-experimental design for the body worn camera project.

Intervention Design

The design and implementation of the intervention included the purchase of 56 camera systems and deploying them in the Maryvale Precinct. The implementation of the VIEVU camera system occurred in one of the two Maryvale Precinct squad areas, Area 82. This group is referred to as the treatment group (the other area, 81, is the control group). The equipment provided for simultaneous coverage (using the VIEVU system) seven days of the week, during all three shifts, by all deployed officers, and allowed for all officers to download data prior to next shift. All officers assigned to the six squads in the treatment area were issued the equipment and were provided training in its use and maintenance through a coordinated effort led by the precinct commander and VIEVU. Departmental policy involving the use of the cameras was formulated prior to implementation and was also an integral part of the training by the PPD.

The Maryvale Precinct is divided into two patrol areas, Areas 81 and 82. Given this organizational structure, the two areas had the same command structure, and the same shift assignment and schedule. During the project-planning phase the two areas were examined for differences in the communities they served. As seen in Table 1 we found differences between the two with respect to population, socio-economic characteristics, domestic violence, and crime. The control area is larger in population, more affluent, older, and with fewer minorities. While the control area experiences the same amount of total crime, it tends more towards property crime.

Table 1 also presents statistics for the outcomes of interest. Looking first at complaints, we see that the rate of complaints per 1,000 incidents is comparable between the treatment (1.70) and control (1.66) areas. The treatment area was slightly less productive, as measured by arrests, showing 2.49% of incidents yielding an arrest compared to 2.71% of incidents yielding an arrest in the control area. Resistance to arrest in the treatment area (6.2%), however, was almost a third less than the control area (9.3%).

Quasi-Experimental Research Design and Unit of Analysis

There are several reasons that the design of the intervention precludes a causal analysis. First, the unit of intervention assignment (the two patrol areas) has one a single unit per intervention status; sometimes this is called

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Table 1 Characteristics of target and comparison areas during pre-test period

Characteristic	Control area Area 81	Treatment area Area 82
Total population	71,676	56,630
Age		
% Under 18 years	39.45	43.13
Ethnicity		
% Hispanic	71.1	82.5
% Native American	1.3	1.3
% African American	6.4	3.9
Poverty		
Mean household income	$53,646	$44,895
% Owner occupied	63.7	52.8
Geographic size (Square miles)	7.4	7.9
Number of incidents[b]	33,071	23,576
Incident involving		
Violence	9.1%	10.4%
Drugs	1.2%	1.1%
Property	17.6%	20.3%
Traffic stops	24.5%	22.0%
Number of incidents generating complaints made against officers[b]	55	40
Rate of complaints per 1000 incidents	1.66	1.70
Arrests[b]	896 (2.71%)	587 (2.49%)
Any resistance if an arrest	9.3%	6.2%

[a]Demographic data derived from US Census American Community Survey 2012.; [b]Data obtained from PPD CAD/RMS data from April 1 2012–March 31 2013.

the "*N* of 1" problem. Thus, any differences between these two areas may or may not be due to the intervention, but instead may also be due to other unobserved factors, or both. Second, even without the "*N* of 1" problem, the appropriate unit analysis is not a clear matter. This is due to the lack of a 1:1 correspondence between officers, policing tasks, cameras, and outcomes. We cannot assume that each officer that is assigned a camera consistently activates or fails to activate the device. To capture this variation would require incidents to be nested within officers. Unfortunately, several incidents involve more than a single officer. Thus, we would either need to repeat the same incidents for each officer or use a cross-classified model with as many dimensions as officers to perform the analysis. The former is not appropriate and the latter is difficult to estimate with current technology. Aggregating outcomes to the officer level is not advisable, either, again due to the non-independent nature of the aggregate statistics (officer A's average contains some incidents that are also part of B's average, etc.).

Our solution to this problem is to examine the issue at the incident level. Instead of focusing on the officers, we focus on the incidents and aggregate the behavior of all the officers associated with the incident. That is, our data conditioning produces a single row per incident. Next, because incidents can involve several officers, we ask whether one or more cameras are present and then whether any of those cameras are activated. Thus, officers are not the unit of the analysis per se. Instead, examine whether a camera being present and activated influence the officer and/or citizen behavior related to that incident.

While this methodology solves the problem of repeating events in the data-set, it does not solve the issue of the non-independent observations. For this reason, we estimate the variances of our estimates using boot-strap resampling of the data.

Analysis Data

We linked data from several sources to compile our analysis data-set. As noted above, the unit of analysis was the incident, and recorded in the agency's Call Aided Dispatch/Records Management System (CAD/RMS). Data from the cameras were also coded by incident, and as a consequence, a dichotomous indicator was created that coded whether an officer involved in the incident used a camera (*Video*). The officer(s) assigned to the incident was also known, and so another dichotomous indicator was created to code whether the officers involved in the incident were assigned cameras on a given day (*Assign*). Then, for each incident, outcome data including whether an arrest was made (*Arrest*), whether the officers faced resistance (*Resist*, comprising of flight, passive, or force resistance), and whether the officer(s) received complaints based on the actions related to that incident (*Complaint*). For each incident, we also captured key characteristics as control variables, such as whether the incident involved violence, drugs, disorder, a traffic stop, or suspicious persons. Holding these factors constant in a regression model allowed for a reasonable comparison of the treatment and control areas.

Analytical Approach

The analytical approach for the present study is complicated by the confounding effects of BWC assignment with BWC activation. In cases of perfect compliance with a specified intervention, a simple *t*-test or multiple regression with uncorrelated covariates would suffice to estimate the effect of the program. However, as discussed below, the officers in the Maryvale precinct who were assigned to use the BWCs did not always abide by departmental policy with respect to camera use. Surveys of officers before and after BWCs were implemented found that nearly all of the officers

(99%) were not in favor of their assignment and use in the field. For this reason, analyses that focus only on the effects of assignment of a BWC will produce a smaller effect than one based on perfect compliance. This smaller effect is analogous to the "intent to treat" effect, which is reflective of the average difference between those assigned treatment and those assigned control. However, also of interest is the effect of BWCs on those who activated them. To estimate this quantity, an analogy to a local average treatment effect (LATE, also known as the effect of "treatment on the treated") is employed using an instrumental variable approach to estimating the effect (Angrist, 2006; Angrist, Imbens, & Rubin, 1996; Cameron & Trivedi, 2005). In this analysis, we make the assumption that the choice to assign an area to receive cameras only impacts incident outcomes through the activation of the camera, which is consistent with the assumptions of instrumental variable methods.

Traditionally, this estimator is the ratio of the sample covariances of the outcome, *Y*, and assignment to treatment, *Assign*, and the covariance of employing treatment, *Video*, with assignment to treatment. The IV estimator can be easily computed with the formula from Durbin (1954):

$$\alpha_{IV} = \frac{\sum Y_i Assign_i / \sum Assign_i - \sum Y_i(1 - Assign_i) / \sum (1 - Assign_i)}{\sum Video_i Assign_i / \sum Assign_i - \sum Video_i(1 - Assign_i) / \sum (1 - Assign_i)}.$$

The same estimate of the effect is also possible using a two-stage regression approach. In the first stage, *Video* is regressed onto *Assign*

$$Video_i = \gamma + \lambda Assign_i + v_i$$

and the predicted values of *Video*, $\widehat{Video}_i$, are saved. The second stage includes the predicted values of *Video* in a model that predicts the outcome, *Y*.

$$Y_i = \alpha_0 + \alpha_{IV} \widehat{Video}_i + e_i.$$

In the case where control variables are added to the model, we also follow the common practice of including the controls themselves as instruments to the video camera. This is methodologically sound, but has prima-faci benefits in that officers may choose to employ cameras in part due to the nature of the incident.

In order to estimate the appropriate variances (i.e. standard errors) for the regression model, specialized software is often required to perform instrumental variable analysis. Also, given that our outcome is dichotomous, a generalized liner model is more appropriate in cases of low event rates, especially when employing controls, but for simple models the least squares approach provides unbiased estimates (Angrist, 2006). For our final models, we employed both linear (2SLS) and generalized linear (relative risk) regressions to estimate

the effect of BWC use on likelihood of arrest, complaints, and resistance in the case of arrest. For inference, we employ a bootstrapping technique of estimating the standard errors. This is especially important given the nature of our data where multiple officers might be present at an incident; which happened in 29% of the incidents analyzed.

Because of this, a simple random sample approach to inference is not appropriate, nor is a simple cluster correction for standard errors easily obtainable. As a result, we employ the sub-sampling bootstrapping approach to estimate the standard errors of our effect for use in all of our statistical tests. Bootstrapping is a technique whereby random samples (with replacement) are drawn repeatedly. For each sample, the analysis is performed, and the variance of the results from the samples comprise the standard error. This essentially creates an empirical sampling distribution, not one based on assumptions of independence which in this case are not met.

Each outcome was coded as a dichotomous indicator, and so our generalized models employed a relative-risk estimator. Unlike the typical logistic model, where the link function is the log-odds, $\ln(p/1-p)$, where p is the probability of the outcome being 1, the link function is simply the log of the probability $\ln(p)$. We do this to avoid common misunderstandings of the exponential slopes. In logistic regression, the exponent of the slope is the odds ratio, however, many readers of such effects consider this to be a ratio of the likelihood (probability) of a positive outcome. This can lead to misunderstandings—typically overstatements of the magnitude of the effect. Instead, the exponent of our slopes is the rate-ratio, which literally is the ratio of the chances was used to increase the interpretability of our findings. Our generalized models also require the instrumental variable approach. To achieve this, we employed the QVF package available for Stata (Hardin, Schmiediche, & Carroll, 2003) that allows for instrumental variables. In addition, this package also allows for bootstrapped standard errors for inference.

For each outcome, we estimated two different models using a generalized linear estimation techniques (for a total of four regressions). First, we estimated a simple intent to treat (ITT) model that enters treatment assignment into the predictive model (Model 1). This estimates the effect of being assigned a camera, which includes a mix of those who used and did not use the device. The second model (Model 2) is also an ITT analogy model with controls for incident and time of year controls (quarters). Next, we estimated the analogy of a LATE using camera use instrumented by assignment as the predictor (Model 3). Finally, the LATE analogy model is again estimated with controls both in the second stage regression model as well as instruments (Model 4). These controls include a calendar time set of dummies for quarters 2, 3 and 4, and separate variables for whether the incident was classified as violent, drug, property, or traffic stop (other types of incidents are the reference group).

Results

Descriptive Results

Table 2 presents descriptive univariate statistics for variables employed in the analysis. The first column represents the control group and the second column is the treatment group. Examining the percentage of various types of incidents, we see that the treatment and control groups are generally comparable. The largest difference is that the control group's incidents involved property crime 18% of the time while the treatment groups incidents property crime 21% of the time.

Incidents with video data also differed, as expected. The control group's incidents included video data about 1.6% of the time, compared to the treatment group that had video data in 32.1% of incidents. Turning to the outcome variables, complaints in the control group occurred in .8% of the incidents, which is more than double the treatment group, which only recorded complaints in .3% of incidents. Arrests, however, were comparable with each group reporting arrests for about 3% of the incidents. For the incidents in which an arrest occurred, resistance of any kind occurred more often in the control group (8.2%) compared to the treatment group (7.4%).

Thus, based on the descriptive results, it appears that the control group experienced more complaints during the experimental period whereas the treatment group experienced more complaints during the pretest observation period. However, these results are only descriptive, and a specific effect of actually employing the BWCs can only be estimated using the IV regression approach. We now turn to the results of these models.

Table 2 Description of incidents used in the analysis[a]

	Control	Treatment
Incidents	22,720	21,660
Incident involving		
Violence	15.8%	15.9%
Drugs	1.2%	1.1%
Property	18.5%	21.3%
Traffic stops	24.8%	23.0%
Video data	1.6%	32.1%
Complaint	.8%	.3%
Arrests		
Arrests	631 (2.8%)	566 (2.6%)
Any resistance if an arrest	8.2%	7.4%

[a]Comprising the dates of April 1 2013—March 31 2014.

Results of the Analysis

Tables 3–5 present the results from the generalized regression models predicting likelihood of each outcome using the 4 models outlined above. The outcomes include arrest (Table 3), complaints (Table 4), and any resistance (Table 5). For each model, the controls were centered on their means to preserve the intercept's interpretation as the control group average; all else being equal. For each table, bootstrapped standard errors and the results of normal (z distribution) hypothesis tests are presented.

Table 3 presents the results of the models predicting an arrest. As expected, violent, drug, and property incidents are more likely to lead to arrest. Yet, no other effects, including those of BWCs are evident in the generalized linear models.

Table 4 presents the results of the model predicting whether an incident is associated with a complaint. We find significant effects in this model. Model 1 is the simple generalized relative-risk models, where the treatment effect is

Table 3 Regression models predicting the likelihood of arrest as a function of treatment conditions

	Generalized linear models (relative risk)			
	Model 1	Model 2	Model 3	Model 4
Treatment assigned	−.061	−.055		
	(.054)	(.055)		
Video (instrumented)			−.200	−.180
			(.178)	(.182)
Violent incident		1.740*		1.760*
		(.099)		(.100)
Drug incident		2.282*		2.283*
		(.142)		(.143)
Property incident		.831*		.833*
		(.109)		(.109)
Traffic stop incident		1.156*		1.158*
		(.093)		(.092)
Second quarter		−.038		−.014
		(.162)		(.163)
Third quarter		−.043		−.034
		(.154)		(.154)
Fourth quarter		−.002		.004
		(.163)		(.163)
Intercept	−3.584*	−3.808*	−3.580*	−3.805*
	(.038)	(.044)	(.040)	(.046)

Notes. N = 44,380 incidents. Control variables centered on the grand mean. Bootsrapped standard errors in parentheses.
*p < .001.

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Table 4 Regression models predicting the likelihood of a complaint as a function of treatment conditions

	Generalized linear models (relative risk)			
	Model 1	Model 2	Model 3	Model 4
Treatment assigned	−.958**	−.957**		
	(.116)	(.115)		
Video (instrumented)			−3.142**	−3.142**
			(.381)	(.377)
Violent incident		.325*		.674**
		(.165)		(.154)
Drug incident		.796*		.809*
		(.401)		(.379)
Property incident		−.047		−.014
		(.188)		(.205)
Traffic stop incident		.019		.056
		(.168)		(.170)
Second quarter		6.147**		6.565**
		(.653)		(.676)
Third quarter		6.113**		6.267**
		(.674)		(.684)
Fourth quarter		6.275**		6.379**
		(.672)		(.684)
Intercept	−4.866**	−5.067**	−4.815**	−5.015**
	(.063)	(.074)	(.067)	(.077)

Notes. N = 44,380 incidents. Control variables centered on the grand mean. Bootsrapped standard errors in parentheses.
**p < .001.

−.96 in Models 1 and 2, which can be interpreted as a rate ratio of .38, or a 62% reduction in the chance of a complaint. This effect increases to −3.14 in the instrumental variable analysis, which translates into a rate ratio of .04; or a 96% reduction in complaints. Effectively, these models suggest that if BWCs are employed as prescribed, a majority of complaints against officers would be eliminated. Table 5 presents the results for predicting any type of resistance (which includes flight, passive, and force resistance), and here we do not find any significant effects.

Discussion

Public focus on police interactions has increased with recent public events across the country. BWCs are quickly being considered one solution to these negative police interactions with the public. However, to date, relatively little rigorous empirical analyses has been employed to test these hypotheses. Using

Table 5 Regression models predicting the likelihood of any resistance as a function of treatment conditions

	Generalized linear models (relative risk)			
	Model 1	Model 2	Model 3	Model 4
Treatment assigned	−.105	−.159		
	(.180)	(.187)		
Video (instrumented)			−.177	−.267
			(.306)	(.315)
Violent incident		−.380		−.366
		(.283)		(.281)
Drug incident		−.931		−.970
		(2.971)		(2.967)
Property incident		−.892*		−.885*
		(.406)		(.406)
Traffic stop incident		.182		.163
		(.273)		(.272)
Second quarter		.075		.120
		(3.836)		(3.839)
Third quarter		.539		.577
		(3.870)		(3.868)
Fourth quarter		.663		.705
		(3.848)		(3.849)
Intercept	−2.496**	−2.512**	−2.481**	−2.490**
	(.116)	(.232)	(.132)	(.241)

Notes. $N = 1197$ incidents. Control variables centered on the grand mean. Bootsrapped standard errors in parentheses.
*$p < .05$; **$p < .001$.

data obtained from 44,380 incidents (1197 resulting in an arrest) in Phoenix, Arizona, the current study examined the impact of police worn body cameras on police-suspect behavior. Our study adds to this body of literature by acknowledging that while officers may be assigned to wear a body camera, the impact of their activation (not just presence) in the field is unknown. We used an instrumental variable approach to provide reasonable estimates of the effectiveness of cameras as they are *meant* to be used. Of course, fidelity of implementation will not be consistent, and our findings suggest that compliance is a vital component to any BWC program. However, our estimate of treatment on the treated provides a useful upper bound to the potential effects that adds to the public discussion of BWCs. We believe that this approach is particularly important in the early adoption phase of BWCs. As mentioned above, agencies across the country are adopting BWCs, but little discussion has focused on their actual use in the field.

Our findings suggest that the mere presence of a BWC has an impact on the number of complaints made against an officer, with the likelihood of a

complaint being reduced by about 62% (1 − exp (−.957) = .62) when a BWC was present. This suggests that officers, and those who they interact with, behave at least sometimes differently when a camera is present. This could be the result of the officer and/or citizen being more cognizant of their behavior, regardless of whether the BWC has been activated. Poyner (1988), for example, reported that both active and dummy cameras in school busses significantly reduced crime due to the passengers being unaware of those cameras that were active and not active. The mere presence of the cameras, in other words, deterred misconduct because of the perceived increased likelihood that misconduct would be observed and result in punishment. Although not directly examined in the present study, citizens, for example, might have been deterred from engaging in certain actions that they perceive being recorded. Officers likewise might have been cognitively aware that if the incident escalated to a problem, and the officer had not activate their camera, the officer could be disciplined. Thus the officer might be deterred from engaging in actions that could escalate the situation to a problem. Then again, it could be that citizens perceive their interactions with an officer differently when they observe the presence of a BWC. Citizens, for instance, might consider the actions of police officers who wear a BWC as a more legitimate due to their perception that they will receive fair treatment by officers who are wearing a body-worn camera. Much further research is needed to examine the possible reasons complaints decline even when a BWC has not been activated.

Our findings also indicated, as noted above, BWC activation was relatively limited, with cameras only being activated in about 32% of incidents. Analysis of our data indicated that officers were more likely to activate their BWC in some incidents more than others. For example, a BWC was activated in about 47% of incidents involving domestic violence, about 39% of incidents involving violent offenses, 26.5% of incidents involving property crime offenses, and 6.5% of traffic offenses (findings not shown in table). These low compliance rates resulted in more complaints than otherwise might have occurred—as our findings suggested that if officers who were assigned a BWC would have complied with department policy there would have been an estimated 96% reduction in the number of complaints. As agencies begin to institutionalize BWC use, and officers embrace the technology, our findings indicate that the potential impact on complaints against the police might be more substantial than previously reported.

Our finding of frequent non-compliance is not unusual. For example, when Miranda warnings were first required by the Supreme Court to be read to suspects, early research on the subject suggested that many police officers did not abide. It took years for the judicial ruling to become rooted and normalized within police operations (Leo, 2008). This issue was discussed at length by Musheno (1980) who pointed out that the availability of an intervention (e.g. a new policy, law, technology) might be widely available, but that actual implementation does not take place until the intervention has been enculturated and institutionalized. As a consequence, further research is needed on

the factors that foster "officer buy-in" and increase compliance. Some early research suggests that officer support and opposition to the implementation of BWCs may vary widely across agencies (Jennings et al., 2014; White, 2014) and that varying strategies might need to be employed in the implementation of BWC based on local circumstances and culture.

A recently study by Young and Ready (2015), examined BWC integration in a southwestern police department and the importance of social networks relative to first hand experiences with BWC. They reported that those who were more embedded in a work group that viewed BWCs as legitimate (aka. positive) were slightly but significantly more likely to report using the device; and that this peer effect was stronger than the impact of the officer's fist hand experiences with the device. They posited that it might be useful to initiate BWCs with police officers who "endorse and volunteer" to use the device to reduce cultural barriers and increase levels of compliance.

Drawing on Young and Ready's (2015) work our findings of non-compliance could be attributed to officer culture and perceptions of organizational justice within the PPD. Officers who were assigned and required to wear the BWC might have been concerned that video obtained during a police citizen contact could be used unfairly against them. In findings not reported here (references omitted for review purposes) self-report data collected from the study officers indicated that only a minority of the officers (30%) wanted the BWC to be deployed elsewhere in the PPD, and less than 1% of officers reported that BWC were received well by their co-workers.[5] As a consequence, our findings of low compliance might be the result of a combination of high levels of resistance to the implementation of BWCs and the technology not being viewed as a legitimate means of police accountability. Our findings further suggest that additional research is needed on how compliance rates might differ between those who volunteer to wear BWCs and those who are required to where them, and how perceptions of organizational justice in the implementation of BWC might be important to successful field implementation.

We also found that BWCs did not influence the number of arrests made by officers. Officers have speculated that wearing a BWC might reduce the number of arrests they make for concern that the video captured through the BWC might be used against them if they were to make a mistake in the field (Reference omitted for review purposes). Our findings, however, did not support this concern; and are similar to those recently reported by Ready and Young (2015) who found that the number of self-reported arrests made by an officer did not change with the assignment of BWCs. While we can only speculate as to why officer arrest behavior did not change, we suspect that BWCs did not effect what the officers do as much as how they do it. There is a long

5. It is worth noting that in Phoenix, where wearing BWCs was mandatory among the treatment group, officer self-reported attitudes about BWC were substantially more negative than officers in Orlando and Mesa where BWC were worn voluntarily by the treatment group.

and strong culture within policing that emphasizes the importance of arrest (Kelling & Moore, 1989). The presence of BWCs might have resulted in officers emphasizing elements of procedural justice during incidents, but remaining focused on obtaining the same outcome they otherwise would have before the implementation of BWCs. These findings provide an optimistic view of BWCs: reducing police misbehavior but not altering officer performance, as measured through arrest.

Last, we examined the impact of BWCs on suspect behavior through the examination of resisting arrest. Our analysis showed that the presence and activation of BWCs did not have a significant impact on resisting arrest by the suspect. Our finding is somewhat contrary to that of Jennings et al. who reported that response to resistance declined significantly for both BWC and non-BWC officers, but more so for BWC officers. Further research is needed to determine the impact of BWC on suspect behavior, including but not limited to whether the suspect observes and recognizes the presence of the officer worn body camera; and if they do, how that might impact suspect behavior. Future research should also evaluate the impact of various BWC notification techniques (e.g. color of camera, verbal notification) on suspect behavior. This research could be conducted, for example, through surveys of persons who have been contacted by an officer in the field who was wearing a BWC, or though post apprehension interviews with suspect's pre and post BWC implementation.

Our findings have a number of important policy implications related to BWC assignment, activation and compliance. First, our findings first suggest that as police agencies implement BWCs in the field, they might experience an immediate decline in complaints against the police simply from officer's wearing and occasionally using the technology. The Department of Justice recently funded the Body Worn Camera Pilot Implementation Project that will result in more than 55,000 BWC being purchased and deployed in more than 70 police agencies across the country. Our findings suggest that this program may rapidly alter the police-community relationship in many communities across the country.

Second, our findings suggest that as police organizations implement BWCs in the field they should not assume that officers will necessarily comply with departmental policy in their activation; and that policy compliance is important if agencies and their officers are to reap the full benefit of BWCs. In Phoenix officers only activated their BWC 32% of the time (Table 2) when they were required by departmental policy. Agencies that have mandatory activation policies might need to develop mechanisms that monitor officer compliance. These policies could range from a supervisor at the end of an officer's shift reviewing CAD/RMS data and ensuring that the officer submitted captured video, or policies that require a unit to randomly audit or inspect officer compliance. The impact of various forms of supervisory and disciplinary practices related to non-compliance need to be examined and will require much further research to determine their positive and negative impact on BWC use.

Third, our findings suggest that policymakers should have a stronger understanding of the strengths and weaknesses of various policies requiring BWC activation during contact with the public; and the impact of these policies on more general issues related to police performance, public expectations of BWC use, and costs vs. benefits. An analysis presented in Slate.com by Ready and Young (2015) reported that one agency's change from a mandatory activation policy to a discretionary activation policy resulted in a 42% decrease in self-reported activations. They concluded that on the one hand a discretionary activation policy might result in critics questioning why BWCs were sometimes not activated, which could intensify conflict within a community; but on the other hand they posited that a mandatory activation policy might result in increased privacy violations, the needless recording of non-serious crimes, and result in officers being more risk adverse and less productive. Our findings of fairly regular non-compliance with BWC policy suggests that the creation of policies alone will not address these important issues, but rather close attention to policy compliance is required to fully understand the impact of particular policies on particular outcomes.

Limitations

While the present study builds on the strengths of prior research on BWCs, our results are tempered by at least three limitations. The first limitation is the method of organizing the observations. On one hand, the assignment to cameras varied only by two clusters (Areas 81 and 82). Thus, our assignment to treatment is confounded with the areas assigned. Given the similarities in structure, the statistical controls, and the unit of analysis as the incident, we attempt to resolve this issue to maintain interval validity, but confirmation is difficult. Communities with varying levels of criminality, a different relationship between the police and public, or distinctive police subculture might yield different results, and so external validity may be violated. The second limitation, which was also noted by Ariel and colleagues, is whether we violated the stable unit treatment value assumption. This assumption requires that assignment to treatment for any one incident does not impact the outcomes of other incidents. For example, the fact that a robbery occurs in the treatment area that does not result in an arrest has no bearing on whether a traffic stop in the same area results in an arrest. This issue is somewhat mitigated by our project design, namely that the entire squad was randomized. However, in situations for which officers from both squads responded to a call, it is unlikely that this assumption was met. Post hoc analysis (tables not shown) indicated that about .7% of incidents involved officers from a mix of the treatment and control squads.[6] Thus, our overall

6. It should be noted that PPD data only contained the names of up to two officers who were at the incident. Therefore, if an incident resulted in three or more officers at the scene, we would not be able to capture information on whether they were wearing a BWC.

results may be overly optimistic in their statistical significance, but this was addressed in part by bootstrapping the standard errors. The third limitation is that the study was limited to one large police agency. A number of factors might influence compliance rates, including but not limited to local police culture (Jennings et al., 2015; Ready & Young, 2015), BWC policy attributes, and officer perceptions of organizational justice.

Future Designs for BWC Evaluations

It is clear from all evaluations that so far that few attempts to ascertain a causal effect have been successful. The ideal design would be one of two possibilities. The first would be a true cluster randomized trial whereby entire organizational clusters are assigned to treatment or control randomly. As we have shown here, it is imperative to keep track of which officers activate cameras so an "intent to treat" and "treatment on the treated" effects can be estimated. A cluster randomized trial may be infeasible, however, because of the high sample demands (i.e. many clusters would be needed) of statistical power for generalized outcomes such as a dichotomous measure of arrest or complaint. Another possibility would be a block-randomized design where officers within organizational clusters are assigned to treatment or control, making the unit of analysis the officer. This will aid power, but again leads to issues of contamination where an incident involves an officer with and an officer without a camera. We recommend specific research agendas to better understand the research designs necessary to estimate causal effects.

In the end, officer worn body cameras present a unique opportunity that benefit both the police and public. Our results indicate that complaints against the police are reduced when BWCs are present but are much more effective when the cameras are activated. Little is known about police officer compliance with BWC policy but our findings suggest that there is much room for improvement. Gains in compliance should increase positive interactions between police and the public they serve. We note that these effects are non-trivial and have the potential to produce a major shift in the relationship between the police and public. We also found that BWCs do not necessarily reduce officer productivity as measured through arrest, meaning that it is plausible for the nature of the interactions between police officers and the public to change for the better, but not the number or type of encounters.

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