

Evaluating Compliance and Outcomes associated with Body Worn Camera Implementation in the Phoenix Police Department: A Technical Report

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## Introduction

The Bureau of Justice Assistance (BJA) awarded funding to the Phoenix Police Department (PPD) to purchase, deploy and evaluate on-person video cameras that record the interactions between the public 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 the police to document statements, observations, behaviors, and other evidence.

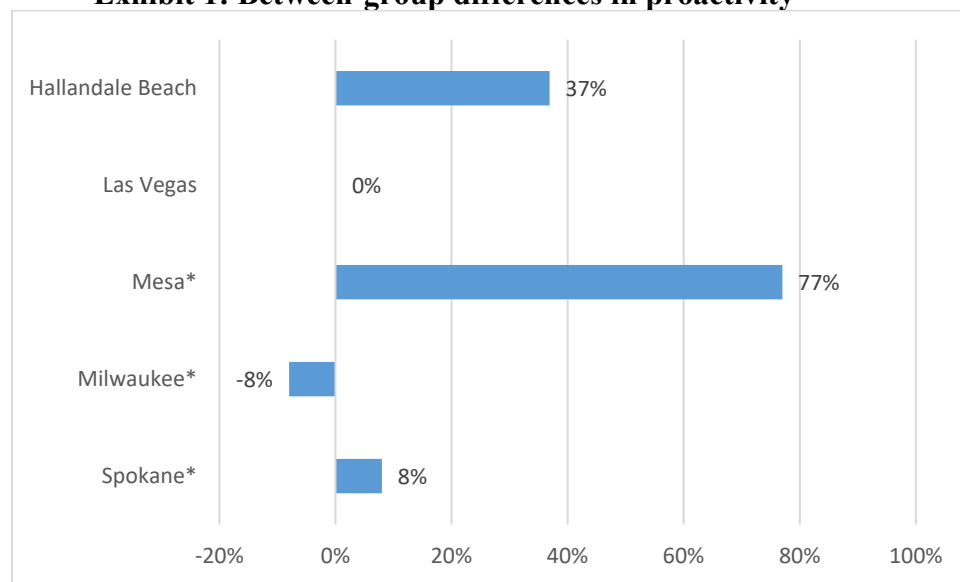
The purpose of the present study thus examined the effect of the body worn camera technology in four principal areas. The first is on BWC activation compliance. We seek to understand overall compliance rates, how compliance might vary over time, and whether method of BWC assignment is related to compliance rates. The second purpose of the evaluation is to examine the impact of BWCs on officer passivity. We focus on whether the assignment of BWCs changes officers' behavior with respect to such issues as officer initiated stops, number of responses to dispatched calls, self-initiated activities, response time, and arrest. The third purpose of the evaluation is to examine the impact of BWCs on issues related to accountability. Specifically, we examine the impact of BWCs on complaints against the police and officer use of force. The final purpose of the evaluation was to examine officer attitudes toward BWCs over time. We surveyed a sample of PPD officers regarding their perceptions of the use and utility of BWCs, in addition to their perceptions of organizational justice. Findings from that section of the study are presented separately in another report. In the next section, prior to discussing our methodology and findings, we discuss what is currently known about BWCs and their impact on officer productivity, use of force, and complaints.

## Literature Review

Many have suggested that BWCs will change the way officers behave. Some BWC proponents suggest that cameras will make officers more proactive because they can be used to record evidence of the suspicious activities an officer sees (Ready & Young, 2015). Others have raised concerns that BWCs could result in officers disengaging from their jobs due to the potential for increased scrutiny of their actions as a result of BWC footage (Wallace, White, Gaub, & Todak, 2018). This issue has been referred to as BWC induced passivity. One strategy that has been used to examine officer induced passivity has been to examine the impact of BWCs on officer responses to dispatched calls. One study, conducted in Las Vegas (NV), however, found no differences in the number of dispatched calls officers responded to between treatment (those with BWCs) and control officers (those without BWCs) (Braga, Sousa, Coldren, & Rodriguez, 2018). More research attention has been paid to whether BWCs influence officer proactivity, though findings are inconsistent across studies. Researchers in Mesa (AZ) and Spokane (WA) found that BWC officers engaged in significantly higher levels of proactive activity than control officers (Ready & Young, 2015; Wallace et al., 2018). Similarly, officers in Hallandale Beach (FL) conducted a higher number of field contacts after being assigned to wear a BWC, though this increase was not significantly different compared to control officers (Headley, Guerette, & Shariati, 2017). In contrast, a study in Milwaukee (WI) found that BWC

officers engaged in significantly fewer subject stops, compared to control officers (Peterson, Yu, La Vigne, & Lawrence, 2018). Finally, researchers in Las Vegas (NV) and Tempe (AZ) found no relationship between BWCs and officer proactivity (Braga et al., 2018; White et al., 2018). The way BWCs were assigned to officers varies across prior research. Some studies randomly assign BWCs to officers who volunteer to wear a camera (e.g., Las Vegas) and others randomly select officers and mandate them to wear BWCs (e.g., Milwaukee). Notably, the way BWCs were assigned to officers does not seem to be associated with outcomes that have been used to measure BWC induced passivity . As shown below studies that mandated officers to wear BWCs resulted in decreased proactivity in some agencies (Milwaukee), increased proactivity in other agencies (Spokane), and no change (Tempe).

**Exhibit 1: Between-group differences in proactivity**



\*indicates significant difference between BWC and non-BWC officers

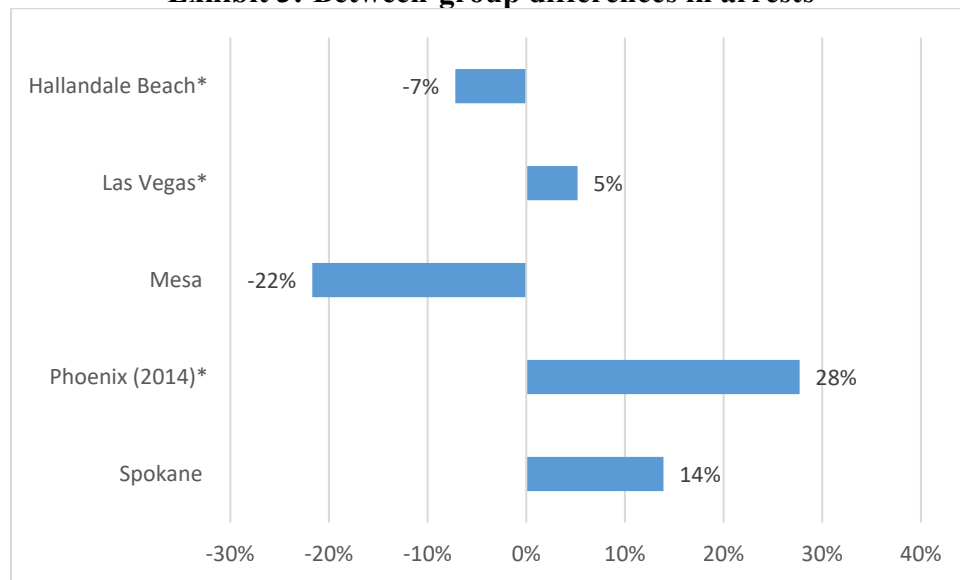
**Exhibit 2: Impact of BWC assignment on proactivity**

	<b>Proactivity Declined</b>	<b>Proactivity Increased</b>	<b>No change in Proactivity</b>
Mandated	Milwaukee	Spokane	Tempe
Volunteers			Las Vegas
Volunteers & mandated		Hallandale Beach Mesa	

Two other measures used to examine BWC induced passivity is response time and arrests. The impact of BWCs on officer response time, however, has received limited research attention to date. In a study in Spokane (WA), Wallace et al. (2018) found no significant differences in response time between officers mandated to wear BWCs and control officers. Like self-initiated activities, researchers examining the impact of BWCs on arrest have similarly uncovered mixed findings (Exhibit 3). In both Las Vegas (NV) and Phoenix (AZ) officers

assigned to wear a BWC conducted a significantly greater number of arrests than officers assigned to the control group (Braga et al., 2018; Katz, Choate, Ready, & Nuno, 2014). These findings are supported in agencies outside of the US as well, as officers wearing BWCs in the Plymouth Constabulary (England) and the Toronto Police Service (Canada) conducted more arrests than officers who were not using BWCs (Goodall, 2007; Whynot, Nykorchuk, Zisis, & Deane, 2016). Although, researchers in Hallandale Beach (FL) found that officers conducted significantly fewer arrests after being assigned to wear a BWC (Headley et al., 2017). In one southwestern agency and in Milwaukee, researchers identified general reductions in arrests after the implementation of BWCs (McClure et al., 2017; Peterson et al., 2018). Several studies have identified no relationship between BWCs and arrests (Grossmith et al., 2015; Hedberg, Katz, & Choate, 2017; Ready & Young, 2015; Wallace et al., 2018; Yokum, Ravishankar, & Coppock, 2017). As shown in Exhibit 4, the manner in which BWCs were assigned to officers does not appear to explain the different outcomes across findings. For instance, studies involving officers who volunteered to participate resulted in decreased arrests in one Southwestern US police department but increased arrests in the Las Vegas Metropolitan Police Department.

**Exhibit 3: Between-group differences in arrests**



\*indicates significant difference between BWC and non-BWC officers

**Exhibit 4: Impact of BWC assignment on arrests**

	Decrease in arrest	Increase in arrest	No change in arrest
Mandated	Milwaukee	Phoenix (2014)	London

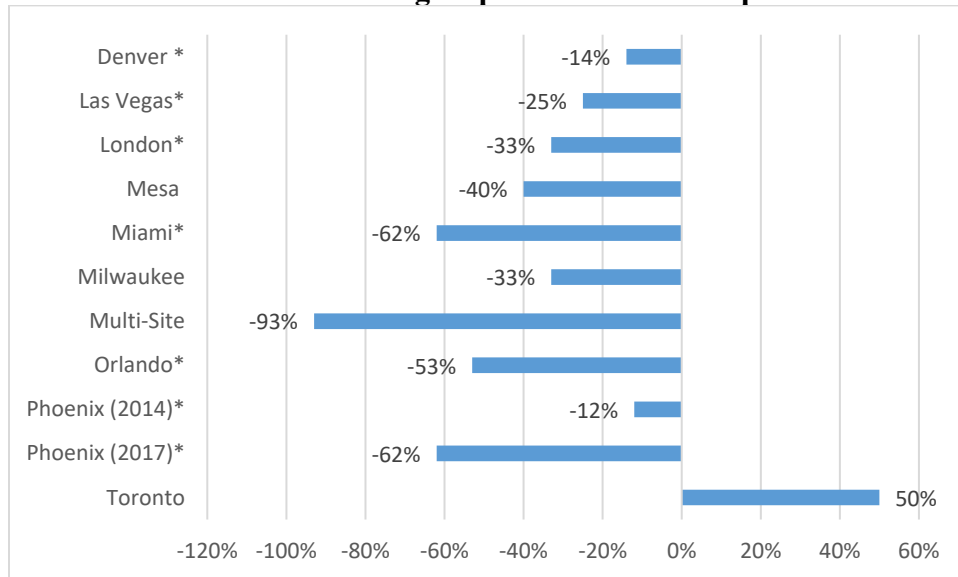
		Plymouth	Wash D.C.
			Phoenix (2017)
			Spokane
Volunteers	Southwestern US	Las Vegas	
Volunteers & mandated	Hallandale Beach		Mesa

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The rapid expansion of BWCs in the US has sometimes been attributed to a ‘crisis in policing’. In response to this crisis, the President’s Task Force on 21<sup>st</sup> Century Policing promoted the use of BWCs to increase police transparency and accountability (*Final Report of the President’s Task Force on 21st Century Policing*, 2015). The adoption of BWCs in response to this recommendation (often supported by federal funding) has led to a large body of work examining whether BWCs can reduce the number of complaints citizens file against police officers and/or the number of use of force incidents that the police engage in.

As shown in Exhibit 5, most of the research studying the impact of BWCs on complaints have found BWCs either reduce or have no impact on citizen complaints against officers. Significant reductions in citizen complaints have been found for officers who wear BWCs in Denver (CO), Las Vegas (NV), London (England), Miami (FL), Orlando (FL), and Phoenix (AZ), relative to their control officer counterparts (Ariel, 2017; Braga et al., 2018; Chin-Quee, 2018; Grossmith et al., 2015; Hedberg et al., 2017; Jennings, Lynch, & Fridell, 2015). Several studies also found that complaints against BWC officers decreased at the same time complaints against control officers were increasing, including researchers in Arlington (TX), (Goodison & Wilson, 2017), Milwaukee (WI) (Peterson et al., 2018), Phoenix (AZ) (Katz et al., 2014), and Spokane (WA) (White, Gaub, & Todak, 2017). The only study that has identified an increase in complaints against officers wearing BWC was in Toronto, though the increase was not statistically significant (Whynot et al., 2016). As shown in Exhibit 6, the way BWCs were assigned to officers does not appear to consistently effect the results of these studies, as reductions were identified for both officers mandated to wear BWCs and those who volunteered to wear BWCs.

**Exhibit 5: Between-group difference in complaints**



\*indicates significant difference between BWC and non-BWC officers  
 Note: all results taken from White et al. (2019). Impact of BWCs on Citizen Complaints: Directory of Outcomes

**Exhibit 6: Impact of BWC assignment on complaints**

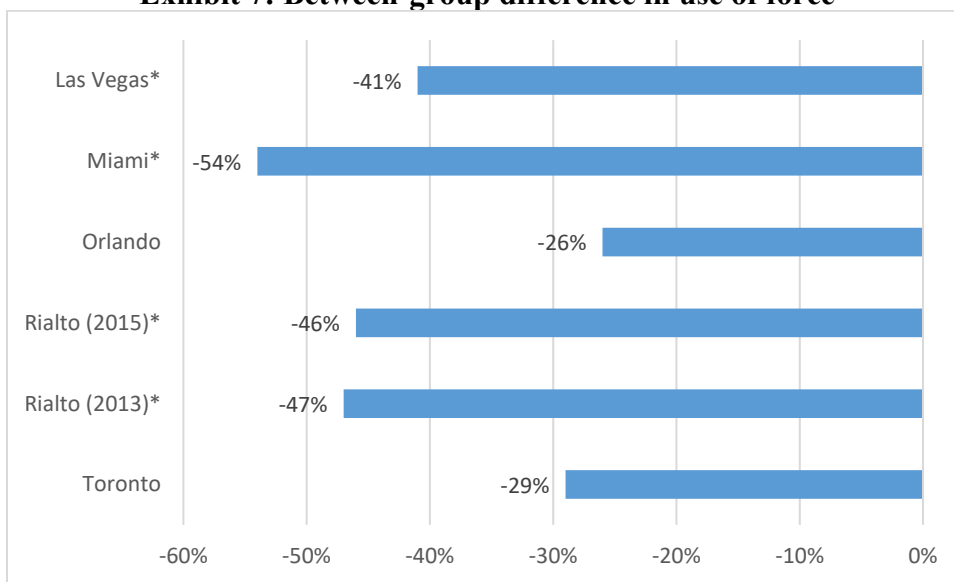
	Complaints declined	Complaints increased	No change in complaints
Mandated	Denver Isle of Wight London Miami Milwaukee Orlando Phoenix Plymouth Republic of Uruguay Rialto Spokane		Boston DC Edmonton
Volunteers	Arlington Las Vegas		
Volunteers & mandated	Mesa		Hallandale Beach

Note: all results taken from White et al. (2019). Impact of BWCs on Citizen Complaints: Directory of Outcomes. BWC assignment method collected by the authors of this report.

Like complaints, most researchers have found that BWCs either reduce use of force, or do not significantly influence use of force. As shown in Exhibit 7, researchers in Rialto (CA), Las Vegas (NV), and Miami (FL) have found that officers wearing BWCs use force significantly

less often than control officers (Ariel, Farrar, & Sutherland, 2015; Braga et al., 2018; Chin-Quee, 2018). BWCs did not impact officer use of force in Denver (CO), Hallandale Beach (FL), Birmingham (UK), Milwaukee (WI), Edmonton (Canada), or Washington DC (Ariel, 2017; Headley et al., 2017; Henstock & Ariel, 2017; Peterson et al., 2018; Stratton, Clissold, & Tuson, 2015; Yokum et al., 2017). As shown in Exhibit 8, the way BWCs were assigned to officers does not seem to impact the relationship between BWCs and officer use of force. For instance, officers who were mandated to wear BWCs and those who volunteered to wear BWCs in separate studies experienced similar reductions in use of force, relative to control officers.

**Exhibit 7: Between-group difference in use of force**



\*indicates significant difference between BWC and non-BWC officers  
 Note: all results taken from White et al. (2019). Impacts of BWCs on Use of Force: Directory of Outcomes

**Exhibit 8: Impact of BWC assignment on use of force**

	Decline in use of force	Increase in use of force	No change in use of force
Mandated	Miami Orlando Rialto Spokane		Birmingham Boston DC Denver Edmonton Milwaukee
Volunteers	Las Vegas Tampa		
Volunteers and mandated			Hallandale Beach

Note: all results taken from White et al. (2019). Impacts of BWCs on Use of Force: Directory of Outcomes. BWC assignment method collected by the authors of this report.

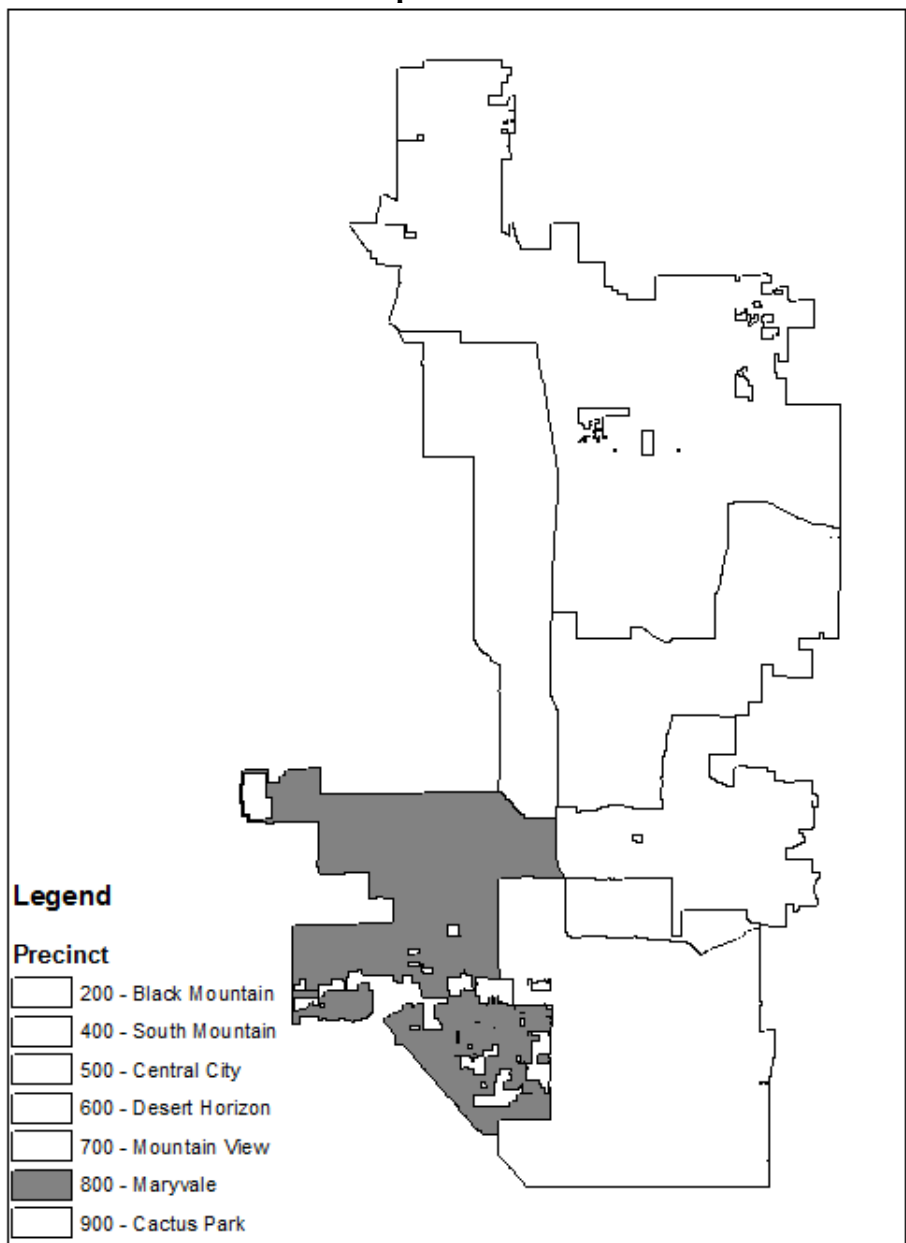
## The Setting

The city of Phoenix is the capital of Arizona and is bordered by the cities of Glendale, Scottsdale, Avondale, Peoria, Paradise Valley, Cave Creek, Tolleson, Chandler, and Tempe. Phoenix is located in the center of the Phoenix metropolitan area, which is comprised of more than 4.8 million people. The city of Phoenix is one of the fastest growing cities in the United States, with a population close to 1.7 million; making it the fifth largest city in the U.S. According to a current census estimate, the city's population is growing at about 25,000 residents a year. The city is primarily comprised of White (43.3%), Hispanic (42.5%), and African American (6.9%) residents. About 20% of residents are foreign born, and 37.3% of residents speak a language other than English at home. The median income of residents is about \$52,000 and 20.9% of residents live below the poverty line. The crime rate in Phoenix has remained fairly stable over the past several years. In 2018, the UCR crime index for Phoenix was approximately 43.6 crimes per 1,000 residents.

The Phoenix Police Department (PPD) has grown by roughly 15% over the past 18 years. It is currently staffed with about 3,000 authorized sworn officers and more than 1,000 civilian personnel. It is the ninth largest municipal police department in country. 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 seven precincts, shown in Exhibit 9.



**Exhibit 9: Phoenix Police Department Precinct Boundaries**



The Phoenix Police Department (PPD) has been on the forefront of BWC technology. In 2013, the PPD was the first agency in the United States to be sponsored by the Bureau of Justice Assistance (BJA) to pilot test BWCs. In that study, BWCs were evaluated in Maryvale, one of the seven patrol precincts. Several key findings were produced through the quasi-experimental evaluation of their program:

- Only 13.2 to 42.2 percent of incidents that involved an officer who was assigned a BWC activated the camera;
- Officer productivity as measured through the number of arrests increased significantly;
- Complaints against the police who wore a BWC declined by 23% compared to a 10.6% increase among comparison officers; and
- Those officers who wore cameras and received a complaint were significantly less likely to have the complaint sustained when compared to the comparison group.

Consequently, the PPD has been at the forefront of BWC implementation. PPD was asked to participate in a White House conference on BWC implementation, and its work influenced much of the content used to populate the BWC Toolkit <https://www.bja.gov/bwc/>. Materials created as part of the above pilot project were also used to produce training materials included in the BWC training guide that is used by police agencies nationwide to train officers in the use of BWCs. In 2016, PPD received additional BJA funding to further test the effects of BWCs through a randomized control trial, and this document serves as this projects final report.

### Intervention Design

The present study relies on a sample of PPD officers who participated in a study on the effectiveness of BWCs. A total of 841 officers assigned to patrol units in six of the seven PPD precincts were eligible to participate in the study. Patrol officers assigned to one precinct (Maryvale) were excluded from the study because it served as the location of the BWC pilot test, which is noted above.

Of the 841 eligible officers, 668 officers were approached and asked to participate in the study. Contact was not made with the remaining officers (n=173) due to absences and temporary reassignments (vacation, sick, training, leave, light duty, etc.). Up to three attempts were made to contact officers who were absent or temporarily reassigned. Participation in the study was voluntary. Of the 668 approached officers, 467 gave preliminary consent to participate in the study, resulting in a 70% participation rate for officers who were present at the time of the request.

The research team randomly selected officers to wear a BWC from the pool of 467 officers who provided preliminary consent. Randomly selected officers who declined to volunteer to wear a BWC were replaced by another randomly selected officer who was assigned to the same precinct. Forty-seven officers who were randomly selected and asked to wear a BWC volunteered to do so (hereafter referred to as “volunteers”). Ninety-six officers who were randomly selected and asked to wear a BWC refused to do so and were not assigned a BWC (hereafter referred to as “resistors”). The PPD elected to mandate officers to wear the remaining BWCs due to grant related time constraints. For the purposes of this study, thirty-four BWCs were randomly assigned to officers who were mandated to wear them, (hereafter referred to as “mandated”). The remaining eight BWCs were assigned in violation of study protocol to officers non-randomly selected by their precinct commanders. Those officers who were non-randomly selected and assigned to wear a BWC by their commander are excluded from the officer activities analysis, though they are included in the activation compliance analysis. The remaining

281 officers who were not asked or assigned to wear a BWC serve as the control group (hereafter referred to as “control”). Due to officer assignment changes over the course of the study, fourteen control officers and two resistors were assigned to wear a BWC at some point during the study period. These officers were removed from the study to eliminate potential contamination effects. An additional four control officers and two resistant officers were removed due to missing data on key study variables (e.g., educational attainment, response time). The final sample used in these analyses included: 47 officers who volunteered to wear BWCs, 34 officers who were mandated to wear a BWC, 92 officers who resisted wearing a BWC, and 263 officers who were assigned to the control group. See Appendix A for the intervention design. BWCs were assigned to PPD officers and deployed starting May 24, 2017.

## Data

The present study relied on BWC metadata generated by camera activation, official police computer-aided dispatch (CAD) data, official arrest data, official use of force reports, and citizen complaints reported to the PPD. Each of the data sources used for the present study is discussed below.

**CAD data.** CAD data from November 24, 2015 through November 23, 2018 were obtained from the PPD for the purpose of evaluating the project. Data included all unique incident reports from the department to identify crime and disorder events for 18-months pre and 18-months post camera implementation, for treatment and control group officers. These data included officer activity logs, which are obtained through dispatch records when officers report status changes. These data were provided in their original form as both incident-based and officer-based, and were converted for analysis within our research design focusing on pre-post deployment and treatment and control group assignment.

These data were used to analyze camera activation compliance by matching police activity with the camera meta-data (described below) and calculating the ratio of the number of incidents to the number of BWC records. These data included 998,524 incident entries for the treatment and control group officers.

The CAD data included records of all dispatched calls for service, response times, and officer-initiated stops during the evaluation period. The original data contained information on 836,040 dispatched calls for service, response times for 808,106 events, as measured in minutes, and 162,447 officer initiated stops. The data included officer(s) serial numbers and the date and time of event.

**Official arrest data.** Official arrest data from November 24, 2015 through November 23, 2018 were obtained from the PPD to identify those incidents that resulted in either a citation or an arrest. These data include the incident number and the arresting officers serial number for 261,696 arrests. These data were merged with the CAD data using the incident number.

**Camera metadata.** Camera metadata were automatically generated by the BWC technology. These data included the camera serial number, the officer to whom it was assigned, date/time stamps of activation and deactivation, length of recordings, and freeform data entered

by users that briefly described the nature or important details of the recording (e.g. departmental report number, accidental activation, relevant information about the recording). The camera metadata generated by the VIEVU system was made available in its entirety, and included 171,653 individual video files created over an 18-month period; beginning with the first day of active deployment, May 24, 2017, through November 23, 2018, which was the most recently available data at the time of request. Measures used for analyses included the description of the type of activation (e.g. incident recording, accidental activation, testing), the length of the recording, and whether the video file was attributed to a particular incident number. Additionally, those video files tagged with an incident number were linked to official CAD/RMS incident data to measure rates of compliance, and activations by incident type.

**Official complaint data.** Department wide official complaint data were gathered from PPD's Professional Standards Bureau (PSB) for all sworn officers from May 24, 2015 through November 23, 2018. These data included all complaints, regardless of the source of the complaint's initiation (e.g. citizen, officer's supervisor, complaint website). The data identified the officer, the officer's assignment at the time of complaint, the allegation against the officer (e.g., rude behavior, use of force), the disposition of the investigation (e.g. founded, unfounded, suspension), a narrative of the incident, and the incident number associated with the complaint. Included in the narrative was whether body-camera video was reviewed as part of the investigation. The complaint data were used to examine the change in the number of complaints pre-posttest by treatment and control groups.

The complaint data initially included 3,351 cases. We first removed 926 cases from the analysis file because no name or identifying information was known to PSB (e.g. some cases reported to PPD concerned non-PPD police officers or were about the department as a whole). Another 1,340 cases were removed from the analysis because they were related to personnel other than treatment and control officers, who were the focus of the present study. Finally, we removed 80 complaints about events that were unrelated to an officers' job performance (e.g., complaints about officer behavior off duty that would not be impacted by a BWC). In the end, the final dataset contained 1,005 complaints.

The measures created from these data were the number of complaints made against each officer 18-months prior to the deployment of BWCs and 18-months following BWC deployment. Given the low incidence of citizen complaints, we also estimated a complaint rate for the pre-deployment and post-deployment periods. We used a Poisson model to predict the total number of complaints against each officer in each time period, controlling for several officer-level covariates captured prior to BWC deployment, using the total number of calls-for-service the officer responded to in that time period as an exposure variable. The pre-deployment officer-level covariates included were: gender, race/ethnicity, educational attainment, precinct assignment, age, years of service, number of dispatched calls, response time, number of self-initiated calls, percentage of calls that were self-initiated, number of arrests, percentage calls that resulted in arrest, number of complaints, and number of use of force incidents.

**Use of force data.** Data on use of force were collected through the Crime Analysis and Research Unit. These data initially included 3,017 use of force incidents from November 24, 2015 to November 23, 2018. The PPD's Standard Operating Procedures (SOP) requires that

supervisors complete an official use of force report if the incident included: ECD (e.g., TASER), intermediate control techniques (e.g., hard empty hand techniques, flashlights, K9s, stunbags), carotid control technique, and deadly force. There were 1,700 use of force incidents resulting in a mandatory use of force report over the study period. The use of force data used in this evaluation do not include those events that involved verbal persuasion, negotiation or command, soft empty hand and restraining devices, and tripping/tackling, as SOP only requires that these incidents are recorded through a use of force report when an injury occurs or is alleged (n=1,100). Use of force reports that did not include the type of force used were excluded from the analysis (n=217). Over the study period, the PPD recorded 402 incidents of use of force involving the treatment and control groups that resulted in the mandatory creation of a use of force report.

The measures created from these data were the number of use of force incidents each officer engaged in prior to the deployment of BWCs (November 24, 2015 to May 23, 2017) and following BWC deployment (May 24, 2017 to November 23, 2018). As in complaints, a use of force rate was also created given the low incidence of these events. We again used a Poisson model to predict the total number of use of force incidents against each officer in each time period, controlling for officer-level covariates captured prior to BWC deployment and using the total number of calls-for-service the officer responded to in that time period as an exposure variable. The pre-deployment officer-level covariates included were: gender, race/ethnicity, educational attainment, precinct assignment, age, years of service, number of dispatched calls, response time, number of self-initiated calls, percentage of calls that were self-initiated, number of arrests, percentage calls that resulted in arrest, number of complaints, and number of use of force incidents. All of these covariates were collected from the pre-deployment period (November 24, 2015 to May 23, 2017) to ensure there was no relationship between treatment assignment and the outcomes.

### **Analytic strategy**

**Activation compliance.** We examine officer compliance with BWC activation policy for officers who were randomly selected and mandated to wear a BWC (n=34), officers who were randomly selected and agreed to volunteer to wear a BWC (n=47), and for officers who were non-randomly selected by their commanders and required to wear a BWC (n=8). We first assess whether officers in each of these groups differ significantly from each other in terms of their demographic characteristics using chi-square and ANOVAs. Using employee personnel data, we examine differences in officer gender, race/ethnicity, educational attainment, age, and years of service. We also examine differences in officer activity levels in the 18-months prior to BWC deployment (November 24, 2015 to May 24, 2017). We compare officers in each group on their percentage of calls that were self-initiated, percentage of calls that resulted in arrest, use of force rate, and complaint rate.

We then examine the types of videos created by the mandated, volunteer, and commander selected groups. Using the incident report numbers from the BWC activation metadata, we linked activations to the CAD data. This allowed us to determine if the activation was associated with an incident in CAD, if the activation did not have an incident number, or if the activation did not result in the creation of an incident report. We further use the activation metadata to

compare the duration of each type of video between the groups. We use ANOVAs to determine whether any differences between these groups of officers exist in activation time, by video type.

We also examine differences in the number of each video type created by officers in each group. We compare the number of total activations for officers in the mandated, volunteer, and commander pick groups. We also compare the total number of activations with and without an incident report number between officers in these groups. We again use ANOVA to determine whether there are any significant differences in the number of activations between officers in each group.

Given the importance of incident report numbers in establishing officer compliance with activation policies, we examine the percentage of videos that are missing incident numbers for each month in the study period. For each month in the study period, we calculated the percentage of videos missing incident numbers as the total number of videos missing incident numbers in each month divided by the total number of videos created in that month. This is important to examine, as those videos that are missing incident reports could result in underestimations of officer compliance with BWC activation policies if those videos should be linked to an incident.

We then use the activation data, as linked to the CAD data, to examine what types of incidents officers are recording. We descriptively examine which proportion of videos created by officers in each group fall into the following incident types: violent, property, disorder, subject/vehicle stops, traffic violations, and other (see Appendix C for call codes falling in each category).

Next, we examine compliance with activation policy for each of the incident types, again using the activation data linked to the CAD data. To do so, we take the total number of activations for each incident type divided by the total number of incidents for each type that involved an officer who was wearing a BWC at the time of the incident. This enables us to determine the proportion of each type of incident that should have resulted in a BWC activation that did indeed result in the creation of a video.

We further examine officer compliance with activation policy by each of these incident types, and compare compliance between mandated officers, volunteers, and commander picks. For each officer, we created the portion of calls that resulted in an activation by taking the total number of activations for each incident type for each officer divided by the total number of incidents for each type that officer responded to after being assigned a BWC. We use ANOVA to identify any statistically significant differences between officers in each of our groups in their compliance rates by incident type.

Finally, we examine the proportion of incidents that should have resulted in the creation of a BWC video that did indeed result in a BWC activation for each month of the study period. For each month, we took the total number of activations divided by the total number of calls involving an officer wearing a BWC to create a monthly rate of activation compliance. This is used to examine whether officers became more or less compliant with BWC activation policy over the course of the study period.

**Outcomes resulting from BWC assignment.** In this portion of the report, we examine whether officers in the resistor, BWC mandated, and BWC volunteer groups experienced BWC induced passivity or change in complaints received or use of force as a result of BWC assignment. We compare groups on the following outcomes in the 18-months following the deployment of BWCs in the PPD:

- the number of dispatched calls they responded to
- the number of self-initiated calls they engaged in
- the percentage of their calls that were self-initiated
- their response time
- the number of arrests they conducted
- the percentage of their calls that resulted in arrest
- the number of complaints they received
- their complaint rate
- the number of use of force incidents they engaged in
- their use of force rate

We first examine demographic characteristics between officers in the control, resistor, mandated, and volunteer groups. We specifically examine officer gender, race/ethnicity, educational attainment, precinct assignment, age, and years of service using data from personnel files. We use chi-square and t-tests to compare differences between officers in each of the treatment groups (resistor, mandated, volunteer) to officers in the control group. We also examine the magnitude of these differences using effect size.

We then examine mean percent change in each of our outcome variables by group, from the pre-deployment period (November 24, 2015 to May 23, 2017) to the post-deployment period (May 24, 2017 to November 23, 2018). Mean percent change was calculated for each group as:  $\frac{Posttest\ score - Pretest\ score}{Pretest\ score} \times 100$ . This enables a descriptive examination of change in officer activities over time. We used t-tests to compare within group change in each outcome from pre-deployment to post-deployment. We used regression to examine between-group differences. For each outcome, we estimated a regression model including a group dummy variable (using the control group as the reference category) and a control for the pre-deployment measure of that outcome.

Next, we use difference-in-difference (DID) estimators to examine change in each outcome over time, for officers in the resistant, mandated, and volunteer groups, relative to the control group. DID models are particularly informative for the current study because they capture within group change in each outcome from pre-deployment to post-deployment, relative to the change experienced by officers in the control group for the same outcome. For each outcome, we estimate a regression model to predict the post-deployment outcome using an independent variable for group assignment (using the control group as the reference category) and a control variable for the pre-deployment measure of that outcome.

To further assess differences in these outcomes, we present potential outcome means for the resistant, mandated, and volunteer groups. Potential outcome means were estimated using

regression models predicting the outcome, including a dummy variable for group assignment (using the control group as the reference category) and a number of officer-level covariates. The officer-level covariates included are: gender, race/ethnicity, educational attainment, precinct assignment, age, years of service, and pre-BWC deployment measures for each outcome variable examined (e.g., number of dispatched calls, response time, number of self-initiated calls, etc.). After using regression to estimate potential outcome means for each group, we use a bonferroni adjustment to examine between-group differences, again using the control group as the reference category.

Given some notable differences in demographic characteristics and pre-deployment activity levels between officers in some of our study groups (see Appendix B), we also examine propensity score weighted regression models. Propensity score weighting is used to reduce differences between officers in the resistant, mandated, volunteer, and control group on pre-BWC deployment covariates. This is accomplished through reweighting officers in each group to create more homogenous groups of officers so that the groups are comparable to each other. To create the propensity weights for each officer, we estimated a multinomial probit model predicting group membership using the following officer-level covariates: gender, race/ethnicity, educational attainment, precinct assignment, age, years of service, and pre-BWC deployment measures for each outcome variable examined. We used the results of the multinomial probit to predict the probability of each officer being part of the group they were ultimately assigned to. We then used these predicted probabilities to create propensity weights as  $\frac{1}{P}$ , where P is the probability that officer was assigned to their respective group. To ensure that our results are doubly robust, we then include the propensity weights and the same officer-level covariates used to predict the propensity weights (all of which were measured pre-BWC deployment) in a regression model predicting our post-deployment outcomes. The inclusion of pre-deployment covariates in both the model used to estimate our propensity weights and the regression model used to examine our outcomes results in unbiased estimates by eliminating potential relationships between pre-deployment differences between officers and post-deployment outcomes. Using the propensity weighted data, we replicate our above analyses. We first present weighted potential outcome means for each of our outcomes, as discussed above. We then replicate our DID models, weighting the regression using our propensity weights.

## Findings

### Activation Compliance

PPD's on-officer video camera policy was first established in April 2013. The operational guidelines note that prior to each shift, officers must ensure that the VIEVU device is sufficiently charged. The camera must be worn above the duty belt, in a manner that maximizes the functionality of the camera. The device must be worn anytime the user may become involved in enforcement activity.

The PPD's policy states that safety of the patrol officers and citizens is the first priority and always comes before any considerations relating to when to activate the camera. "Bearing this in mind, all officers and supervisors who engage with scenes or participate in an



enforcement contact must place their VIEVU camera in the on/record mode as soon as it is safe and practical to do so.” Enforcement contacts include, vehicle stops, pedestrian stops, consensual encounters that are investigative in nature, calls for service, on-view events requiring enforcement activities, suspect and witness statements and interviews, vehicle and foot pursuits, and emergency response to critical incidents.

The policy was updated on March 1, 2018 and activation requirements were changed to the following:

Users must activate the On/Record Mode upon receiving a call for service and/or prior to engaging in any investigative or enforcement contact, such as, but not limited to: Vehicle stops, Pedestrian stops, Consensual encounters that are investigative in nature, Radio calls for service, On-view events requiring enforcement activity, Official suspect and witness statements and interviews, [and] Vehicle and foot pursuits. (PPD, 2019, operations order 4.49, page 4).

Once the VIEVU camera’s on/record mode is activated, officers must continue to record the event or encounter until either the completion of the event or until they leave the scene. After the videos are uploaded, officers must tag the video file with the appropriate incident number, citation number, or department report number. Each week, supervisors are required to inspect at least one video for officers assigned to their squad who are assigned a BWC and record their findings in the user’s/employee’s supervisor notes. Each month, the precinct inspections lieutenant is required to randomly inspect at least six (6) body-worn camera videos and record their findings in the Monthly Inspections Report. The Department also has the ability to review video to ensure officer compliance with policy, to investigate citizen complaints, and for training purposes (PPD, 2019, operations order 4.49, page 4).

An analysis of camera meta-data was conducted to assess the activation characteristics of the video files produced, and the data associated with each file. As seen in Exhibit 10, we first examined the quality of the data by reviewing the amount of missing incident numbers contained in the BWC meta data. In these cases, a video cannot be linked to the incident. There were a total of 104,713 valid video files. More than 98% of video files were attributed to an incident number in the camera meta-data. We define “valid video files” as those attributed to an officer’s activity and/or possible interaction with the public, thus excluding test and accidental activations and various file creation errors. The proportion of missing incident numbers in the meta data remained fairly stable over the study period (Exhibit 11), with an average of 1.6% of valid videos missing an incident number each month.

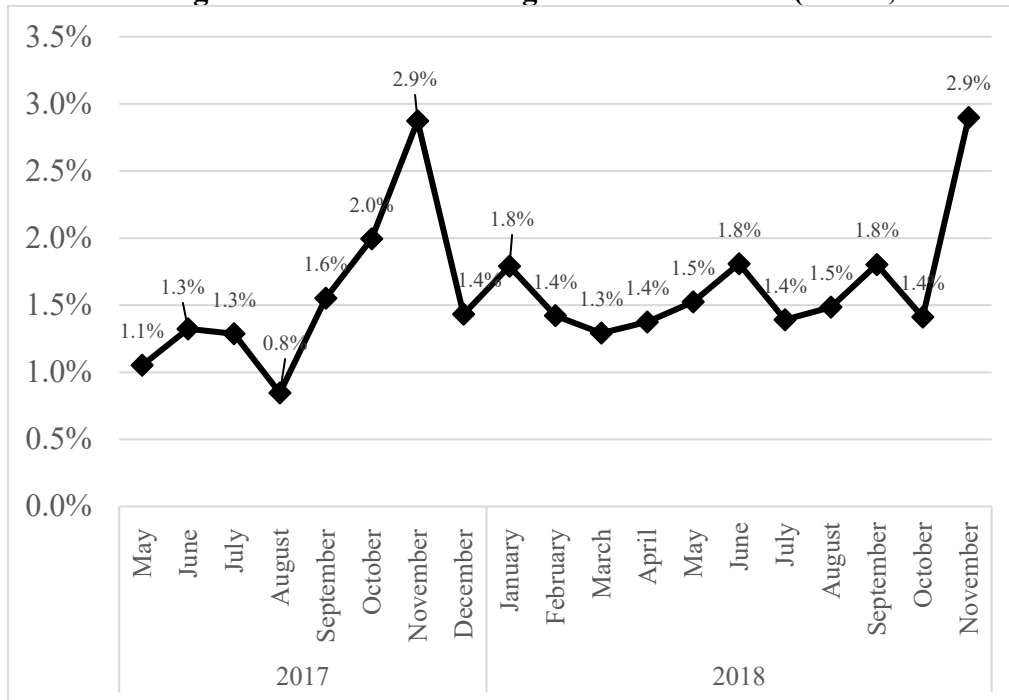
**Exhibit 10: BWC Activation types and time (minutes) (5/24/17 - 11/23/18)**

	Randomly selected & mandated to wear BWC		Randomly selected & volunteered to wear BWC		Commander pick to wear BWC		All BWC officers	
	%	Mean (SD) minutes	%	Mean (SD) minutes	%	Mean (SD) minutes	%	Mean (SD) minutes
Video attributed to an incident	98.50	16.40 (19.66)	98.10	15.73 (17.95)**	98.50	16.71 (18.66)	98.30	16.06 (18.64)
Video w/o incident number	1.40	11.53 (19.13)	1.80	8.27 (13.03)*	1.50	13.07 (20.31)	1.70	9.70 (15.95)
Video of incident that did not result in IR	0.00	3.63 (3.85)	0.10	5.73 (8.92)	0.10	13.34 (18.71)	0.00	6.70 (10.82)
Total # activations	36,297 activations		57,584 activations		10,832 activations		104,713 activations	

Note: Mean and standard deviation reported in minutes

\*p<0.05 \*\*p<0.01 using ANOVA to compare between group differences in activation time

**Exhibit 11: Percentage of valid videos missing incident numbers (n=104,671 activations)**



Next, we examined activations by the length of time the BWC was activated. Exhibit 10 (above) shows that on average BWC activations that were attributed to an incident report lasted about 16 minutes. Those without an incident number lasted about 10 minutes and those recordings that did not result in an incident report lasted about 7 minutes. Commander picks BWC activations that were attributed to an incident lasted for about 17 minutes, compared to about 16 minutes for mandated and volunteer officers. Length of activation for BWC activations without an incident number also significantly varied by group assignment. Activations without an incident number on average lasted by 13 minutes for commander picks, 11.5 minutes for mandated officers, and 8 minutes for volunteers. A similar trend was observed for activations that did not result in an incident report, the differences were not significant.

We then examined the mean number of BWC activations by group assignment (Exhibit 12). Officers assigned to wear a BWC activated it on average 1,177.55 times over the study period. We found that, on average, volunteers activated their BWC about 1,225 times over the study period, followed by 1,354 times by the commander picks, and about 1,067 times by the officers who were mandated to wear a BWC. Similar trends were observed for mean number of activations with and without a report number.

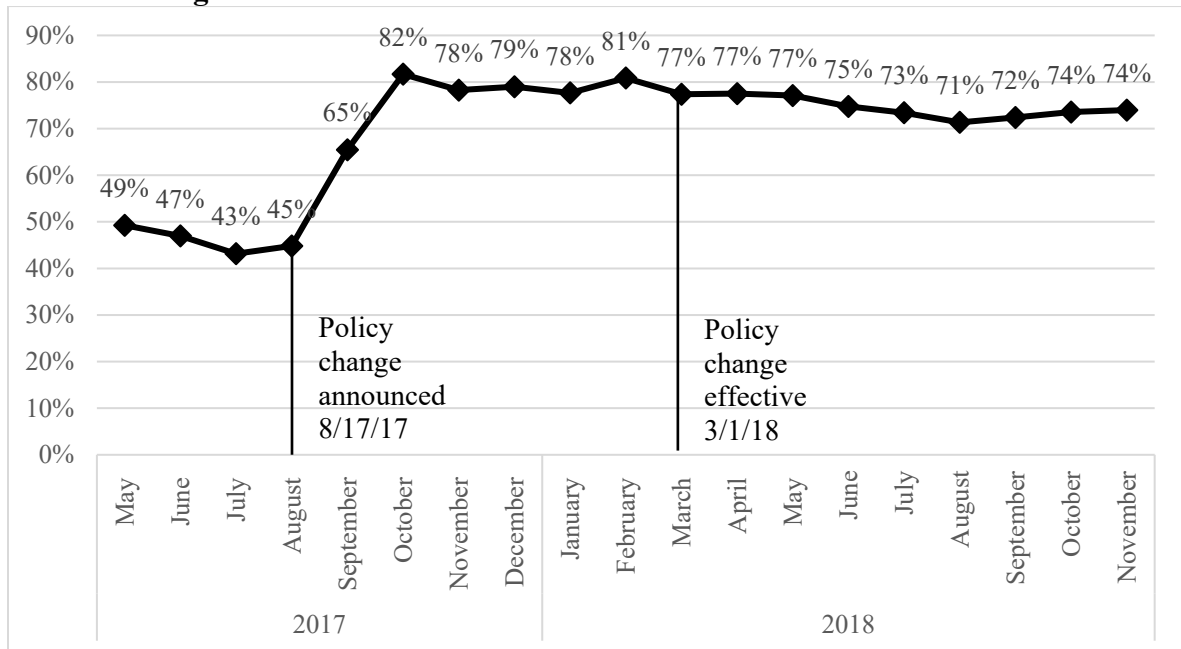
**Exhibit 12: BWC activations by officer (n=104,713; 5/24/17 to 11/23/18)**

	Randomly selected & mandated to wear BWC	Randomly selected & volunteered to wear BWC	Commander pick to wear BWC	All BWC officers
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
# total activations/officer	1067.56 (470.19)	1225.19 (568.02)	1354.00 (475.04)	1176.55 (527.47)
# activations w/ IR number/officer	1051.94 (462.76)	1201.91 (559.29)	1333.25 (473.01)	1156.43 (519.40)
# activations w/o IR number/officer	15.44 (15.05)	22.66 (29.94)	19.88 (12.19)	19.65 (24.02)

\*p<0.05 \*\*p<0.01 using ANOVA to compare between group differences in number of activations by type

We next examined general levels of compliance over time. As seen in Exhibit 13, we found that officers assigned a BWC activated it about 43 to 49% of the time until August 2017. In August 2017, the PPD announced its new BWC activation policy that required BWCs be activated upon receipt of a call for service. Compliance rates following the policy change announcement increased from 45% in August 2017 to 82% in October 2017. Compliance rates then stabilized and have held stable at 74% or so through November 2018. As shown in Exhibit 14, compliance rates do not vary substantially depending on officer group assignment.

**Exhibit 13: Percentage of incidents with video**



**Exhibit 14: Percentage of incidents with video by group**

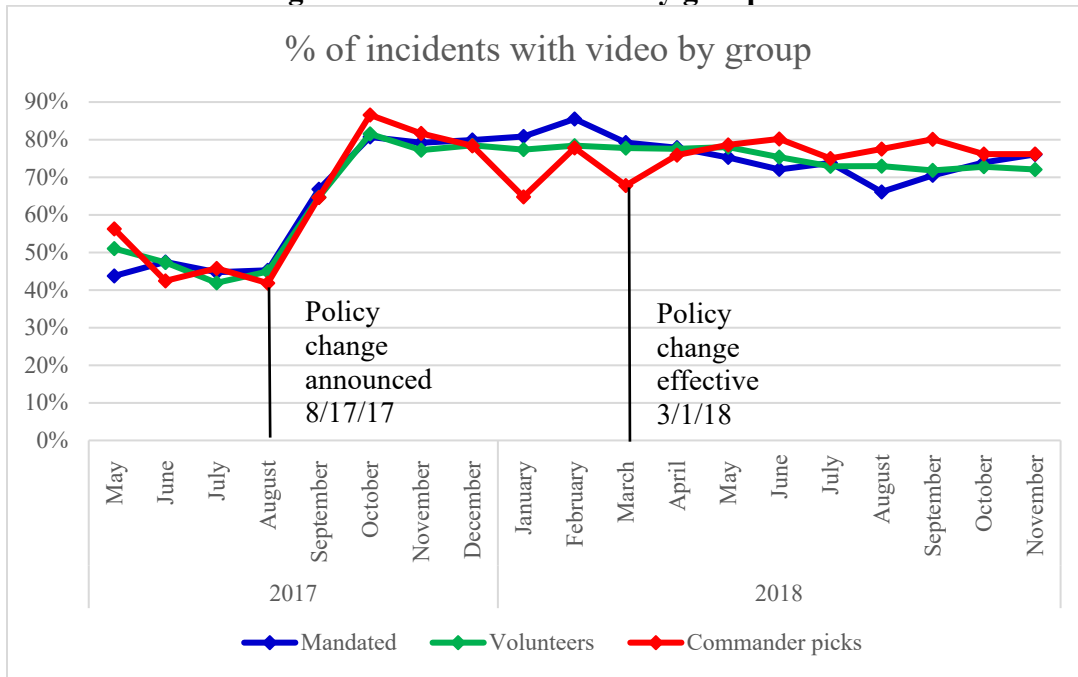
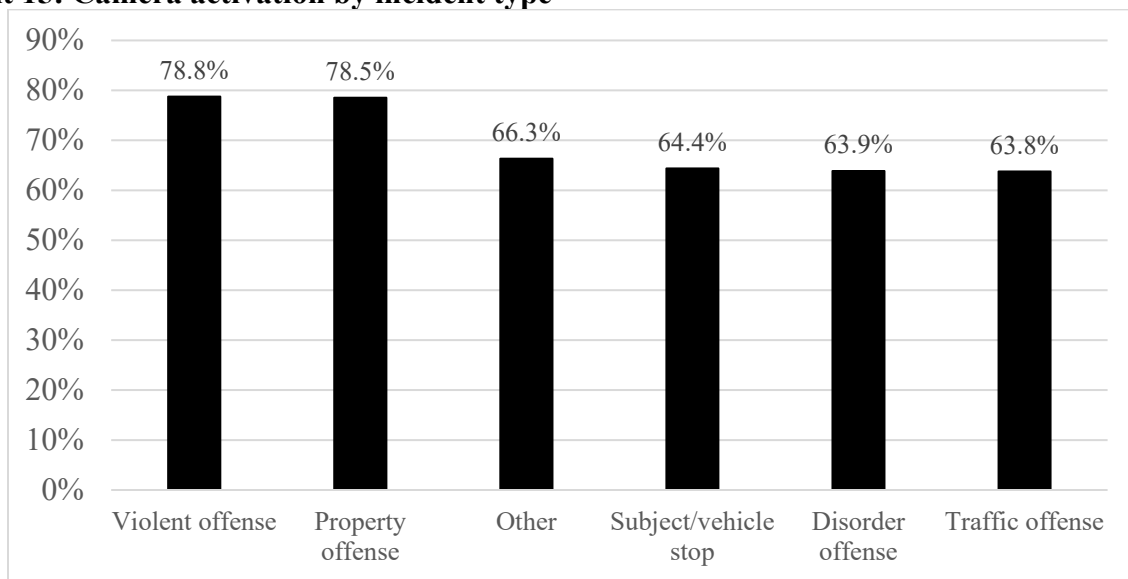


Exhibit 15 displays camera activation compliance by incident type using radio code entries from the incident data (see Appendix C for coding of call types). Compliance was most frequent when the incident was identified as a violent offense (78.8%), followed by property offense (78.5%), other offense (66.3%), subject/vehicle stop (64.4%), disorder offense (63.9%) and traffic offense (63.8%). We examined compliance rates by incident type and group assignment but there were no significant differences (see Exhibit 16).

**Exhibit 15: Camera activation by incident type**



**Exhibit 16: BWC activations by officer and incident type (5/24/17 to 11/23/18)**

	Randomly selected & mandated to wear BWC (n=34)	Randomly selected & volunteered to wear BWC (n=47)	Commander pick to wear BWC (n=8)	All BWC officers (n=89)
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Violent offense	0.77 (0.11)	0.75 (0.16)	0.79 (0.05)	0.77 (0.14)
Property offense	0.77 (0.12)	0.76 (0.16)	0.79 (0.07)	0.77 (0.14)
Disorder offense	0.62 (0.16)	0.60 (0.19)	0.65 (0.09)	0.61 (0.17)
Traffic offense	0.62 (0.18)	0.59 (0.19)	0.69 (0.11)	0.61 (0.18)
Subject/vehicle stop	0.58 (0.19)	0.59 (0.17)	0.65 (0.11)	0.59 (0.17)
Other	0.65 (0.13)	0.62 (0.16)	0.66 (0.08)	0.64 (0.15)
Total calls	0.70 (0.12)	0.68 (0.16)	0.71 (0.06)	0.69 (0.14)

\*p<0.05 \*\*p<0.01 using ANOVA to compare between group differences in activation by call type

### Outcomes Associated with BWCs

We begin by assessing demographic differences between officers in the resistor, mandated, and volunteer groups, relative to the control group, to determine whether our study groups are comparable. As shown in Exhibit 17, officers who resisted wearing a BWC statistically differed in terms of precinct assignment, compared to officers in the control group (5.4% vs. 29.3% from Mountain View,  $p<.05$ ,  $g=-0.29$ ). Though not statistically significant, resistors had a small effect size difference in age, relative to control officers ( $M=36.3$ ,  $SD=8.8$  vs.  $M=38.3$ ,  $SD=9.2$ ,  $g=0.22$ ). There were no statistically significant differences between officers mandated to wear a BWC and control officers. However, a small effect size difference shows that mandated officers were more likely to be white than control officers (79.4% vs. 69.4%,  $g=0.22$ ). BWC volunteers significantly differed in terms of precinct assignment, relative to control officers (6.4% vs. 29.3% from Mountain View,  $p<.05$ ,  $g=-0.04$ ). Volunteers also had a small effect size difference in race/ethnicity (78.7% vs. 69.4% white,  $g=0.21$ ), educational attainment (4.3% vs. 13.9% high school graduates,  $g=0.29$ ), and years of service ( $M=8.2$ ,  $SD=7.7$  vs.  $M=9.8$ ,  $SD=7.5$ ,  $g=0.21$ ), relative to control officers.

**Exhibit 17: Descriptive statistics and balance assessment**

	Control (n=263)		Resistor (n=92)			Mandated (n=34)			Volunteer (n=47)		
	n	%	n	%	Hedge's g	n	%	Hedge's g	n	%	Hedge's g
Sex					-0.19			0.02			0.05
Female	29	11.11	5	5.43		4	11.76		6	12.77	
Male	232	88.89	87	94.57		30	88.24		41	87.23	
Race/ethnicity					-0.04			0.22			0.21
White	181	69.35	62	67.39		27	79.41		37	78.72	
Nonwhite	80	30.65	30	32.61		7	20.59		10	21.28	
Highest education completed					0.02			-0.11			0.29
HS/GED	36	13.90	12	13.04		6	17.65		2	4.26	
>HS/GED	223	86.10	80	86.96		28	82.35		45	95.74	
Precinct			†		-0.29			0.06	†		-0.04
Black Mountain	39	14.83	8	8.70		9	26.47		4	8.51	
South Mountain	54	20.53	6	6.52		5	14.71		11	23.40	
Central City	3	1.14	23	25.00		0	0.00		5	10.64	
Desert Horizon	62	23.57	17	18.48		7	20.59		13	27.66	
Mountain View	77	29.28	5	5.43		5	14.71		3	6.38	
Cactus Park	28	10.65	33	35.87		8	23.53		11	23.40	
Age					0.22			0.00			0.14
Mean (SD)	38.27 (9.15)		36.30 (8.81)			38.24 (8.92)			37.00 (9.96)		
Years of Service					0.14			-0.14			0.21
Mean (SD)	9.80 (7.52)		8.73 (7.08)			10.85 (8.17)			8.21 (7.72)		

† p<0.05 between group differences using the control group as the reference category

Note: missing data not shown; mean (standard deviation); effect size reported in Hedge's g; officer nonwhite includes Hispanic, Black, Asian, and other race/ethnicity - categories were collapsed due to small n and insignificant differences between groups

## Unweighted results.

We started our analyses examining mean percent change over time. Exhibit 18 shows a significant increase in the number of self-initiated calls and the percentage of calls that were self-initiated for control officers (26.6%, 17.4%), resistors (31.7%, 16.5%), and volunteers (50.9%, 23.1%) from pre-deployment to post deployment ( $p<.05$ ). Resistors (-11.1%) and volunteers (-7.7%) also had a significant decrease in the percentage of calls they engaged in that resulted in arrest ( $p<.05$ ). For mandated officers, the only significant within-group difference was a decrease in the number of dispatched calls they responded to (-16.8%,  $p<.05$ ). Exhibit 18 also shows between-group differences. Though not statistically significant, the mandated officers had small effect size differences in the number of dispatched calls they responded to (-16.8% vs. +2.4%,  $g=-0.4$ ), the number of self-initiated calls they engaged in (-2.8% vs. +26.6%,  $g=-0.38$ ), the number of arrests they conducted (-11.8% vs. +3.9%,  $g=-0.3$ ), and the percentage of calls they responded to that resulted in arrest (+5.7% vs. -4.9%,  $g=-0.24$ ), relative to the control group. There were no statistically significant differences between volunteers and control officers, though there were some small effect size differences in the number of self-initiated calls officers engaged in (+50.9% vs. +26.6%,  $g=0.28$ ) and response time (-1.5% vs. -0.1%,  $g=-0.2$ ).

**Exhibit 18: Mean percent change over time officer activities**

	Control (n=263)	Resistor (n=92)	Effect size	Mandated (n=34)	Effect size	Volunteer (n=47)	Effect size
Mean % change in # dispatched calls	2.36	0.93	-0.02	-16.79*	-0.40	8.17	0.10
Mean % change in # officer initiated calls	26.56*	31.71*	0.07	-2.84	-0.38	50.85*	0.28
Mean % change in % of officer initiated calls	17.41*	16.46*	-0.02	10.63	-0.14	23.08*	0.15
Mean % change in response time (minutes)	-0.12	0.09	0.03	-0.16	0.00	-1.46	-0.20
Mean % change in # arrests	3.85	3.10	-0.01	-11.82	-0.30	4.90	0.03
Mean % change in % calls resulting in arrest	-4.92	-11.07*	-0.14	5.73	0.24	-7.69*	-0.18

\* $p<0.05$  for within group difference; † $p<0.05$  for between group difference using the control group as the reference category

Mean % change calculated as (group posttest mean-group pretest mean/group pretest mean)\*100

Exhibit 19 shows mean percent change over time in complaints and use of force. Control officers had a significant increase in their use of force rate per call post-BWC deployment (18.2%,  $p<.05$ ). Mandated officers had a significant decrease in their complaint rate per call (-46.2%,  $p<.05$ ). Turning to between-group differences, both resistors (-35.5%,  $g=-0.25$ ) and mandated officers (-46.2%,  $g=-0.39$ ) had a significantly greater reduction in their complaint rate, relative to control officers (-0.4%,  $p<.05$ ). Though not statistically significant, mandated officers also had a small effect size difference in the number of complaints they received, relative to control officers (-48.0% vs. +5.8%,  $g=-0.33$ ). There were no statistically significant or



substantively meaningful differences in complaints and use of force between volunteers and control officers.

**Exhibit 19: Mean percent change over time – complaints and use of force**

	Control (n=263)	Resistor (n=92)	Effect size	Mandated (n=34)	Effect size	Volunteer (n=47)	Effect size
Mean % change in mean # complaints	5.76	-26.79	-0.17	-48.00	-0.33	19.35	0.08
Mean % change in complaint rate per call	-0.37	-35.49†	-0.25	-46.15*†	-0.39	-2.20	-0.02
Mean % change in mean # use of force	10.53	22.22	0.08	20.00	0.02	40.00	0.10
Mean % change in use of force rate per call	18.18*	26.24	0.08	37.50	0.09	-6.36	-0.16

\*p<0.05 for within group difference; †<0.05 for between group difference using the control group as the reference category

Mean % change calculated as (group posttest mean-group pretest mean/group pretest mean)\*100

Next, we examine DID estimates for officer activity measures. As shown in Exhibit 20, there were no statistically significant differences between resisters, mandated, or volunteer officers compared to control officers. Mandated officers did have a small effect size difference in the number of self-initiated calls they engaged in, compared to control officers (b=-50.2, g=-0.27). Volunteers had a small effect size difference in the number of dispatched calls they responded to (b=163.9, g=0.24) and the number of self-initiated calls they engaged in (b=58.4, g=0.3), relative to control officers.

**Exhibit 20: Unweighted difference-in-differences coefficients – officer activities**

	Resistor		Mandated		Volunteer	
	Coef.	Effect size	Coef.	Effect size	Coef.	Effect size
# dispatched calls	48.02 (82.45)	0.07	-43.07 (127.38)	-0.06	163.90 (107.82)	0.24
# officer-initiated calls	19.58 (24.13)	0.10	-50.16 (33.88)	-0.27	58.41 (31.22)	0.30
% officer-initiated calls	-0.00 (0.01)	-0.02	-0.01 (0.01)	-0.13	0.01 (0.01)	0.15
Response time (logged minutes)	0.00 (0.02)	0.01	-0.00 (0.03)	0.00	-0.02 (0.02)	-0.18
# arrests	-4.10 (14.47)	-0.03	-17.39 (21.57)	-0.15	19.61 (18.95)	0.16
% calls resulting in arrest	-0.01 (0.01)	-0.18	0.01 (0.01)	0.19	-0.00 (0.01)	-0.10

\*\* p<0.01, \* p<0.05

Note: Standard errors in parentheses; all difference-in-difference estimations included a control for pretest score, pretest coefficient omitted from tables to save space

Turning to our DID estimates for use of force and complaints (Exhibit 21), resisters ( $b=6.07E-05$ ,  $g=-0.53$ ) and mandated officers ( $b=-8.04E-05$ ,  $g=-0.74$ ) had a significantly lower complaint rate, relative to control officers ( $p<.01$ ). Interestingly, BWC volunteers had a significantly higher complaint rate compared to the control group ( $b=8.45E-05$ ,  $p<.01$ ,  $g=0.69$ ). Resisters also significantly decreased their use of force rate, relative to control officers ( $b=5.69E-05$ ,  $p<.01$ ,  $g=0.56$ ). Though the difference was not statistically significant, resisters had a small effect size difference in the total number of complaints they received compared to control officers ( $b=0.16$ ,  $g=0.20$ ). Mandated officers had a small effect size difference in the number of complaints they received relative to the control group ( $b=-0.18$ ,  $g=-0.23$ ), though this difference was not significant. BWC volunteers had a small effect size difference in the number of complaints compared to the control group ( $b=0.22$ ,  $g=0.25$ ), though this difference was not statistically significant either.

**Exhibit 21: Unweighted difference-in-differences coefficients – complaints/use of force**

	Resistor		Mandated		Volunteer	
	Coef.	Effect size	Coef.	Effect size	Coef.	Effect size
# complaints	-0.12 (0.10)	-0.14	-0.18 (0.15)	-0.23	0.22 (0.14)	0.25
Complaint rate/call	6.07E-05** (0.00)	-0.53	-8.04E-05** (0.00)	-0.74	8.45E-05** (0.00)	0.69
# use of force incidents	0.16 (0.09)	0.20	-0.03 (0.12)	-0.04	0.06 (0.11)	0.08
Use of force rate/call	5.69E-05** (0.00)	0.56	-1.51E-05 (0.00)	-0.16	-1.06E-05 (0.00)	-0.10

\*\*  $p<0.01$ , \*  $p<0.05$

Note: Standard errors in parentheses; all difference-in-difference estimations included a control for pretest score, pretest coefficient omitted from tables to save space

Next, we use regression to examine potential outcome means, as shown in Exhibit 22. Consistent with the DID results, BWC resisters had a significantly lower complaint rate ( $1.57E-04$  vs.  $2.81E-04$ ,  $p<.01$ ,  $g=-0.2$ ) but a significantly higher use of force rate ( $2.51E-04$  vs.  $2.12E-04$ ,  $p<.01$ ,  $g=0.07$ ), compared to control officers. Though the difference was not significant, resisters had a small effect size difference in the number of complaints they received, relative to control officers ( $0.34$  vs.  $0.61$ ,  $g=-0.3$ ). Officers mandated to wear a BWC had a significantly lower complaint rate than control officers ( $1.94E-04$  vs.  $2.81E-04$ ,  $p<.01$ ,  $g=-0.0$ ). Mandated officers also had small effect size differences in the number of self-initiated calls they engaged in ( $293.7$  vs.  $326.6$ ,  $g=-0.21$ ) and the number of complaints they received ( $0.38$  vs.  $0.61$ ,  $g=-0.27$ ) compared to control officers, though these differences were not statistically significant. BWC volunteers had a significantly higher complaint rate than control officers ( $3.42E-04$  vs.  $2.81E-04$ ,  $p<.01$ ,  $g=-0.14$ ).

**Exhibit 22: Outcome means using unweighted regression**

	Control	Resistor		Mandated		Volunteer	
	POM	POM	Effect Size	POM	Effect Size	POM	Effect Size
# dispatched calls	1754.29 (43.32)	1731.25 (68.45)	-0.03	1742.96 (97.42)	-0.02	1803.25 (67.52)	0.06
# officer-initiated calls	326.59 (11.57)	309.24 (22.21)	-0.10	293.72 (21.28)	-0.21	344.27 (25.88)	0.12
% officer-initiated calls	0.15 (0.00)	0.15 (0.01)	-0.11	0.14 (0.01)	-0.14	0.16 (0.01)	0.06
Response time (minutes)	2.04 (0.01)	2.07 (0.01)	0.14	2.03 (0.03)	-0.08	2.03 (0.02)	-0.07
# arrests	225.28 (7.39)	226.74 (11.60)	0.01	236.86 (16.10)	0.09	235.32 (13.13)	0.08
% calls resulting in arrest	0.11 (0.00)	0.11 (0.00)	-0.01	0.12 (0.01)	0.17	0.11 (0.01)	-0.02
# complaints	0.61 (0.05)	0.34 (0.08)	-0.30	0.38 (0.10)	-0.27	0.72 (0.15)	0.13
Complaint rate/call	2.81E-04 (0.00)	1.57E-04** (0.00)	-0.20	1.94E-04** (0.00)	0.00	3.42E-04** (0.00)	0.14
# use of force incidents	0.43 (0.05)	0.50 (0.09)	0.09	0.44 (0.13)	0.03	0.43 (0.13)	0.00
Use of force rate/call	2.12E-04 (0.00)	2.51E-04** (0.00)	0.07	2.16E-04 (0.00)	0.02	2.07E-04 (0.00)	-0.01

\*\* p<0.01, \* p<0.05 between group differences using the control group as the reference category

Note: Robust standard errors in parentheses; all POM estimations included covariates for officer sex, race/ethnicity, educational attainment, age, years of service, substation, #/% of self-initiated calls predeployment, response time predeployment, #/% of calls resulting in arrest predeployment, #/% of calls resulting in a complaint predeployment, #/% of arrests resulting in use of force predeployment; coefficients for covariates omitted from table to save space.



## Weighted results

In order to correct for covariate imbalance between study groups, we re-estimated the potential outcome means using propensity weighted regression adjustment (Exhibit 23). The differences between the weighted and unweighted results are fairly limited. Resistors again have significantly lower complaint rate ( $1.69E-04$  vs.  $2.78E-04$ ,  $p<.01$ ,  $g=-0.21$ ), but a significantly higher use of force rate ( $2.30E-04$  vs.  $2.04E-04$ ,  $p<.05$ ,  $g=0.05$ ), compared to the control group. Resistors also have a small effect size difference in their number of complaints compared to the control group (0.3 vs. 0.54,  $g=-0.24$ ), though the difference is non-significant. Mandated officers had a significantly lower complaint rate than control officers ( $1.85E-04$  vs.  $2.78E-04$ ,  $p<.01$ ,  $g=-0.17$ ). Though not statistically significant, mandated officers also had a small effect size difference in the number of the complaints they received, relative to control officers (0.34 vs. 0.54,  $g=-0.21$ ). BWC volunteers had a significantly higher complaint rate compared to control officers ( $3.48E-04$  vs.  $2.78E-04$ ,  $p<.01$ ,  $g=0.13$ ). Volunteers also had small effect size differences in the number of self-initiated calls (354.4 vs. 310.1,  $g=0.29$ ), percent of calls that were self-initiated (0.17 vs. 0.15,  $g=0.26$ ), and number of complaints (0.81 vs. 0.54,  $g=0.29$ ) contrasted to the control group, but these differences were not significant.

**Exhibit 23: Potential outcome means using inverse-probability weighted regression**

	Control		Resistor		Mandated		Volunteer	
	POM		POM	Effect Size	POM	Effect Size	POM	Effect Size
# dispatched calls	1651.97 (50.13)		1694.92 (63.48)	0.05	1725.54 (82.85)	0.10	1713.61 (74.33)	0.08
# officer-initiated calls	310.09 (14.10)		322.20 (24.59)	0.08	302.97 (24.40)	-0.05	354.42 (27.91)	0.29
% officer-initiated calls	0.15 (0.00)		0.15 (0.01)	-0.10	0.15 (0.01)	-0.08	0.17 (0.01)	0.26
Response time (minutes)	2.04 (0.01)		2.06 (0.02)	0.08	2.05 (0.03)	0.03	2.05 (0.01)	0.03
# arrests	217.78 (7.36)		233.72 (13.84)	0.12	239.14 (16.96)	0.17	225.10 (14.52)	0.06
% calls resulting in arrest	0.11 (0.00)		0.11 (0.00)	0.08	0.12 (0.01)	0.18	0.11 (0.01)	-0.05
# complaints	0.54 (0.06)		0.30 (0.08)	-0.24	0.34 (0.09)	-0.21	0.81 (0.20)	0.29
Complaint rate/call	2.78E-04 (0.00)		1.69E-04** (0.00)	-0.21	1.85E-04** (0.00)	-0.17	3.48E-04** (0.00)	0.13
# use of force incidents	0.40 (0.05)		0.37 (0.07)	-0.04	0.39 (0.13)	-0.01	0.51 (0.14)	0.16
Use of force rate/call	2.04E-04 (0.00)		2.30E-04* (0.00)	0.05	2.03E-04 (0.00)	0.00	2.19E-04 (0.00)	0.04

\*\* p<0.01, \* p<0.05 between group differences using the control group as the reference category based on Bonferroni-adjusted p-values; † indicates weighted difference > 0.25 in balance statistics

Note: Robust standard errors in parentheses; all POM estimations included covariates for officer sex, race/ethnicity, educational attainment, age, years of service, substation, #/% of self-initiated calls predeployment, response time predeployment, #/% of calls resulting in arrest predeployment, #/% of calls resulting in a complaint predeployment, #/% of arrests resulting in use of force predeployment; coefficients for covariates omitted from table to save space

We also re-estimated our DID models predicting officer activity levels, including our propensity score weights. As shown in Exhibit 24, there were no significant differences between resisters, mandated, and volunteer officers and the control group in terms of officer activity levels. Resisters did have small effect size differences in their number of self-initiated calls (b=43.7, g=0.21) and number of arrests (b=32.9, g=0.30), relative to control officers. Mandated officers had small effect size differences in the number of dispatched calls they responded to (b=136.1, g=0.2) and the percentage of their calls resulting in arrest (b=0.02, g=0.34), contrasted to control officers. Relative to the control group, volunteers had small effect size differences in the number of dispatched calls they responded to (b=141.8, g=0.23) and the number of self-initiated calls they engaged in (b=76.6, g=0.36).

**Exhibit 24: Propensity score weighted difference-in-differences coefficients**

	Resistor		Mandated		Volunteer	
	Coef.	Effect size	Coef.	Effect size	Coef.	Effect size
# dispatched calls	128.68 (132.07)	0.19	136.09 (176.94)	0.20	141.75 (131.08)	0.23
# officer-initiated calls	43.73 (40.23)	0.21	-9.01 (36.39)	-0.05	76.59 (51.59)	0.36
% officer-initiated calls	-0.01 (0.01)	-0.11	-0.01 (0.01)	-0.10	0.01 (0.02)	0.18
Response time (minutes)	0.00 (0.02)	0.03	0.00 (0.04)	0.03	-0.01 (0.02)	-0.11
# arrests	32.85 (17.66)	0.30	14.63 (23.27)	0.13	22.30 (26.49)	0.19
% calls resulting in arrest	0.00 (0.01)	0.09	0.02 (0.01)	0.34	-0.00 (0.01)	-0.05

\*\* p<0.01, \* p<0.05

Note: Robust standard errors in parentheses; all difference-in-difference estimations included a control for pretest score, pretest coefficient omitted from tables to save space

Finally, we included propensity score weights in our DID estimates of use of force and complaints. As shown in Exhibit 25, these results are largely consistent with our prior findings. Resisters (b=-1.01E-04, p<.01, g=-1.06) and mandated officers (b=-9.21E-05, p<.01, g=-0.97) had significantly lower complaint rates than control officers. BWC volunteers had significantly higher complaint rates than control officers (b=-9.06E-05, p<.01, g=0.71). Though not significant, resisters also had small effect size differences in the number of complaints (b=-0.15, g=-0.2) and their use of force rate (b=2.61E-05, g=0.26), compared to control officers. Mandated officers had a small effect size difference in the number of complaints they received (b=-0.16, g=-0.23), though the difference was not significant, relative to control officers. Finally, contrasted to control officers, volunteers had small effect size differences in the number of complaints they received (b=0.32, g=0.35), the number of use of force incidents they engaged in (b=0.16, g=0.22), and their use of force rate (b=2.32E-05, g=0.21), though these differences were not statistically significant.

**Exhibit 25: Propensity score weighted difference-in-differences coefficients**

	Resistor		Mandated		Volunteer	
	Coef.	Effect size	Coef.	Effect size	Coef.	Effect size
# complaints	-0.15 (0.12)	-0.20	-0.16 (0.12)	-0.23	0.32 (0.22)	0.35
Complaint rate/call	-1.01E-04** (0.00)	-1.06	-9.21E-05** (0.00)	-0.97	-9.06E-05** (0.00)	0.71
# use of force incidents	-0.04 (0.10)	-0.05	-0.02 (0.13)	-0.02	0.16 (0.15)	0.22
Use of force rate/call	2.61E-05 (0.00)	0.26	-6.82E-06 (0.00)	-0.07	2.32E-05 (0.00)	0.21

\*\* p<0.01, \* p<0.05

Note: Standard errors in parentheses; all difference-in-difference estimations included a control for pretest score, pretest coefficient omitted from tables to save space

**Post-Hoc Analysis**

Given our consistent findings that officers in the resistor, mandated, and volunteer groups differed from control officers in terms of complaints, we conducted some post-hoc analyses to try to identify what was driving these differences. To do so, we examined differences in the types of allegations that were made in complaints against officers in each group. We specifically examined complaints alleging that the officer was rude to the citizen, that the officer abused their authority, and that the officer used force. Allegations that the officer was rude include complaints that officers were not respectful toward citizens, used inappropriate language, and other similar types of allegations. Allegations that officers abused their authority were related to officers using their position as a police officer for personal gain. These included allegations that officers tried to convert police contacts into personal relationships, tried to use their position as officer to get discounts, etc. Allegations that the officer used force included complaints that the officer was too rough when conducting an arrest or otherwise interacting with citizens. We replicated our above tables using the following officer-level outcomes:

- A total complaint rate per 1,000 calls (calculated as total n complaints/total n calls x 1,000)
- A complaint rate regarding rude behavior per 1,000 calls (calculated as n complaints about rude behavior/total n calls x 1,000)
- A complaint rate regarding abuse of authority per 1,000 calls (calculated as n complaints officer abused their authority/total n calls x 1,000)
- A complaint rate regarding use of force per 1,000 calls (calculated as n complaints officer used force/total n calls x 1,000)

We examine unweighted DID estimates in Exhibit 26. Volunteers had significantly higher use of force complaint rates, compared to the control group (b=0.07, p<.01, g=0.54). There were no



statistically significant or meaningful effect size differences in allegations against resisters or mandated officers, relative to the control group.

**Exhibit 26: Post-hoc complaints analysis - Unweighted DID in complaints/1,000 contacts**

	Resistor		Mandated		Volunteer	
	Coef.	Effect size	Coef.	Effect size	Coef.	Effect size
Complaint rate/1,000 calls	-0.25 (0.32)	-0.09	-0.31 (0.53)	-0.11	-0.09 (0.45)	-0.03
Rude behavior complaint rate	-0.04 (0.03)	-0.17	0.01 (0.04)	0.05	0.00 (0.04)	0.01
Abuse of authority complaint rate	-0.17 (0.32)	-0.06	-0.18 (0.53)	-0.06	-0.2 (0.45)	-0.07
Use of force complaint rate	0.01 (0.02)	0.08	-0.01 (0.02)	-0.07	0.07** (0.02)	0.54

\*\* p<0.01, \* p<0.05

Note: Standard errors in parentheses; all difference-in-difference estimations included a control for pretest score, pretest coefficient omitted from tables to save space

As shown in Exhibit 27, the unweighted potential outcome means indicate that there were no significant differences in the types of complaint allegations between groups, though there were several effect size differences. Resistant officers had a medium effect size difference in their total complaint rate (0.18 vs. 0.48,  $g=-0.6$ ), a small effect size difference in their rude behavior complaint rate (0.03 vs. 0.08,  $g=-0.25$ ), and a large effect size difference their abuse of authority complaint rate (0.04 vs. 0.22,  $g=-1.04$ ), compared to control officers. Mandated officers also had a small effect size difference in their total complaint rate (0.27 vs. 0.48,  $g=-0.43$ ) and a medium difference in their abuse of authority complaint rate (0.1 vs. 0.22,  $g=-0.73$ ), relative to control officers. These findings indicate that mandated officers were less likely to have complaints filed against them overall, and regarding abuse of authority specifically, as compared to control officers. Finally, compared to control officers, volunteers had a small effect size difference in their total complaint rate (0.28 vs. 0.48,  $g=-0.44$ ), a large difference in their abuse of authority complaint rate (-0.09 vs. 0.22,  $g=-1.91$ ), and a small effect size difference in their use of force complaint rate (0.09 vs. 0.03,  $g=0.45$ ). This indicates that BWC volunteers were also less likely than control officers to have complaints filed against them overall, and alleging abuse of authority specifically. However, volunteers were more likely to have complaints alleging use of force filed against them than control officers.

**Exhibit 27: Post-hoc complaint analysis - Potential outcome means using unweighted regression**

	Control	Resistor		Mandated		Volunteer	
	POM	POM	Effect Size	POM	Effect Size	POM	Effect Size
Complaint rate/1,000 calls	0.48 (0.20)	0.18 (0.07)	-0.60	0.27 (0.13)	-0.43	0.28 (0.14)	-0.44
Rude behavior complaint rate	0.08 (0.15)	0.03 (0.16)	-0.25	0.07 (0.03)	-0.02	0.07 (0.03)	-0.04
Abuse of authority complaint rate	0.22 (0.20)	0.04 (0.05)	-1.04	0.10 (0.12)	-0.73	-0.09 (0.11)	-1.91
Use of force complaint rate	0.03 (0.01)	0.02 (0.02)	-0.06	0.03 (0.17)	-0.02	0.09 (0.03)	0.45

\*\* p<0.01, \* p<0.05 between group differences using the control group as the reference category

Note: Robust standard errors in parentheses; all POM estimations included covariates for officer sex, race/ethnicity, educational attainment, age, years of service, substation, #/% of self-initiated calls predeployment, response time predeployment, #/% of calls resulting in arrest predeployment, #/% of calls resulting in a complaint predeployment, #/% of arrests resulting in use of force predeployment; coefficients for covariates omitted from table to save space

We also replicated the above tables including propensity weights. Exhibit 28 shows the propensity weighted potential outcome means. These findings indicate that resisters had a significantly lower rude behavior complaint rate (0.03 vs. 0.08,  $p<.05$ ,  $g=-0.35$ ), relative to control officers. Resisters also had a medium effect size differences in their overall complaint rate (0.14 vs. 0.44,  $g=-0.66$ ) and a large effect size difference in their abuse of authority complaint rate (0.01 vs. 0.20,  $g=-0.93$ ), relative to control officers, though these differences were not significant. Though there were no statistically significant differences between mandated and control officers, there was a medium effect size difference in their total complaint rate (0.16 vs. 0.44,  $g=-0.57$ ) and a large effect size difference in their abuse of authority complaint rate (0.00 vs. 0.20,  $g=-1.0$ ). Though not statistically significant, volunteers had a large effect size difference in their abuse of authority complaint rate (-0.01 vs. 0.20,  $g=-1.23$ ) and a medium effect size difference in their use of force complaint rate (0.11 vs. 0.02,  $g=0.62$ ), compared to control officers. These findings are consistent with the unweighted data, and indicate that volunteers are more likely than control officers to have allegations that they used force filed against them.

**Exhibit 28: Post-hoc complaint analysis - Potential outcome means using weighted regression**

	Control	Resistor		Mandated		Volunteer	
	POM	POM	Effect Size	POM	Effect Size	POM	Effect Size
Complaint rate/1,000 calls	0.44 (0.19)	0.14 (0.05)	-0.66	0.16 (0.07)	-0.57	0.41 (0.11)	-0.06
Rude behavior complaint rate	0.08 (0.02)	0.03* (0.01)	-0.35	0.07 (0.03)	-0.05	0.09 (0.04)	0.00
Abuse of authority complaint rate	0.20 (0.19)	0.01 (0.03)	-0.93	0.00 (0.05)	-1.00	-0.01 (0.03)	-1.23
Use of force complaint rate	0.02 (0.01)	0.01 (0.01)	-0.04	0.02 (0.02)	0.04	0.11 (0.04)	0.62

\*\* p<0.01, \* p<0.05 between group differences using the control group as the reference category

Note: Robust standard errors in parentheses; all POM estimations included covariates for officer sex, race/ethnicity, educational attainment, age, years of service, substation, #/% of self-initiated calls predeployment, response time predeployment, #/% of calls resulting in arrest predeployment, #/% of calls resulting in a complaint predeployment, #/% of arrests resulting in use of force predeployment; coefficients for covariates omitted from table to save space

Last, Exhibit 29 shows propensity weighted DID estimators for complaint rates. These results again show that volunteers had a significantly higher use of force complaint rate than control officers (b=0.07, p<.01, g=0.51). Though not statistically significant, resistors did have a small effect size difference in their rude behavior complaint rate (b=-0.04, g=-0.29), relative to control officers. There were no statistically significant or meaningful effect size differences between mandated and control officers.

**Exhibit 29: Post-hoc complaints analysis - Weighted DID in complaints/1,000 contacts**

	Resistor		Mandated		Volunteer	
	Coef.	Effect size	Coef.	Effect size	Coef.	Effect size
Complaint rate/1,000 calls	-0.25 (0.32)	-0.10	-0.31 (0.53)	-0.12	-0.09 (0.45)	0.02
Rude behavior complaint rate	-0.04 (0.03)	-0.29	0.01 (0.04)	0.03	0.00 (0.04)	0.03
Abuse of authority complaint rate	-0.17 (0.32)	-0.08	-0.18 (0.53)	-0.07	-0.2 (0.45)	-0.08
Use of force complaint rate	0.01 (0.02)	0.01	-0.01 (0.02)	-0.03	0.07** (0.02)	0.51

\*\* p<0.01, \* p<0.05

Note: Standard errors in parentheses; all difference-in-difference estimations included a control for pretest score, pretest coefficient omitted from tables to save space

In short, these post-hoc analyses suggest that differences in complaints between resisters, mandated, and volunteer officers identified in Exhibit 25 could be driven by some differences in the types of allegations made against officers in each group. The difference for resisters could be related to a reduction in the rude behavior complaint rate for resistant officers. These officers, more than control officers, could be increasingly aware of the deployment of BWCs to officers throughout the department. The reduction in complaints for mandated officers, relative to control officers, does not appear to be linked to any particular type of allegation made against these officers. Finally, the significant increase in complaints against BWC volunteers appears to be driven by an increase in allegations that these officers used force. Interestingly, there were no significant differences between volunteer and control officers in the official use of force measures (Exhibit 25). However, there were small effect size differences indicating that volunteers increased their number of use of force incidents ( $g=0.22$ ) and rate of use of force incidents ( $g=0.21$ ), relative to control officers.

## Conclusions

We began by examining officer compliance with BWC activation policies. We found that over 98% of the videos created by officers wearing BWCs during the study period could be successfully linked to an incident report number in the CAD data – we refer to these as valid videos. This suggests that officers are generally providing accurate report numbers that can be used to assess their compliance with BWC policies. In terms of the duration of the BWC videos, activations that could be linked to an incident averaged about 16 minutes in length. Officers who were mandated to wear a BWC and those who were non-randomly assigned to wear a BWC by their commander recorded significantly longer videos than officers who volunteered to wear a BWC ( $p<.01$ ; Exhibit 10).

In terms of the number of BWC activations, commander picks had an average of 1,354 valid activations over the study period, followed by 1,225 valid activations for volunteers, and 1,067 activations officers who were mandated to wear a BWC (Exhibit 12). There were no significant differences in the number of activations between groups. In terms of compliance rates, we found that officers assigned a BWC activated it about 43 to 49% of the time until PPD announced a change to its activation policy in August 2017. Compliance rates increased to 82% in October 2017 and remained stable at 74% or so through November 2018 (Exhibit 13).

Officers had the highest activation compliance rates when they responded to violent offenses (78.8%), followed by property offenses (78.5%), other offenses (66.3%), subject/vehicle stops (64.4%), disorder offenses (63.9%) and finally traffic offenses (63.8%). We did not identify any significant differences in activation compliance for different incident types between officers mandated to wear a BWC, those who volunteered to wear a BWC, and those who were assigned a BWC by their commander (see Exhibit 16). Taken as a whole, these findings suggest the way an officer receives a camera is not related to their compliance with BWC activation policies.

With respect to the impact of BWC on officer induced passivity, the findings generally show that the deployment of BWCs had limited impact on activity measures for officers who resisted wearing a BWC. These officers did not change the number of calls they responded to, self-initiated contacts, response time, or arrest behaviors after the deployment of BWCs. Officers who were randomly selected and mandated to wear a BWC also had limited changes in these activity measures over time. Though there is some indication that mandated officers engaged in fewer self-initiated contacts after BWC deployment (a non-significant, small effect size change  $g=-0.27$ ). Officers who volunteered to wear a BWC, on the other hand, increased the number of dispatched calls they responded to ( $g=0.24$ , not-significant) and the number of self-initiated contacts they engaged in ( $g=0.30$ , not significant). This suggests that being forced to wear a BWC could slightly reduce officer willingness to proactively contact citizens. Alternatively, officers who volunteer to wear a BWC could engage in slightly more self-initiated contacts because they feel that the presence of a BWC provides additional evidence to justify contacting citizens.

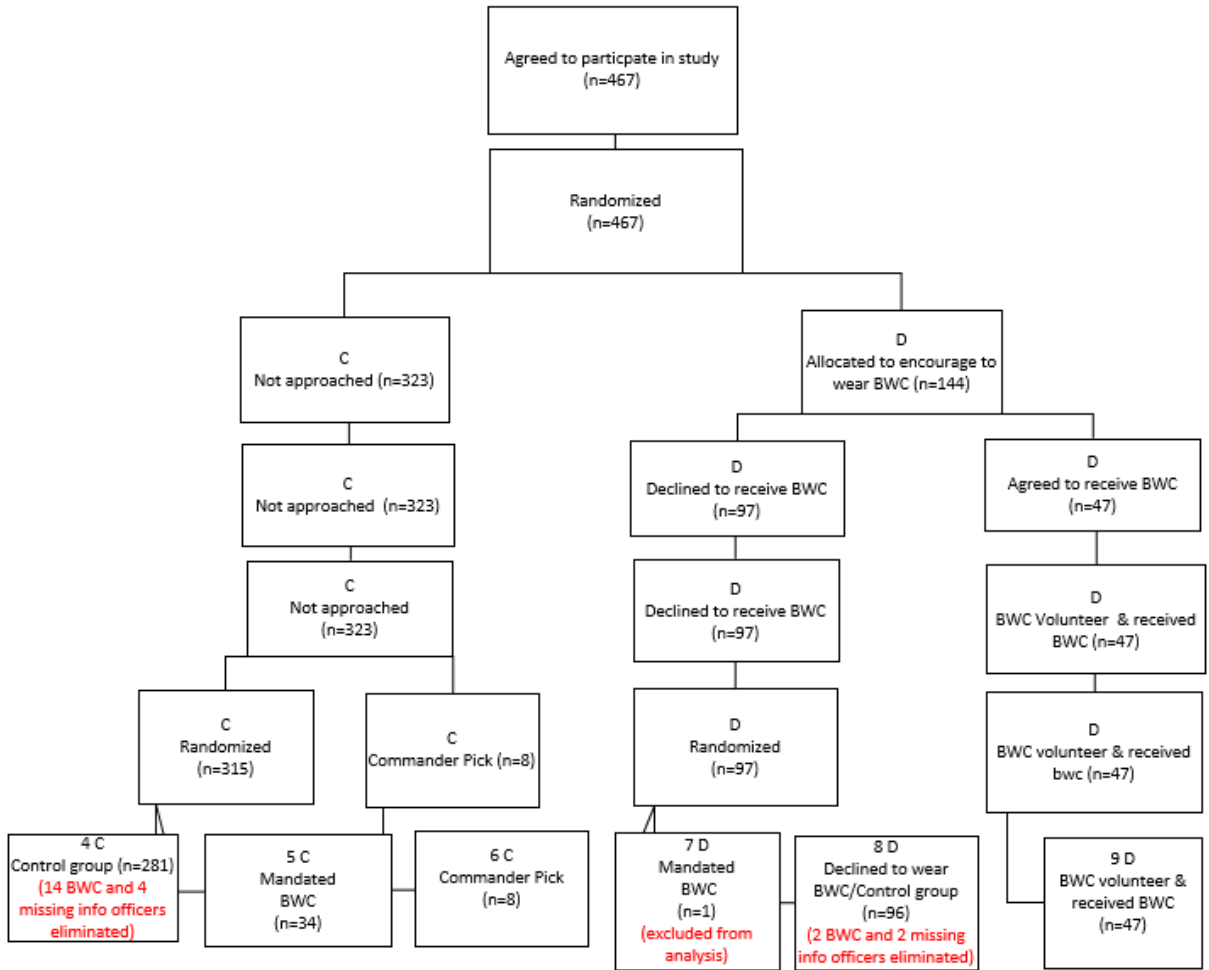
BWC deployment had a more notable impact on citizen complaints. Officers who resisted wearing a BWC had significantly lower complaint rates after BWCs were deployed ( $p<.01$ ). The post-hoc analysis suggests this could be related to reductions in allegations that the officer was rude ( $g=-0.29$ , not significant), compared to control officers. Though the reason for these differences is unknown, it is possible that resisters were more aware of their behavior after BWCs were deployed because they had the potential to wear a BWC themselves. This could have resulted in resisters being more thoughtful about their behavior than control officers who were not asked to wear a BWC. Officers who were randomly selected and mandated to wear a BWC also had a significant reduction in their complaint rate, relative to control officers ( $p<.01$ ). The post-hoc analysis suggests that this reduction is not attributable to decreased complaints about rudeness, abuse of authority, or use of force. As such, the reduction appears to be linked to a more general decline in complaints against mandated officers after BWC deployment. Unlike resisters and mandated officers, officers who volunteered to wear a BWC had a significantly higher complaint rate after BWCs were deployed ( $p<.01$ ). The post-hoc analysis suggests this could be driven by a significant increase in allegations regarding officer use of force filed against volunteers ( $p<.01$ ). However, as discussed next, this significant increase in citizen complaints alleging volunteers used force against them is not associated with a significant increase in officially reported use of force.

Finally, there were no significant differences in officer use of force between resisters, mandated, and volunteer officers following the deployment of BWCs. However, the results suggest that both officers who resisted wearing a BWC ( $g=0.26$ ) and officers who volunteered to wear a BWC ( $0.21$ ) increased their use of force rate to a small degree after BWCs were deployed. It is unclear why resisters would increase their use of force rate relative to control officers. Relative to control officers, the findings suggest that BWC volunteers were responding to a higher number of dispatched calls and were more likely to self-initiate contacts with citizens. This increased activity might partially explain the increase in the number of use of force incidents that BWC volunteers engaged in ( $g=0.22$ ). In terms of the increased use of force rate, some scholars have suggested that BWCs could increase force because officers feel like they have stronger evidence to support their use of force (Owens & Finn, 2017). Other scholars suggest BWCs could result in increased reporting of use of force incidents that were previously

occurring (Henstock & Ariel, 2017). We can only speculate about whether either of these explanations apply to Phoenix PD officers.

Overall, our findings suggest that BWC deployment did not uniformly impact the different groups of officers we examine. Further, the way BWCs were deployed (either to officers who wore a BWC voluntarily or to those who were mandated to wear a BWC) appears to differentially affect outcomes. Several of our notable findings in relation to officer proactivity and citizen complaints occurred in opposite directions for officers mandated to wear a BWC than officers who volunteer to wear a BWC. For instance, BWC volunteers became more proactive after receiving a BWC while officers mandated to wear a BWC became less proactive after being assigned a camera. As BWCs are deployed in the rest of the department, the way these cameras are assigned to officers should be considered.

## Appendix A: Intervention design



Appendix B shows balance statistics comparing standardized differences between the raw data and the weighted data for the resisters, mandated, and volunteer officers compared to the control group. Standardized differences exceeding  $|0.25|$  indicate imbalance between the treatment group and the control group. The raw standardized differences show balance between groups prior to the use of propensity score weighting. The weighted standardized differences show whether covariate balance improved after the application of the propensity score weights. The weighted standardized differences show potential imbalance between officers in the resistant and control groups in terms of the number of dispatched calls officers responded to, prior to BWC deployment (standardized difference=-0.29). Even after weighting, it appears the mandated and control officers are imbalanced on race ethnicity (standardized difference=-0.31), assignment to Central City (standardized difference=-0.53), and the number of arrests officers engaged in prior to BWC deployment (standardized difference=0.32). Officers in the volunteer group are imbalanced on race/ethnicity (standardized difference=-0.33) relative to the control group, even after applying the propensity score weights.

**Appendix B: Balance statistics standardized differences**

Variables	Resistor		Mandated		Volunteer	
	Raw	Weighted	Raw	Weighted	Raw	Weighted
Sex	0.21	-0.11	-0.02	-0.01	-0.05	0.03
Race/ethnicity	0.04	0.11	-0.23	-0.31	-0.22	-0.33
Highest education completed	-0.03	0.07	0.10	0.03	-0.34	-0.16
Black Mountain	-0.19	-0.02	0.29	0.09	-0.20	-0.07
South Mountain	-0.42	0.10	-0.15	0.09	0.07	-0.03
Central City	0.76	-0.18	-0.15	-0.53	0.41	-0.16
Desert Horizon	-0.13	-0.01	-0.07	0.08	0.09	0.09
Mountain View	-0.66	0.05	-0.36	0.07	-0.63	0.11
Cactus Park	0.63	0.02	0.35	-0.02	0.34	-0.01
Age	-0.22	-0.08	0.00	0.21	-0.13	-0.13
Years of Service	-0.15	-0.21	0.13	0.12	-0.21	-0.19
# dispatched (pre)	0.10	-0.29	0.52	0.16	0.09	-0.15
Response time (pre)	-0.06	0.16	0.01	-0.15	0.08	-0.11
# self-initiated (pre)	0.06	-0.18	0.39	0.10	-0.01	0.04
% self-initiated (pre)	0.00	-0.02	0.11	0.05	-0.01	0.16
# arrest (pre)	-0.04	0.04	0.35	0.32	0.24	0.13
% arrest (pre)	-0.09	0.24	-0.17	0.04	0.24	0.17
# complaints (pre)	0.09	-0.14	0.25	0.17	0.16	-0.06
Complaint rate/arrest (pre)	0.11	-0.08	0.20	0.15	0.22	-0.04
# use of force (pre)	0.16	0.04	-0.10	0.18	-0.06	0.10
Use of force rate/arrest (pre)	0.08	0.00	-0.20	0.08	0.10	-0.01



### Appendix C: Incident type coding

Call type	Call code description
Violent	ABS/NEG OF VULNERABLE ADULT DV
Violent	ABS/NEG OF VULNERABLE ADULT DV SUP
Violent	ABUSE/NEG OF VULNERABLE ADULT
Violent	ABUSE/NEG OF VULNERABLE ADULT SUPPL
Violent	AGGRAVATED ASSAULT
Violent	AGGRAVATED ASSAULT ATTEMPT
Violent	AGGRAVATED ASSAULT SUPPLEMENT
Violent	ARMED ROBBERY
Violent	ARMED ROBBERY ALARM
Violent	ARMED ROBBERY ALARM BEACON
Violent	ARMED ROBBERY ALARM SUPPLEMENT
Violent	ARMED ROBBERY ATTEMPT
Violent	ARMED ROBBERY SUPPLEMENT
Violent	ASSAULT
Violent	ASSAULT ATTEMPT
Violent	ASSAULT SUPPLEMENT
Violent	BIGAMY ADULTERY ETC ATTEMPT
Violent	BIGAMY, ADULTERY, ETC
Violent	BOMB THREAT
Violent	CHILD ABUSE
Violent	CHILD ABUSE ATTEMPT
Violent	CHILD ABUSE SUPPLEMENT
Violent	CHILD NEGLECT
Violent	CHILD NEGLECT ATTEMPT
Violent	CHILD NEGLECT SUPPLEMENT
Violent	CHLDPORN/EXPLOIT SUPPL
Violent	CMP CHLDPORN/EXPLOIT
Violent	CONSPIRACY TO MURDER
Violent	CUTTING ATTEMPT
Violent	CUTTING SUPPLEMENT
Violent	CUTTING/STABBING
Violent	DOMESTIC VIOLENCE
Violent	DOMESTIC VIOLENCE ATTEMPT
Violent	DOMESTIC VIOLENCE SUPPLEMENT
Violent	FIGHT
Violent	FIGHT SUPPLEMENT
Violent	HOMICIDE
Violent	HOMICIDE - OTHER AGENCY ASST SUPP
Violent	HOMICIDE ATTEMPT

Call type	Call code description
Violent	HOMICIDE SUPPLEMENT
Violent	HOMICIDE-OTHR AGCY ASST
Violent	INDECENT EXPOSURE
Violent	INDECENT EXPOSURE ATTEMPT
Violent	INDECENT EXPOSURE SUPPLEMENT
Violent	KDNAP SEX AD/JV-NOSP
Violent	KIDNAP SEXUALLY MOTIVATED ATTEMPT
Violent	KIDNAP SEXUALLY MOTIVATED SUPPL
Violent	KIDNAPPING ATTEMPT
Violent	KIDNAPPING NON-SEXUALLY MOTIVATED
Violent	KIDNAPPING SEXUALLY MOTIVATED
Violent	KIDNAPPING SUPPLEMENT
Violent	LURE MINOR FOR SEX
Violent	MISUSE OF WEAPON SUPPLEMENT
Violent	MOLESTING
Violent	MOLESTING ATTEMPT
Violent	MOLESTING SUPPLEMENT
Violent	OFFICER INVOLVED SHOOTING
Violent	PEEPING TOM
Violent	RECKLESS ENDANGERMENT
Violent	RECKLESS ENDANGERMENT SUPPLEMENT
Violent	ROBBERY HOME INVASION
Violent	SEX ABUSE OF JUVENILE SUPPLEMENT
Violent	SEX OFFENDER REGISTRATION VIOLATION
Violent	SEXTING
Violent	SEXUAL ABUSE - ADULT
Violent	SEXUAL ABUSE OF ADULT ATTEMPT
Violent	SEXUAL ABUSE OF ADULT SUPPLEMENT
Violent	SEXUAL ABUSE OF JUVENILE
Violent	SEXUAL ABUSE OF JUVENILE ATTEMPT
Violent	SEXUAL ASSAULT
Violent	SEXUAL ASSAULT ATTEMPT
Violent	SEXUAL ASSAULT SUPPLEMENT
Violent	SHOOTING
Violent	SHOOTING SUPPLEMENT
Violent	SHOTS FIRED
Violent	SHOTS FIRED SUPPLEMENT
Violent	SOLICITATION DOOR TO DOOR
Violent	STRONG ARMED ROBBERY
Violent	STRONG ARMED ROBBERY ATTEMPT

Call type	Call code description
Violent	STRONG ARMED ROBBERY SUPPLEMENT
Violent	SUBJECT THREATENING SUPPLEMENT
Violent	SUBJECT WITH A GUN
Violent	SUBJECT WITH A GUN SUPPLEMENT
Violent	SUBJECT WITH A KNIFE
Violent	SUBJECT WITH A KNIFE SUPPLEMENT
Violent	SUICIDE
Violent	SUICIDE ATTEMPT
Violent	SUICIDE SUPPLEMENT
Violent	WEAPON MISUSE/VIOLATION
Property	ARSON
Property	ARSON ATTEMPT
Property	ARSON SUPPLEMENT
Property	BAIT VEHICLE
Property	BURG COM METAL THFT REL
Property	BURG COM METAL THFT REL ATTMPT
Property	BURG COM METAL THFT REL SUPP
Property	BURG FRM VEH CATL CNVTR
Property	BURG RES METAL THFT REL
Property	BURG RES METAL THFT REL ATTMPT
Property	BURGLARY
Property	BURGLARY - UNSPECIFIED
Property	BURGLARY ALARM
Property	BURGLARY ALARM SUPPLEMENT
Property	BURGLARY ATTEMPT
Property	BURGLARY COMMERCIAL
Property	BURGLARY COMMERCIAL ATTEMPT
Property	BURGLARY COMMERCIAL SUPPLEMENT
Property	BURGLARY FROM VEHICLE
Property	BURGLARY FROM VEHICLE ATTEMPT
Property	BURGLARY FROM VEHICLE SUPPLEMENT
Property	BURGLARY OF VENDING MACHINE
Property	BURGLARY OF VENDING MACHINE SUPPLEMENT
Property	BURGLARY RESIDENTIAL
Property	BURGLARY RESIDENTIAL ATTEMPT
Property	BURGLARY RESIDENTIAL SUPPLEMENT
Property	BURGLARY SUPPLEMENT
Property	COUNTERFEIT CURRENCY
Property	COUNTERFEIT CURRENCY SUPPLEMENT
Property	CRIMINAL DAMAGE

Call type	Call code description
Property	CRIMINAL DAMAGE ATTEMPT
Property	CRIMINAL DAMAGE SUPPLEMENT
Property	EXTORTION
Property	FINANCIAL EXPLOITATION - ELDERLY ATTEMPT
Property	FINANCIAL EXPLOITATION - ELDERLY SUPP
Property	FINANCIAL EXPLOITATION OF ELDERLY
Property	FORGERY
Property	FORGERY AT BANK/CREDIT UNION
Property	FORGERY ATTEMPT
Property	FORGERY SUPPLEMENT
Property	IDENTITY THEFT
Property	IDENTITY THEFT ATTC
Property	IDENTITY THEFT SUPPLEMENT
Property	INTERNET/COMPUTER FRAUD
Property	LARC
Property	METAL THFT REL SUPP
Property	MONEY LAUNDERING
Property	MONEY LAUNDERING ATTEMPT
Property	NONSUFFICIENT FUND CHECK SUPPLEMENT
Property	NONSUFFICIENT FUNDS CHECK
Property	PURSE SNATCH
Property	PURSE SNATCH ATTEMPT
Property	PURSE SNATCH SUPPLEMENT
Property	SHOPLIFTING
Property	SHOPLIFTING ATTEMPT
Property	SHOPLIFTING SUPPLEMENT
Property	STOLEN BICYCLE
Property	STOLEN BICYCLE ATTEMPT
Property	STOLEN BICYCLE SUPPLEMENT
Property	STOLEN POLICE CAR
Property	STOLEN POLICE CAR SUPPLEMENT
Property	STOLEN PROPERTY
Property	STOLEN PROPERTY ATTEMPT
Property	STOLEN PROPERTY SUPPLEMENT
Property	STOLEN VEHICLE
Property	STOLEN VEHICLE ATTEMPT
Property	STOLEN VEHICLE ATTEMPT SUPPLEMENT
Property	STOLEN VEHICLE SUPPLEMENT
Property	THEFT
Property	THEFT ATTEMPT

Call type	Call code description
Property	THEFT BY FRAUD
Property	THEFT BY FRAUD ATTEMPT
Property	THEFT BY FRAUD SUPPLEMENT
Property	THEFT CATALYTIC CONVERTER
Property	THEFT FROM VEHICLE
Property	THEFT FROM VEHICLE ATTEMPT
Property	THEFT FROM VEHICLE SUPPLEMENT
Property	THEFT OF CREDIT CARD
Property	THEFT OF CREDIT CARD ATTEMPT
Property	THEFT OF CREDIT CARD SUPPLEMENT
Property	THEFT OF METAL
Property	THEFT SUPPLEMENT
Property	TRESPASSING
Property	TRESPASSING SUPPLEMENT
Property	WATCH CAR AUTO THEFT
Property	WATCH CAR AUTO THEFT SUPPLEMENT
Disorder	ANIMALS DISTURBING
Disorder	CITY ORDINANCE OFFENSE
Disorder	CITY ORDINANCE OFFENSE SUPPLEMENT
Disorder	CONTR TO DEL MINOR ATTEMPT
Disorder	CONTRIBUTE DELINQUENCY MINOR SUPPL
Disorder	CONTRIBUTE TO DELINQUENCY OF MINOR
Disorder	CURFEW VIOLATION
Disorder	CYBERBULLYING
Disorder	DANGEROUS DRUGS
Disorder	DANGEROUS DRUGS SUPPLEMENT
Disorder	DRUNK DISTURBING/DOWN/CAR
Disorder	DRUNK DRIVER
Disorder	DRUNK DRIVER ATTEMPT
Disorder	DRUNK DRIVER SUPPLEMENT
Disorder	ESCORT LICENSE VIOLATION
Disorder	ESCORT VIOLATION
Disorder	FOUND NARCOTICS
Disorder	FOUND NARCOTICS SUPPLEMENT
Disorder	GAMBLING
Disorder	GAMBLING ATTEMPT
Disorder	GLUE SNIFFING
Disorder	GRAFFITI
Disorder	GRAFFITI SUPPLEMENT
Disorder	HARASSMENT

Call type	Call code description
Disorder	HARASSMENT SUPPLEMENT
Disorder	ILLEGAL DUMPING
Disorder	ILLEGAL DUMPING ATTEMPT
Disorder	ILLEGAL DUMPING SUPPLEMENT
Disorder	INCORRIGIBLE JUVENILE
Disorder	INCORRIGIBLE JUVENILE SUPPLEMENT
Disorder	INJURED ANIMALS
Disorder	INJURED ANIMALS ATTEMPT
Disorder	INJURED ANIMALS SUPPLEMENT
Disorder	INSANE PERSON
Disorder	INSANE PERSON SUPPLEMENT
Disorder	JUVENILES DISTURBING
Disorder	JUVENILES DISTURBING SUPPLEMENT
Disorder	LIQUOR VIOLATION
Disorder	LIQUOR VIOLATION SUPPLEMENT
Disorder	LOITERING
Disorder	LOOSE ANIMALS
Disorder	LOOSE ANIMALS SUPPLEMENT
Disorder	LOUD NOISE DISTURBANCE
Disorder	LOUD NOISE DISTURBANCE-BROADCAST
Disorder	LOUD PARTY DISTURBANCE
Disorder	MARIJUANA REPORT
Disorder	MARIJUANA REPORT SUPPLEMENT
Disorder	MASSAGE VIOLATION
Disorder	MENTALLY ILL SUBJECT TRANSPORT
Disorder	NARCOTICS
Disorder	NARCOTICS SUPPLEMENT
Disorder	NARCOTICS SUPPLEMENT
Disorder	OBSTRUCTING THOROUGHFARE
Disorder	OVERDOSE BROADCAST
Disorder	OVERDOSE VICTIM
Disorder	OVERDOSE VICTIM SUPPLEMENT
Disorder	PANDERING/PIMPING - ADULT
Disorder	PRESCRIPTION VIOLATION
Disorder	PRESCRIPTION VIOLATION ATTEMPT
Disorder	PRESCRIPTION VIOLATION SUPPLEMENT
Disorder	PROSTITUTION
Disorder	PROSTITUTION - CHILD
Disorder	PROSTITUTION - ILLEGAL ENTERPRISE
Disorder	RESISTING ARREST

Call type	Call code description
Disorder	SOLICIT FOR PROSTITUTION
Disorder	SOLICIT FOR PROSTITUTION ATTEMPT
Disorder	SOLICITING
Disorder	SOLICITING ATTEMPT
Disorder	SUBJECT THREATENING
Disorder	SUSP PERSON IN VEHICLE BROADCAST
Disorder	SUSPICIOUS PERSON
Disorder	SUSPICIOUS PERSON IN VEHICLE
Disorder	SUSPICIOUS PERSON IN VEHICLE SUPPLEMENT
Disorder	SUSPICIOUS PERSON SUPPLEMENT
Disorder	TRUANCY
Disorder	TRUANCY SUPPLEMENT
Disorder	UNDERAGE LIQUOR SUPPLEMENT
Disorder	UNDERAGE LIQUOR VIOLATION
Disorder	URINATE IN PUBLIC SUPPLEMENT
Disorder	URINATING IN PUBLIC
Traffic violations	ABANDONED VEHICLE
Traffic violations	ABANDONED VEHICLE SUPPLEMENT
Traffic violations	ACCIDENT FATALITY
Traffic violations	ACCIDENT FATALITY SUPPLEMENT
Traffic violations	ACCIDENT NO INJURIES
Traffic violations	ACCIDENT NO INJURIES SUPPLEMENT
Traffic violations	ACCIDENT WITH INJURIES
Traffic violations	ACCIDENT WITH INJURIES SUPPLEMENT
Traffic violations	FELONY FLIGHT
Traffic violations	FELONY FLIGHT SUPPLEMENT
Traffic violations	HIT & RUN FATALITY
Traffic violations	HIT & RUN ACCIDENT NO INJURIES SUPP
Traffic violations	HIT & RUN ACCIDENT NO INJURY
Traffic violations	HIT & RUN ACCIDENT W/ INJURY SUPPLEMENT
Traffic violations	HIT & RUN ACCIDENT WITH INJURIES
Traffic violations	HIT & RUN FATALITY SUPPLEMENT
Traffic violations	ILLEGAL PARKING
Traffic violations	SPEEDING BROADCAST
Traffic violations	SPEEDING/RACING
Traffic violations	SPEEDING/RACING SUPPLEMENT
Traffic violations	STOLEN LICENSE PLATE
Traffic violations	TRAFFIC CONTROL
Traffic violations	TRAFFIC HAZARD
Traffic violations	VEHICLE SEIZURE

Call type	Call code description
Subject/vehicle stop	FELONY WARRANT OUTSTANDING
Subject/vehicle stop	FOJ FELONY WARRANT
Subject/vehicle stop	FOJ MISDEMEANOR WARRANT
Subject/vehicle stop	LOCATE ONLY VEHICLE
Subject/vehicle stop	LOCATE ONLY VEHICLE SUPPLEMENT
Subject/vehicle stop	MISDEMEANOR WARRANT OUTSTANDING
Subject/vehicle stop	SUBJECT STOP
Subject/vehicle stop	VEHICLE STOP
Other	9-1-1 HANG-UP CALL
Other	911 HU CLEARED CALL
Other	ACCESS INTERFERENCE
Other	ACCESS INTERFERENCE SUPPLEMENT
Other	ASSIST MOTORIST
Other	BA OPERATOR NEEDED
Other	BACK-UP
Other	BARRICADE
Other	BOMB SCARE SUPPLEMENT
Other	BOMB THREAT ATTEMPT
Other	CALL BY PHONE
Other	CHECK WELFARE
Other	CHECK WELFARE SUPPLEMENT
Other	CIVIL MATTER SUPPLEMENT
Other	CIVIL MATTER/STANDBY
Other	COMMUNITY ENGAGEMENT
Other	CRIME LAB PRINT SPECIALIST
Other	CRIME STOP HU
Other	CUSTODIAL INTERFERENCE
Other	CUSTODIAL INTERFERENCE ATTEMPT
Other	CUSTODIAL INTERFERENCE SUPPLEMENT
Other	CYBER STALKING/COMPUTER TAMPERING
Other	DEAD BODY
Other	DEAD BODY SUPPLEMENT
Other	DUI DRIVER-BROADCAST
Other	EMERGENCY MESSAGE
Other	ESCAPE
Other	ESCAPE ATTEMPT
Other	ETA REQUEST
Other	FALSE REPORTING
Other	FALSE REPORTING ATTEMPT
Other	FALSE REPORTING SUPPLEMENT



Call type	Call code description
Other	FEMALE OFFICER FOR SEARCH
Other	FIRE FOLLOW-UP
Other	FOUND BICYCLE
Other	FOUND BICYCLE SUPPLEMENT
Other	FOUND EXPLOSIVES
Other	FOUND MISSING PERSON
Other	FOUND MISSING PERSON SUPPLEMENT
Other	FOUND PROPERTY
Other	FOUND PROPERTY SUPPLEMENT
Other	GENERIC
Other	GENERIC BROADCAST
Other	HARASSING PHONE CALLS
Other	HARASSING PHONE CALLS SUPPLEMENT
Other	HAZARDOUS MATERIALS
Other	HUMAN SMUGGLING
Other	ILLEGAL BURNING
Other	ILLEGAL BURNING ATTEMPT
Other	IMMIGRATION MATTER
Other	INFORMATION CALL
Other	INJURED/SICK PERSON
Other	INJURED/SICK PERSON BROADCAST
Other	INJURED/SICK PERSON SUPPLEMENT
Other	INT/CMP HACK/INTRU
Other	INTENSIVE PATROL
Other	INTERNET COMPUTER CRIME
Other	JUDICIAL INTERFERENCE
Other	JUDICIAL INTERFERENCE ATTEMPT
Other	JUDICIAL INTERFERENCE SUPPLEMENT
Other	LANDLORD/TENANT DISPUTE
Other	LANDLORD/TENANT DISPUTE SUPPLEMENT
Other	LOSS REPORT
Other	LOSS REPORT SUPPLEMENT
Other	MEET
Other	MISSING JUVENILE
Other	MISSING JUVENILE SUPPLEMENT
Other	MISSING PERSON
Other	MISSING PERSON SUPPLEMENT
Other	MOBILE ALARM
Other	NEIGHBOR DISPUTE
Other	NEIGHBOR DISPUTE SUPPLEMENT

Call type	Call code description
Other	NO FURTHER ACTION
Other	NOTIFY OWNER OF VEHICLE RECOVERY
Other	NOTIFY PARENT OF JUVENILE DETENTION
Other	NUCLEAR/BIOLOGICAL/CHEM SITUATION
Other	OFFICER NEEDS HELP
Other	OPEN DOOR, WINDOW SUPPLEMENT
Other	OPEN DOOR, WINDOW, GATE
Other	ORGANIZED CRIME/CONSPIRACY
Other	PD AIR UNIT DOWN WITH INJURIES
Other	PICK UP PAPERS
Other	PR CONTACT
Other	PRONET ALARM
Other	PROWLER
Other	PROWLER SUPPLEMENT
Other	RADIOACTIVE MATERIAL
Other	REC F.O.J./PROPERTY SUPPLEMENT
Other	RECOVERED BICYCLE
Other	RECOVERY F.O.J./PROPERTY
Other	RECOVERY OF VEHICLE
Other	RECOVERY OF VEHICLE ATTEMPT
Other	RECOVERY OF VEHICLE SUPPLEMENT
Other	SHOTS FIRED BROADCAST
Other	STALKING
Other	STALKING ATTEMPT
Other	STALKING SUPPLEMENT
Other	STREET VENDING VIOLATION
Other	TEST CALL
Other	THEFT BROADCAST
Other	THREAT
Other	THREAT SUPPLEMENT
Other	TOW REQUEST
Other	TRANSFER CALL TO SUPERVISOR
Other	TRANSFER PHONE CALL
Other	TRANSFER TO FIRE
Other	TRANSLATION DETAIL
Other	UNDETERMINED FIRE
Other	UNKNOWN TROUBLE
Other	UNWANTED GUEST
Other	UNWANTED GUEST SUPPLEMENT
Other	WAGON WANTED

