

THE IMPACT OF BODY-WORN CAMERAS ON COMPLAINTS AGAINST OFFICERS AND OFFICER USE OF FORCE INCIDENT REPORTS: PRELIMINARY EVALUATION FINDINGS

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THESE RESULTS ARE PRELIMINARY AND SUBJECT TO CHANGE AS DATA COLLECTION PROCEEDS AND FURTHER ANALYSES ARE COMPLETED.

SUMMARY

- The Boston Police Department collaborated with Northeastern University to develop a randomized controlled trial of its pilot implementation of 100 body worn cameras on patrol officers in 5 police districts and plainclothes officers in the Youth Violence Strike Force.
- The Northeastern research team randomly allocated 281 officers into treatment (camera wearers) and control groups from these assignments. The selected officers worked the day and first half shifts and were actively providing police services to Boston residents.
- The randomization procedure generated treatment (140 officers) and control (141 officers) groups that were equivalent in terms of officer sex, race, age, years on the job, shift, assignment, prior complaints, and prior use of force reports. All treatment officers were trained on the body worn camera policy and the use of the technology.
- At the commencement of the pilot program, 100 of the 140 officers trained on the use of body worn cameras were assigned to wear the cameras. Over the course of the one-year intervention period, 21 officers stopped wearing the cameras due to promotions, assignment changes, medical incapacitation, resignation, and retirement. A total of 121 of the 140 treatment officers wore cameras during the pilot program.
- The preliminary findings of the randomized controlled trial suggest that the placement of body worn cameras on Boston Police officers may generate small benefits to the civility of police-citizen civilian encounters. Relative to control officers, treatment officers received fewer citizen complaints and generated fewer use of force reports.
- Statistical analysis revealed that the impact of body worn cameras on complaints was small but statistically-significant at a less restrictive statistical threshold. The results suggest a reduction of one complaint per month for 140 treatment officers relative to 141 control officers. The analysis indicated body worn cameras generated a small reduction in officer use of force reports that was not statistically-significant, suggesting no meaningful difference between the treatment and control groups.
- These preliminary results are not final and should be interpreted with caution. The evaluation team will continue to collect data and pursue supplemental analyses to ensure that these findings are robust to different tests and model specifications. The final report will present completed analyses of the impact of body worn cameras on citizen complaints and officer use of force reports as well as analyses of impacts on police proactivity, lawfulness of police enforcement actions, and police-community relations.

Introduction

In January 2015, the Boston Police Department (BPD) committed to implement a pilot body worn camera (BWC) program for its officers. This pilot was intended to help answer policy questions about how the system would operate if and when fully implemented across the department's 2,100 officers and to address concerns of officers and community members on the use of the technology. Boston Mayor Martin Walsh and Boston Police Commissioner William Evans committed to a rigorous evaluation of this pilot program. The BPD implemented its BWC pilot program in September 2016. This pilot involved the random allocation of 100 BWCs to officers who wore these cameras for a twelve month intervention period. The impact evaluation uses a rigorous randomized controlled trial (RCT) design to evaluate the impact of BWCs on police-citizen interactions, police proactivity, police lawfulness, and police-community relations.¹ RCTs are generally considered the “gold standard” in program evaluation as these designs allow researchers to assume that the only systematic difference between the control and treatment groups is the presence of the intervention; this permits a clear assessment of program impacts on outcome measures.

This preliminary impact evaluation report summarizes the randomization of officers to treatment and control groups, assesses the balance between experimental groups, examines attrition in the treatment group, and estimates the statistical power of the experimental design. The report then presents preliminary evaluation findings for the impact of the BWC technology on citizen complaints against officers and officer use of force incident reports.

¹ Shadish, William, Thomas Cook, & Donald Campbell. (2002). *Experimental and Quasi-Experimental Designs for Generalized Causal Inference*. Boston: Houghton Mifflin.

Randomization Procedure and Assessing Experimental Group Balance

The BPD provides policing services directly to Boston residents through 11 district stations. The Youth Violence Strike Force (YVSF, informally known as the “gang unit”) is comprised of plainclothes officers who engage proactive policing tactics to prevent outbreaks of gang violence. Ten districts were matched into 5 pairs based on a range of relevant variables including crime, calls for service, arrests, field interrogation / observation (FIO) reports, citizen complaints, number of officers assigned, population demographics, and levels of neighborhood disadvantage. As part of the initial design work with the BPD, the research team randomly allocated one district from each matched pair to the BWC treatment group. YVSF was also non-randomly assigned to the BWC treatment group.²

A key aspect of the design work for the RCT involved the random allocation of the BWC technology to officers within the 5 treatment districts and YVSF. The BPD provided the research team with a database of N=281 eligible officers from these assignments who worked the day (8:01 AM – 4:00 PM) and first half (4:01 PM – 12:00 AM) shifts as of September 1, 2016. The BPD excluded officers were responsible for administrative duties, medically-incapacitated, on military leave, or assigned to other responsibilities that did not primarily involve law enforcement work on the street. The database included information on age, race, sex, and time on the job. The research team also collected information on citizen complaints and officer use of force incidents generated by these officers for three years prior to the start of the pilot program (2013-2015) through databases maintained by the BPD Bureau of Professional Standards.

² The non-random selection of the YVSF stemmed from two complementary interests. First, the BPD wanted to develop policy and programmatic information on the issues involved in assigning cameras to plainclothes officers relative to uniformed officers. Second, during conversations with the Social Justice Task Force and other community groups on the BWC implementation, community leaders generally recognized YVSF as a key BPD unit engaged in proactive policing activities centered on youth living in disadvantaged minority neighborhoods. These leaders requested that YVSF officers also wear BWCs.

A computer algorithm was used to randomly allocate N=281 officers to treatment and control groups within the 5 treatment districts and YVSF. The initial randomization was used to divide the officers into two nearly equivalent-sized experimental groups (N=140 treatment officers and N=141 control officers). N=100 officers within the treatment group were then randomly assigned to wear the BWC technology at the outset of the pilot program; the available BWCs were randomly allocated across the 5 treatment districts and YVSF proportionate to the number of officers in each assignment.³ The BPD was committed to maintaining 100 active BWC officers working in Boston communities for the entire twelve month pilot program. As such, the N=40 treatment officers that did not receive BWCs were trained in the BWC policy and operations of the technology. As described below, these officers served as “alternates” to the treatment officers outfitted with BWCs as attrition occurred over the course of the study period.

Randomization provides a simple and convincing method for achieving comparability in the treatment and control groups.⁴ If randomization is done correctly, the only systematic difference between treatment and control groups should be the presence or absence of the treatment. To test the balance between the treatment and control groups on key officer variables, we used independent samples *t* tests and standardized mean differences, known as Cohen’s *d*.⁵ Table 1 presents basic descriptive information on officers participating in the experiment and the results of these tests; for binary variables, means are expressed as percentages. A positive *t* test indicates that the treatment group has a higher mean than the control group. Covariate imbalance

³ The 100 BWC cameras were randomly allocated as follows: 20 officers in B-2, 20 officers in B-3, 17 officers in D-4, 16 officers in D-14, 17 officers in E-18, and 10 officers in YVSF.

⁴ Shadish, William, Thomas Cook, & Donald Campbell. (2002). *Experimental and Quasi-Experimental Designs for Generalized Causal Inference*. Boston: Houghton Mifflin.

⁵ Cohen, Jacob. (1988). *Statistical Power Analysis for the Behavioral Sciences*, Second edition. Hillsdale, NJ: Lawrence Erlbaum Associates.

would be exhibited by Cohen's $|d|$ in excess of .20 and a $|t|$ in excess of 1.96. The equality of variances was tested and confirmed for all variables. This reveals that the randomization created balanced treatment and control groups. The balanced treatment and control groups supports the internal validity of the design and suggests that the randomized controlled trial was well positioned to isolate the impact of body worn cameras on the study outcome measures.

Table 1. Summary Characteristics of Officers in Treatment and Control Groups, N = 281

<u>Officer Characteristics</u>	<u>Balance Diagnostics</u>		
	<u>Mean (SD)</u>	<u>t</u>	<u> d </u>
Treatment Group	49.8%	--	--
Control Group	50.2%		
Male	91.1%	0.19	.011
Female	8.9%		
White	65.1%	-1.04	-.062
Black	25.6%	0.58	.034
Hispanic	7.5%	0.70	.041
Asian / Other	1.8%	0.46	.027
Mean Age	40.4 (9.8)	-1.18	-.071
Mean Years on Job	12.2 (9.1)	-1.13	-.067
Mean Yearly Complaints	0.22 (.21)	1.01	.049
Mean Use of Force	0.12 (.19)	.12	.006
Day Shift (8:01 AM – 4:00 PM)	43.4%	0.53	.031
First Half (4:01 PM – 12:00 AM)	56.6%		
B-2	21.7%	-0.98	-.058
B-3	19.6%	.86	.042
D-4	18.9%	-0.73	-.043
D-14	13.9%	1.23	.073
E-18	13.5%	1.02	.050
YVSF	12.5%	-1.77	-.105

Note: N=140 officers in the treatment group and N=141 officers in the control group. SD = Standard Deviation.
+ = $p < .10$, * = $p < .05$, ** = $p < .01$

Attrition and Statistical Power

Attrition represents a threat to the internal validity of randomized experiments as it could affect the equivalence of treatment and control groups and introduce bias into the analysis of experimental data.⁶ In general, attrition from the BWC treatment group during the randomized controlled trial was very modest; only N=21 officers ceased wearing BWCs before the end of the intervention period. These N=21 treatment officers (14.9% attrition from N=140 treatment officer group) who were no longer participating in the BWC pilot program were replaced by trained alternates. Over the course of the one year pilot program, N=18 control officers (12.8% attrition from the N=141 control officer group) were no longer in an active duty assignment eligible for BWC use.

Table 2 presents the reasons for officer attrition from the randomized experiment. The officers left due to an assignment change that did not involve BWC use, medical incapacitation, promotion, and leaving the department via retirement or resignation. The N=21 treatment officers who left the program had worn the BWCs for mean =13.6 weeks (slightly more than 3 months), ranging from only 9 days to 28.6 weeks. As such, N=121 treatment officers actually used BWCs for varying time periods while performing their law enforcement duties during the pilot program (86.4% of the treatment group).

To address the observed attrition issue, we used intention-to-treat (ITT) analyses based on the initial random assignment to treatment.⁷ ITT analyses provide fair comparisons between

⁶ Campbell, Donald & Julian Stanley. (1963). *Experimental and Quasi-Experimental Designs for Research*. Chicago: Rand McNally.

⁷ In the final report, we plan to conduct supplemental analyses of the BWC impacts by estimating the impact of the treatment on the treated (TOT). Other analytical approaches, such as considering the length of time that BWCs were worn by the officers on observed outcomes, will also be pursued. We will also analyze BPD calls for service data to examine possible contamination of control conditions by the presence of treatment officers responding to specific calls for service handled by control officers.

treatment and control groups because it avoids the bias associated with the non-random loss of study participants.⁸ As such, all N=140 treatment officers and N=141 control officers were included in the analyses presented in this preliminary report. The relatively small number of officers in each group resulted in a research design with very modest statistical power. For a two-tailed test with $\alpha=.05$, this randomized controlled trial had an estimated statistical power of .39 to detect a small standardized effect size of .20.⁹

Table 2. Reasons for Officer Attrition from RCT

<u>Reason</u>	<u>Treatment Group</u>		<u>Control Group</u>	
	<u>N</u>	<u>Percent</u>	<u>N</u>	<u>Percent</u>
Assignment Change	10	47.6%	7	38.9%
Medically Incapacitated	5	23.8%	6	33.3%
Promotion	3	14.3%	3	16.7%
Retired/Resigned	3	14.3%	2	11.1%
Total	21	100.00%	18	100.0%

Citywide Trends in Citizen Complaints and Officer Use of Force Reports

Figure 1 presents the yearly citywide counts of citizen complaints filed against BPD officers between 2013 and 2016. The number of complaints decreased by 44.7% from 360 complaints in 2013 to 199 complaints in 2016. The research team will submit a request for 2017 full year citywide complaint data. However, there does seem to be a modest increase in complaints against all BPD officers when 12 month periods before and during the BWC pilot

⁸ Hollis, Sally & Fiona Campbell. (1999). "What is Meant by Intention to Treat Analysis? Survey of Published Randomised Controlled Trials," 319 *British Medical Journal* 670-674.

⁹ Lipsey, Mark. (1990). *Design Sensitivity: Statistical Power for Experimental Research*. Thousand Oaks, CA: Sage Publications.

program are compared. During the 12 month period prior to the implementation of BWCs (September 2015 – August 2016), there were N=181 complaints made against BPD officers. In the 12 month period following the launch of the BWC pilot program (September 2016 – August 2017), the number of complaints increased by 14.4% to N=207 complaints made against BPD officers.

Figure 1. Yearly Citywide Counts of Complaints Against Boston Police Officers, 2013 – 2016

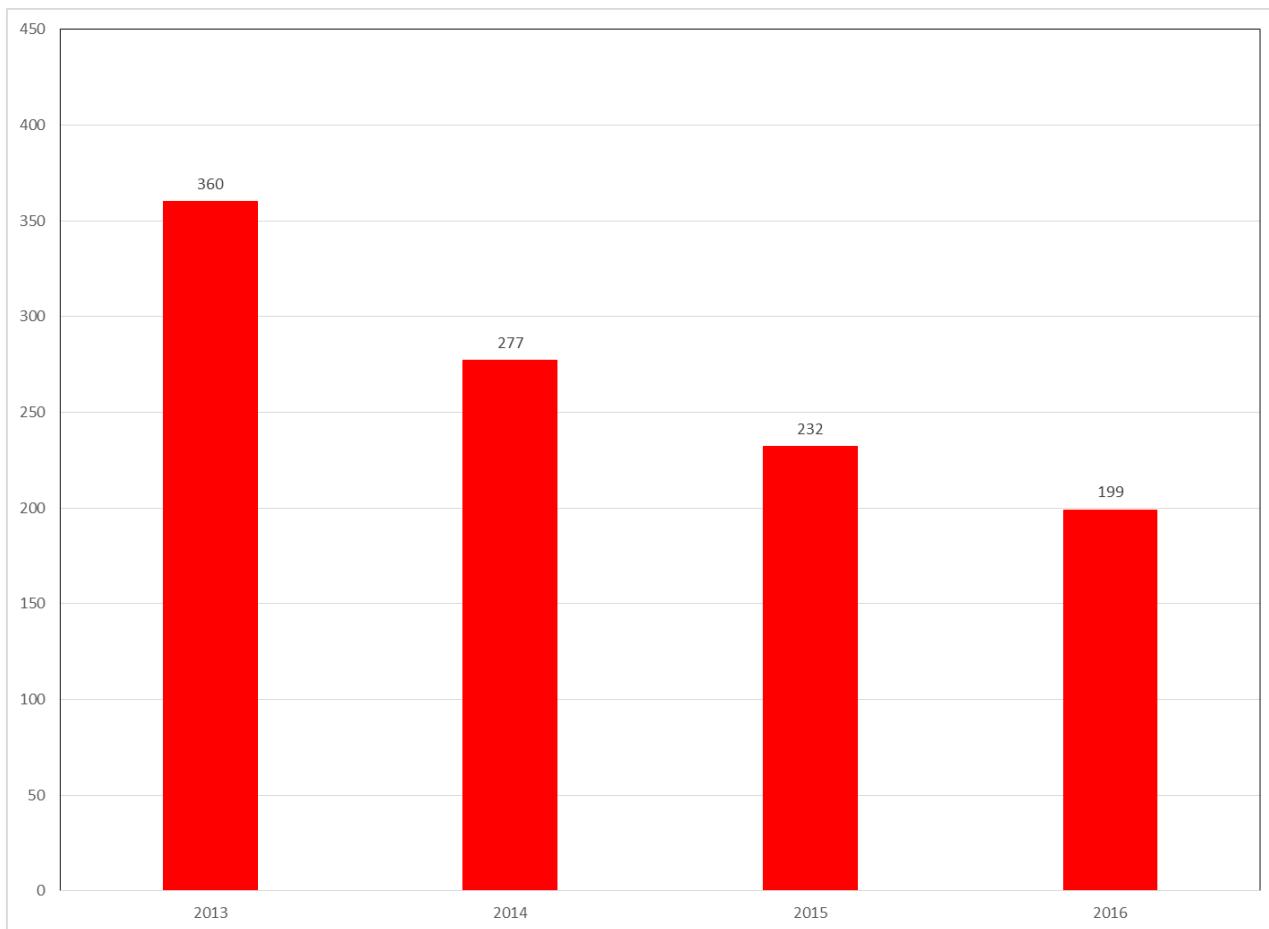
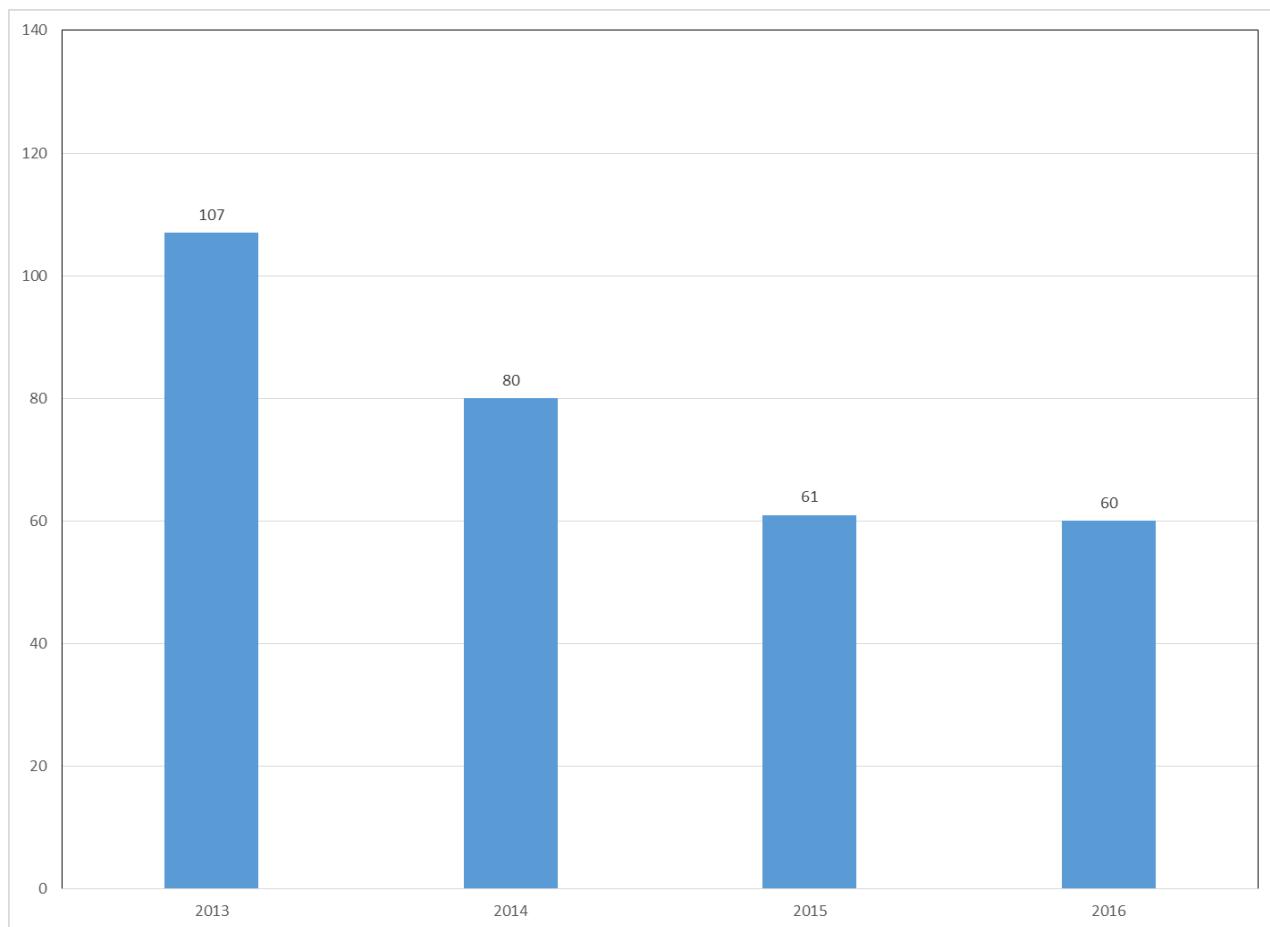


Figure 2 presents the yearly citywide counts of use of force incident reports generated by BPD officers between 2013 and 2016. The number of use of force reports decreased by 43.9%

from 107 reports in 2013 to 60 use of force reports in 2016. While full year 2017 data are not yet available, the number of use of force reports generated by BPD officers continued to decrease when 12 month periods before and during the BWC pilot program are compared. During the 12 month period prior to the implementation of BWCs (September 2015 – August 2016), there were N=58 use of force reports generated by BPD officers. In the 12 month period following the launch of the BWC pilot program (September 2016 – August 2017), the number decreased by 12.1% to N=51 use of force reports made by BPD officers.

Figure 2. Yearly Citywide Counts of Use of Force Reports Generated by Boston Police Officers, 2013 – 2016



Analytical Approach

As suggested by the citywide trends described above, citizen complaints against officers and use of force incident reports were rare events for BPD officers.¹⁰ The relatively small number of officers in the randomized experiment and the low base rates of citizen complaints and officer use of force reports makes it challenging to estimate the true impact of the BWC treatment. Indeed, during the one year period preceding inclusion in the randomized controlled trial, 82.9% of treatment officers (116 of 140) and 85.1% of control officers (120 of 141) did not experience a single citizen complaint, and 92.1% of treatment officers (129 of 140) and 88.6% of control officers (125 of 141) did not generate a single use of force report. When these events occurred, a large majority of treatment officers and control officers generated only a single incident during the 12 months immediately preceding the experiment.¹¹

Since randomized experiments control for confounding factors by design, analyses of experimental data do not require extensive statistical modeling to ensure rival causal influences are identified and controlled.¹² To test the impact of the treatment on officer complaints and use of force reports relative to control conditions during the intervention period, we used independent samples *t* tests and standardized mean differences (Cohen's *d*). Poisson regression models were then used to provide a supplementary estimate of the impact of BWCs on the counts

¹⁰ Other BWC evaluations have noted similar low base rates for citizen complaints and use of force reports. For instance, see Ariel, Barak, Tony Farrar, & Alex Sutherland. (2015). "The Effect of Police Body-Worn Cameras on Use of Force and Citizens' Complaints Against the Police: A Randomized Controlled Trial," 31 *Journal of Quantitative Criminology* 1–27.

¹¹ For instance, for the 24 treatment officers who experienced at least one complaint during the 12 month pre-intervention period, only three treatment officers (12.5% of 24) experienced more than one complaint (all three experienced two complaints each). For the 11 treatment officers who generated at least one use of force incident report during the 12 month pre-intervention period, only one treatment officer (9.1% of 11) generated more than one use of force reports (this officer generated two reports).

¹² Weisburd, David. (2010). "Justifying the Use of Non-Experimental Methods and Disqualifying the Use of Randomized Controlled Trials," 6 *Journal of Experimental Criminology* 209-227.

of complaints and use of force reports during the intervention period relative to their control counterparts.¹³ Using complaint reports as an example, the general equation was:

$$(1) \mu = t \exp(\beta_1 Intercept + \beta_2 Group + \beta_3 PreCount)$$

The dependent variable, μ , represents the Poisson incidence rate of complaints generated by an experimental officer during the twelve month intervention period. The regressor *PreCount* represents the count of complaints generated by experiment officers during the twelve month pre-intervention period. The regressor *Group* is a dummy variable identifying whether an officer was in the treatment group (1) or not (0). The omitted group comprises control officers in the experiment. The coefficient β_2 estimates the impact of BWCs on complaints counts for treatment officers relative to control officers during the intervention period holding complaint counts in the pre-intervention period constant. To ensure that the coefficient variances were robust to violations of the homoskedastic errors assumption of linear regression models, robust variance estimators were used.

Results

Table 3 presents the results of our analyses of the impact of BWC on citizen complaints for treatment officers relative to control officers. In general, these analyses suggested that the placement of BWCs on BPD officers seemed to reduce the incidence of citizen complaints. During the intervention period, the mean number of complaints received per officer in the treatment group (.121, or 17 complaints for N=140 treatment officers) relative to the control group (.206, or 29 complaints for N=141 control officers) was smaller. The absolute mean difference of .084 represents a total reduction of 12 complaints over the one year intervention

¹³ Postestimation goodness of fit tests confirmed that the count data fit a Poisson distribution. For complaint counts, Pearson goodness-of-fit = 272.2238, $p > \chi^2(278) = 0.5864$. For use of force counts, Pearson goodness-of-fit = 293.6395, $p > \chi^2(278) = 0.2485$.

period, or a reduction of one complaint per month for group of treatment officers relative to control officers. The standardized mean difference analysis suggested a very small impact of BWCs on citizen complaints ($d = -.104$) that was statistically significant at a less restrictive level ($p < .10$). The supplemental Poisson regression analysis also suggested a reduction in the incidence of complaints for treatment officers relative to control officers controlling for pre-intervention complaints against officers. However, this difference was also statistically significant at a less restrictive level ($p < .10$).

Table 3. Impact of BWC on Citizen Complaints

Standardized mean difference

	<u>N</u>	<u>Mean (SD)</u>	<u> d </u>	<u>t</u>	<u>p</u>
Treatment	140	.121 (.327)	.104	-1.74	0.083 ⁺
Control	141	.206 (.471)			

Poisson regression

N = 281

Log pseudolikelihood = -129.476

	<u>Coef.</u>	<u>RSE</u>	<u>t</u>	<u>p</u>
Group (1=Treatment)	-.549	.301	-1.82	0.068 ⁺
Pre-Intervention Complaints	.447	.281	1.59	0.111
Intercept	-1.667	.193	-8.61	0.000**

Note: SD = Standard Deviation, Coef. = Coefficient, RSE = Robust Standard Error

+ = $p < .10$, * = $p < .05$, ** = $p < .01$

Table 4 presents the results of our analyses of the impact of BWC on use of force reports generated by treatment officers relative to control officers. While there is some evidence suggestive of a beneficial impact, these analyses generally indicated that the placement of BWCs on BPD officers did not significantly reduce the number of use of force reports for treatment

officers when compared to control officers. During the intervention period, the mean number of force reports generated per officer in the treatment group (.06, or 8 reports for N=140 treatment officers) relative to the control group (.11, or 15 reports for N=141 control officers) was smaller. However, the standardized mean difference analysis suggested a small, non-significant impact of BWCs on use of force reports ($d = -.082$, $p = .169$). The supplemental Poisson regression analysis also suggested a small reduction in the incidence of use of force reports for treatment officers relative to control officers controlling for pre-intervention use of force reports. However, this difference was also not statistically significant.

Table 4. Impact of BWC on Use of Force Reports

Standardized mean difference

	<u>N</u>	<u>Mean (SD)</u>	<u> d </u>	<u>t</u>	<u>p</u>
Treatment	140	.057 (.232)	.082	-1.38	0.168
Control	141	.106 (.352)			

Poisson regression

N = 281

Log pseudolikelihood = -79.783

	<u>Coef.</u>	<u>RSE</u>	<u>t</u>	<u>p</u>
Group (1=Treatment)	-.563	.461	-1.22	0.221
Pre-Intervention Complaints	.619	.377	1.65	0.100 ⁺
Intercept	-2.374	.335	-7.08	0.000**

Note: SD = Standard Deviation, Coef. = Coefficient, RSE = Robust Standard Error

+ = $p < .10$, * = $p < .05$, ** = $p < .01$

Discussion

Several recently-completed randomized controlled trials and quasi-experiments suggest that BWCs improve the civility of police-citizen civilian encounters by reducing complaints

against officers and officer use of force. In the Rialto (CA) randomized experiment, officers wearing BWCs during treatment shifts generated a 90% reduction in complaints and a 50% reduction in use of force reports relative to officers not wearing cameras during comparison shifts.¹⁴ The Mesa Police Department's quasi-experimental evaluation of BWCs revealed a 48% reduction in citizen complaints against treatment officers for misconduct during the study period, and a 75% decline in use of force complaints.¹⁵ In the Orlando (FL) randomized experiment, BWC officers had a significantly lower prevalence of response-to-resistance incidents (involving electronic control devices, chemical agents, impact weapons, and other non-lethal implements) and lower prevalence of serious external complaints relative to control officers without BWCs.¹⁶ A quasi-experimental evaluation in Phoenix (AZ) reported a 62% reduction in complaints lodged against treatment officers relative to control officers.¹⁷ Finally, a randomized controlled trial in Las Vegas (NV) found that the BWCs reduced citizen complaints and use of force reports for treatment officers relative to non-BWC comparison officers.¹⁸

While there is some promising evidence that BWCs de-escalate confrontation and aggression in police-citizen encounters, not all evaluations support this position. A randomized

¹⁴ Ariel, Barak, Tony Farrar, & Alex Sutherland. (2015). "The Effect of Police Body-Worn Cameras on Use of Force and Citizens' Complaints Against the Police: A Randomized Controlled Trial," 31 *Journal of Quantitative Criminology* 1–27.

¹⁵ Mesa Police Department (2013) *On-Officer Body Camera System: Program Evaluation and Recommendations*. Mesa, AZ: Mesa Police Department.

¹⁶ Jennings, Wesley G., Mathew Lynch, & Lorie Fridell (2015) "Evaluating the Impact of Police Officer Body-Worn Cameras (BWCs) on Response-to-Resistance and Serious External Complaints: Evidence from the Orlando Police Department (OPD) Experience Utilizing a Randomized Controlled Experiment," 43 *Journal of Criminal Justice* 480-486.

¹⁷ Hedberg, E.C., Charles Katz, & David Choate (2017) "Body-Worn Cameras and Citizen Interactions with Police Officers: Estimating Plausible Effects Given Varying Compliance Levels," *Justice Quarterly* (in press).

¹⁸ Braga, Anthony A., William Sousa, James Coldren, & Denise Rodriguez (2018) "The Effects of Body Worn Cameras on Police Activity and Police-Citizen Encounters: A Randomized Controlled Trial" *Journal of Criminal Law and Criminology* (in press).

experimental design was used to evaluate the effects of BWCs on complaints against officers in the London Metropolitan Police Service (UK). The study did not reveal any statistically-significant differences in overall complaints made against officers with BWCs relative to officers not wearing BWCs.¹⁹ There were also no statistically-significant differences in self-reported assaults on officers or injuries for BWC officers relative to control officers. A multisite randomized experiment involving 2,122 officers in eight police departments reported no overall reduction in officer use of force and an increase in assaults on officers wearing BWCs during treatment shifts relative to officers not wearing BWCs during control shifts.²⁰ In a re-analysis of the multisite randomized experiment data, the evaluators showed that use of force by treatment officers decreased by 37% in three sites with high compliance to a BWC policy that required officers to notify citizens that they were being recorded at the beginning of the encounter.²¹

The preliminary findings of this randomized controlled trial suggest that the placement of BWCs on BPD officers may generate small benefits to the civility of police-citizen civilian encounters in Boston. It is important to note here that the research design was not well positioned to detect an effect due to modest statistical power and low base rates for key outcomes. Put

¹⁹ Grossmith, Lynne, Catherine Owens, Will Finn, David Mann, Tom Davies, & Laura Baika (2015) *Police, Camera, Evidence: London's Cluster Randomised Controlled Trial of Body Worn Video*. London, UK: College of Policing and the Mayor's Office for Policing and Crime (MOPAC).

²⁰ Ariel, Barak, Alex Sutherland, Darren Henstock, Josh Young, Paul Drover, Jayne Sykes, Simon Magicks, & Ryan Henderson (2016) "Wearing Body-Cameras Increases Assaults Against Officers and Do Not Reduce Police-Use of Force: Results from a Global Multisite Experiment," 13 *European Journal of Criminology* 744-755.

²¹ The evaluators also reported a 71% increase in officer use of force in sites with low compliance to the BWC policy. Based on these findings, they hypothesized that unchecked BWC discretion may increase use of force as camera activation during situations with escalating aggression may further increase aggression during these volatile situations. The evaluators further suggested that verbal notification of video recording by officers at the commencement of encounters may be helpful in deterring aggressive behavior and stimulating civil behavior before police-citizen interactions escalate in a negative direction. Ariel, Barak, Alex Sutherland, Darren Henstock, Josh Young, Paul Drover, Jayne Sykes, Simon Magicks, & Ryan Henderson (2016) "Increases in Police Use of Force in the Presence of Body-Worn Cameras are Driven by Officer Discretion: A Protocol-Based Subgroup Analysis of Ten Randomized Experiments," 12 *Journal of Experimental Criminology* 453-463.

simply, the randomized experiment involved a small number officers and, in general, BPD officers do not generate many complaints and use of force reports. Relative to control officers, BWC treatment officers received fewer citizen complaints and generated fewer use of force reports. Statistical analysis revealed that the BWC impact on complaints was small but statistically-significant at a less restrictive threshold. However, the BWC impact on use of force was not statistically-significant, suggesting no meaningful difference between the treatment and control groups.

The evaluation team will continue to collect data and pursue supplemental analyses to ensure that these findings are robust to different tests and model specifications. As described in our proposal, a full technical report will be submitted to the City of Boston in June 2018. This final report will include a process evaluation of the BWC implementation and completed impact evaluation of BWC effects on the civility of police-citizen encounters, police proactivity, police lawfulness, and police-community relations. The forthcoming report will detail the methods, analytical results, and policy implications of formal impact and process evaluations.