Seventeenth Report of the Independent Monitor

The Deployment of Body Worn Cameras on NYPD Housing Bureau Officers Assigned to Police Service Areas

A Quasi-Experimental Evaluation Measuring Effects on the Civility of Police-Citizen Encounters, Policing Activities, and Police Lawfulness

Mylan Denerstein

October 17, 2022

Floyd, et al. v. City of New York
Ligon, et al. v. City of New York, et al.
Davis, et al. v. City of New York, et al.

MONITOR TEAM

Mylan Denerstein
Monitor

Richard Jerome
Deputy Monitor

Anthony A. Braga

Jennifer Eberhardt

Demosthenes Long

John MacDonald

James McCabe

Jane Perlov

James Yates

Table of Contents

MONITOR'S SEVENTEENTH REPORT

I. Executive Summary

This Seventeenth Report presents the results of the Monitor's evaluation of the deployment of body-worn cameras (BWCs) by New York City Police Department (NYPD) Housing Bureau officers in and around New York City Housing Authority (NYCHA) developments. Specifically, the evaluation focused on officers working in nine Police Service Areas (PSAs) between February 2018 and December 2018.[1] Because the settlement in *Davis v. City of New York,* 10 Civ. 0699 (SAS) (4/28/15) ECF Dkt. 339, specifically identified problematic trespass enforcement practices in NYCHA developments, it was important for the Monitor Team to examine the impact of BWC deployment on Housing Bureau officers and their interactions with civilians. The evaluation assessed whether the deployment of BWCs on Housing Bureau officers affected the civility of police-citizen encounters, the level of police enforcement activity, and the lawfulness of police encounters, comparing those outcome measures both before officers were equipped with BWCs and for a year after BWCs were deployed.[2]

It is important to note that the BWC implementation occurred when there were large declines in NYPD enforcement activity, particularly trespass arrests, stops, and summonses, in and around NYCHA housing buildings. The evaluation controlled for those trends and other factors. The evaluation used regression analysis to estimate the impact of BWC deployment by analyzing data at both the command (PSA) level and data at the individual officer level (for this reason, the results of the evaluation are reported at the command level and at the officer level). Many, but not

[1] PSAs, or Police Service Areas, are similar to precincts; they are geographic areas in which NYPD's Housing Bureau officers operate and provide police services to the residents in New York City's public housing projects. New York City is divided into nine PSAs.

[2] Professors Anthony Braga, John MacDonald, James McCabe, and other members of the Monitor Team developed and executed the research design and evaluation of the PSA BWC study.

all, of the findings of this evaluation are consistent with the findings of the Monitor's earlier evaluation of the impact of BWCs on NYPD officers working in the Patrol Services Bureau (PSB)—patrol officers working in police precincts.[3]

After BWCs were deployed on Housing Bureau officers in the PSA, the main findings of the Housing Bureau BWC evaluation include the following:

1. There were statistically significant improvements in the civility of police-citizen encounters after Housing Bureau officers were equipped with BWCs. Using regression models to estimate the impact of BWC deployment and controlling for other variables, the evaluation found a 43 percent reduction in Civilian Complaint Review Board (CCRB) complaints in the PSA command-level analysis and a 42 percent reduction in CCRB complaints in the officer-level analysis.

2. Similarly, the Housing Bureau BWC evaluation estimated a 16 percent reduction in arrests that reported the use of force in the PSA command-level analysis and a 20 percent reduction in arrests that reported the use of force in the officer-level analysis after deployment of BWCs.

3. The Housing Bureau BWC evaluation estimated that after officers were equipped with BWCs, there was a 23 percent reduction in total arrests, a 23 percent reduction in trespass arrests, and a 60 percent reduction in summonses for disorderly conduct in the PSA command-level analysis. Similarly, the Housing Bureau BWC evaluation estimated a 34 percent reduction in total arrests, a 31 percent reduction in trespass arrests, and a 13 percent reduction in summonses for disorderly conduct in the officer-level analysis.

4. The Housing Bureau BWC evaluation found that interior patrols by Housing Bureau officers were reduced by 30 percent after they were equipped with BWCs.

5. The evaluation also found that the deployment of BWCs on Housing Bureau officers was associated with large increases in the submission of stop reports. Controlling for other variables, the regression models estimated a 48 percent increase in stop reports in the PSA command-level analysis and a 68 percent increase in stop reports in the officer-level analysis after Housing Bureau officers were equipped with BWCs.

6. The regression analysis comparing random samples of stop reports before and after Housing Bureau officers were equipped with BWCs found that after officers were equipped with BWCs, stop reports were more likely to involve Black subjects, less likely to originate from officer-initiated calls for service, and more likely to involve violent crimes or other crimes, rather than drug and disorder offenses (e.g., disorderly conduct, obstruction of government administration (OGA)).

[3] Twelfth Report of the Independent Monitor. https://www.nypdmonitor.org/wp-content/uploads/2022/09/12th-Report.pdf.

7. Further, stops made after Housing Bureau officers were equipped with BWCs were less likely to involve the search of the civilian stopped, less likely to involve an arrest or summons issued as a result of the stop, and less likely to involve searches ultimately judged as lawful when audited by the Monitor Team. This last result appears to be driven by increased reporting of stops by Housing Bureau officers wearing BWCs, thus capturing more stops with questionable constitutionality.

The Housing Bureau BWC evaluation suggests that the fact that officers had a BWC resulted in more civil encounters between NYPD Housing Bureau officers and civilians, and further reduced arrest and summons activity in the PSAs. While policing enforcement appeared to decrease after deployment of BWCs, the evaluation found that the placement of BWCs on Housing Bureau officers resulted in a significant increase in the number of stop reports submitted.

BWCs provide video documentation of officers' and civilians' actions. Because a failure to file a stop report by an officer when required to do so is more likely to be discovered if an officer is properly using a BWC, the increase in stop reports may be the result of increased reporting as opposed to an increase in stops. NYPD policy requires officers to activate BWCs during all pedestrian stops, and officers must document these encounters by filing stop reports. Based on the NYPD's procedures and the results of the Housing Bureau BWC evaluation, we infer that the availability of BWC video for specific encounters increases the likelihood that commanding officers will detect unreported stops. Failing to submit a stop report (or failing to record the encounter on BWC) can subject an officer to a disciplinary violation.[4]

The increased share of stop reports without additional enforcement actions (searches, summonses, or arrests) suggests that Housing Bureau officers outfitted with BWCs increased their documentation of less intrusive encounters that would not have resulted in stop reports in the

[4] Whether officers are actually being disciplined for failure to document stops is a different question and one that is the subject of Monitor Team review.

absence of the use of BWCs. When there is an increase in the documentation of encounters, it is, of course, more likely that problematic encounters will be documented. The Housing Bureau BWC evaluation indicates that this is what happened. As such, BWCs used properly can be useful tools for reducing the underreporting of stops and the number of unlawful stops by making stops more transparent to NYPD supervisors and outside monitors (e.g., district attorneys' offices, courts, and the CCRB). The deployment of BWCs on Housing Bureau officers not only increases their compliance with NYPD directives to document stops, but it also provides the Department with an important opportunity to intervene and monitor their progress toward ensuring constitutional policing.

II. Background

The Remedial Order in *Floyd v. City of New York* noted the potential benefits of outfitting NYPD officers with BWCs. These potential benefits included the creation of objective records of stop-and-frisk encounters, encouraging lawful and respectful police-citizen interaction if both parties know the encounter is being recorded, alleviating mistrust between the NYPD and the public, and offering a way to substantiate whether NYPD officers engage in alleged misconduct. 959 F. Supp. 2d 668, 685 (S.D.N.Y. 2013) (Remedial Order). The Remedial Order mandated that the NYPD work with the Monitor to conduct a one-year pilot BWC program to determine whether the benefits of the cameras outweigh their financial, administrative, and other costs, and whether the program should be expanded or terminated. The Monitor Team developed a randomized controlled trial to evaluate the impacts of equipping patrol officers with BWCs and concluded that the benefits of BWCs justified their continued use by the NYPD. *See* Monitor's Twelfth Report.[5]

Officers in NYPD's Housing Bureau patrolling NYCHA public housing developments in Police Service Areas (PSAs) were not included in the Monitor's randomized controlled trial. Housing Bureau officers were not equipped with BWCs until after the completion of the one-year pilot. Because the 2015 settlement in *Davis v. City of New York* specifically identified problematic trespass enforcement practices in NYCHA developments, it was important for the Monitor Team to also examine the impact of BWCs on Housing Bureau officers and their interactions with civilians. In addition, Housing Bureau officers have unique responsibilities and authority in public

[5] https://www.nypdmonitor.org/wp-content/uploads/2022/09/12th-Report.pdf.

housing. For these reasons, a stand-alone evaluation of the deployment of BWCs on NYPD Housing Bureau officers working in NYCHA developments was needed.[6]

This Seventeenth Report presents the methods and results of the evaluation of the deployment of BWCs on NYPD Housing Bureau commands and officers. This report describes in detail the research designs and statistical models used to determine whether outfitting Housing Bureau officers with BWCs generated any discernible impacts on the civility of police-civilian encounters, on policing activities, and on the lawfulness of police actions. Finally, the report presents the findings and discusses the policy implications of the research.

III. The Deployment of BWCs on NYPD Patrol Services Bureau and Housing Bureau Officers

The NYPD Housing Bureau provides policing services to the vast majority of residents in NYCHA housing. Specifically the Housing Bureau services residents in 258 NYCHA housing developments—75.2 percent of the 343 total NYCHA housing developments—through nine NYPD PSAs in four boroughs: Brooklyn, the Bronx, Manhattan, and Queens.[7] NYPD Housing Bureau officers in the nine PSAs were equipped with BWCs on a rolling basis (by PSA) over the course of a nearly 11-month period (Table 1, below). The PSA BWC implementation began the week of February 12, 2018 (PSA 8) and was completed the week of December 16, 2018 (PSA 9).

[6] There is a growing body of research studies evaluating police use of BWCs. While these studies have consistently found that BWCs have had a positive effect on certain outcomes, such as a reduction in civilian complaints as noted later in this report, for other outcomes the evidence of the impact of BWCs is less clear. With respect to some officer and civilian behavior outcomes, the evidence from other BWC studies have generated conflicting results. These divergent findings may be linked to differing encounter contexts and varying BWC policies and practices across study research sites. *See, e.g.*, Cynthia Lum, Christopher Koper, David B. Wilson, Megan Stoltz, Michael Goodier, Elizabeth Eggins, Angela Higginson, and Lorraine Mazerolle. 2020. "Body-Worn Cameras' Effects on Police Officers and Citizen Behavior: A Systematic Review." *Campbell Systematic Reviews*, 16(3): e1112, *available at* https://onlinelibrary.wiley.com/doi/full/10.1002/cl2.1112 (accessed March 27, 2022).

[7] The Patrol Services Bureau provides policing services to residents in the remaining 85 NYCHA housing developments (24.7 percent of the 343 housing developments) in 25 precincts in all five boroughs, including Staten Island.

Table 1. Deployment of BWCs on NYPD Housing Bureau Officers in PSAs

BWC Implementation Date	PSA	Borough
Week of 2/12/2018	8	Bronx
Week of 3/5/2018	3	Brooklyn
Week of 4/9/2018	4	Manhattan
Week of 6/18/2018	1	Brooklyn
Week of 7/1/2018	7	Bronx
Week of 8/19/2018	6	Manhattan
Week of 10/21/2018	2	Brooklyn
Week of 12/2/2018	5	Manhattan
Week of 12/16/2018	9	Queens

IV. Research Design and Outcomes Measured

A. Quasi-Experimental Design, Step Wedge Evaluation

Given the small number of PSAs and the complexities associated with citywide implementation of BWCs, it was not practical to design a randomized control trial of the placement of BWCs on NYPD Housing Bureau officers. Instead, the research method called "quasi-experimental design" was used. Quasi-experimental designs seek to approximate the characteristics of a randomized controlled experiment, without the benefit of random allocation of units to treatment and control conditions.[8] Quasi-experiments do not have the same degree of internal validity as randomized controlled trials; however, well-designed quasi-experiments can produce results that are of similar quality to randomized controlled trials.[9] As such, there is a high

[8] Donald Campbell & Julian Stanley. 1966. *Experimental and Quasi-Experimental Designs for Research.* Chicago: Rand McNally.

[9] Lipsey, M., & Wilson, D. (1993). *Practical Meta-Analysis*. Thousand Oaks, CA: Sage Publications; Berk, R., Barnes, G., Alhman, L., & Kurtz, E. (2010). "When second best is good enough: A comparison between a true experiment and a regression discontinuity quasi-experiment." *Journal of Experimental Criminology*, 6(2): 191–208. Internal validity is a term used by social scientists to assess the quality of research designs and refers to "inferences about whether observed co-variation between A and B reflects a causal relationship from A to B in the form in which variables were manipulated or measured. To support such an inference, the researcher must show that A preceded B in time, that A covaries with B . . . and that no other explanations for the relationship are plausible." *See*

degree of confidence in the results of the Housing Bureau BWC evaluation that establish a relationship between the BWC deployment and the observed outcomes that cannot be explained by other factors.

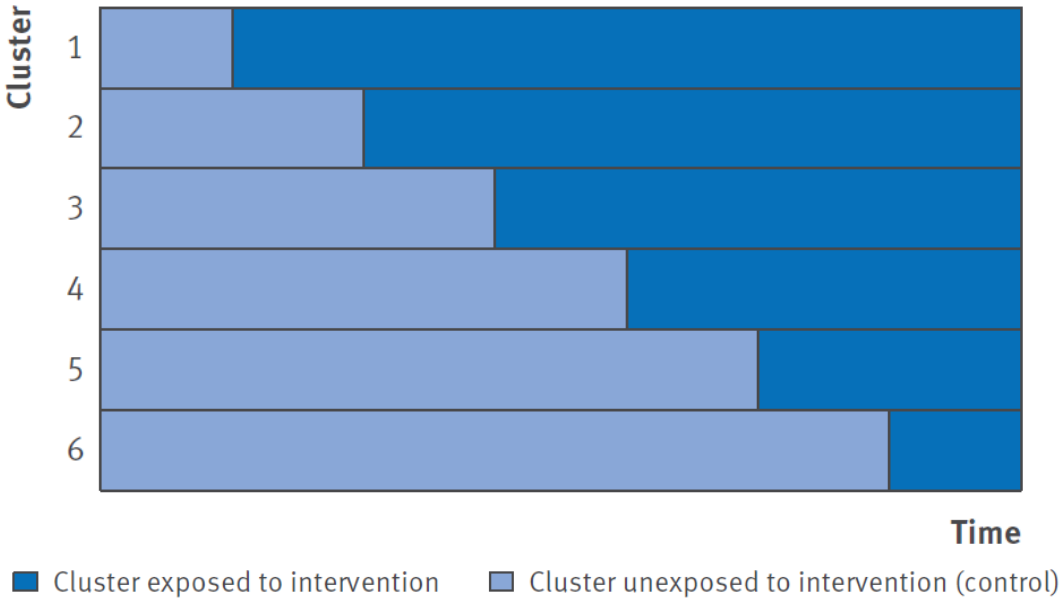
The fact that BWCs were provided to Housing Bureau officers on a rolling basis, one PSA at a time, allowed the Monitor team to use what is known as a "cluster stepped-wedge evaluation design."[10] In the Housing Bureau BWC evaluation, the design included a pre-intervention period in which no PSA commands ("clusters" of officers) were equipped with BWCs (and thus were in the "control" group, and not in the "treatment" group). Subsequently, at regular intervals (the "steps"), one PSA command was equipped with BWCs, and thus crossed from the no-treatment control group to the BWC treatment group under evaluation. This process continued until all Housing Bureau officers in the nine PSAs were equipped with BWCs and thus crossed over to be the treatment group. Figure 1 below illustrates the basic structure of a stepped-wedge design, where all clusters eventually move from the no-treatment control group to the treatment group, in the same way that Housing Bureau officers in each PSA moved from the control group without BWCs to the treatement group with BWCs.[11]

William Shadish, Thomas Cook, and Donald Campbell 53 (2002). *Experimental and Quasi-Experimental Designs for General Causal Inference.* Belmont, CA: Wadsworth.

[10] Hu, Y., & Hoover, D.R. (2018). "Non-Randomized and Randomized Stepped-Wedge Designs Using an Orthogonal Least Squares Framework." *Statistical Methods in Medical Research*, 27 (4): 1202–18.

[11] Figure 1 was adapted from Hemming, K. et al. (2015). "The Stepped Wedge Cluster Randomized Trial: Rationale, Design, Analysis, and Reporting." *British Medical Journal* (Clinical research ed.), 350: h391.

Figure 1. Stepped-Wedge Evaluation Design



At the end of the study, all PSA commands were equiped with BWCs, or in the terms of social science studies, exposed to the BWC intervention. For the Housing Bureau BWC evaluation, data was collected throughout the period of BWC deployments, so that each PSA has data under both control (no use of BWCs) and treatment (use of BWCs) periods.[12]

In evaluating the impact of equipping Housing Bureau officers with BWCs on selected outcome measures, the Housing Bureau BWC evaluation used two complementary analyses. The first analysis used data collected at the PSA level (i.e., data aggregated at each individual PSA

[12] The Maryland Scientific Methods Scale is often used by criminologists to assess the internal validity of evaluation designs across five-levels. "Level 3" is regarded as the minimum design that is adequate for drawing conclusions about program effectiveness. These designs rule out many threats to internal validity such as history, maturation/trends, instrumentation, testing, and mortality. The main problems of Level 3 evaluations center on selection effects and regression to the mean as a result of the nonequivalence of treatment and control conditions. The PSA BWC evaluation would be considered a "Level 4" evaluation as it measured outcomes before and after the program in multiple treatment and control condition units. These types of designs have better statistical control of extraneous influences on the outcome and, relative to lower-level evaluations, deal with selection and regression threats more adequately. As such, conclusions can be drawn from this evaluation on the impacts of deploying BWCs on Housing Bureau officers on selected outcome measures. *See* Lawrence W. Sherman, Denise Gottfredson, Doris L. MacKenzie, John Eck, Peter Reuter, and Shawn Bushway. (1997). *Preventing Crime: What Works, What Doesn't, What's Promising.* Report to the U.S. Congress. Washington, DC: U.S. Department of Justice, Office of Justice Programs, National Institute of Justice.

command) and used regression models to estimate the effects associated with the implementation of BWCs in the PSAs. The second analysis used data collected at the individual officer level and used regression models to estimate the impact of BWC deployment at the officer level (within PSAs).

B. Outcome Measures Evaluated

The Housing Bureau BWC evaluation measured the impact of BWC deployment using three sets of outcome measures: (1) civility of police-citizen interactions; (2) policing activity; and (3) police lawfulness. The evaluation examined the change in these outcome measures before and after the deployment of BWCs in each PSA. The data for the study's outcome measures were collected from data systems of the NYPD and the CCRB.

Civility of Police-Citizen Interactions. The available research suggests that having officers equipped with BWCs may improve the civility of police-citizen interactions by deterring undesirable behaviors—neither officers nor civilians want to be recorded on video doing something inappropriate or illegal—and prompting desirable, respectful behavior.[13] For this evaluation, pre-intervention (without BWC) and intervention (with BWC) data were collected and analyzed for two "civility/de-escalation" outcomes: (1) Housing Bureau officer arrest reports listing use of force and (2) CCRB complaints against Housing Bureau officers between January 1, 2015 and December 31, 2019.

Policing Activity. A comparison of Housing Bureau officer activities in the pre-intervention and intervention periods helps examine whether the use of cameras affect officer work

[13] *See, e.g.*, Barak Ariel, William Farrar, and Alex Sutherland. 2015. "The Effect of Police Body-Worn Cameras on Use of Force and Citizens' Complaints against the Police: A Randomized Controlled Trial." *Journal of Quantitative Criminology*, 31: 509–35; Cynthia Lum, Megan Stoltz, Christopher Koper, and Amber Scherer. 2019. "Research on Body-Worn Cameras: What We Know, What We Need to Know." *Criminology & Public Policy*, 18: 93–118.

behaviors.[14] Metrics included pre-intervention and intervention counts of arrests (total arrests, trespass arrests, and disorder/obstruction arrests), summonses issued (total and disorder summonses), interior patrols, officer-initiated calls, and stop reports made by Housing Bureau officers between January 1, 2015, and December 31, 2019.

Police Lawfulness. Body worn cameras are a potentially effective tool for enhancing the constitutionality of officer actions while performing their law enforcement duties.[15] While the potential benefits were a significant part of the push for equipping officers with body-worn cameras, a recent systematic review of BWC research studies concluded that little is known about the impacts of the technology on police lawfulness beyond speculation.[16] The availability of NYPD stop reports allowed for an analysis considering whether cameras impact the constitutionality of police encounters with citizens. To evaluate police lawfulness, the evaluation compared stop reports from Housing Bureau Officers in PSA's before they were equipped with BWCs, with stop reports from Housing Bureau officers in PSA's after they were equipped with BWCs. Each sample of stop reports were assessed by the Monitor Team for the lawfulness of the

[14] Some observers suggest that wearing cameras might cause officers to be less active or more reluctant to initiate citizen contacts, instead focusing most of their time on dispatched calls. Police Executive Research Forum. 2014. *Implementing a Body-Worn Camera Program: Recommendations and Lessons Learned*. Washington, DC: Police Executive Research Forum. However, a few research studies have suggested that officers wearing cameras are *more likely* to initiate encounters and take enforcement actions than their counterparts without cameras. *See, e.g.*, Justin Ready & Jacob Young. 2015. "The Impact of On-Officer Video Cameras on Police–Citizen Contacts: Findings from a Controlled Experiment in Mesa, AZ." *Journal of Experimental Criminology*, 11: 445–58; Braga, Anthony A., William H. Sousa, James R. Coldren, and 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*, 108: 511–38.

[15] Jay Stanley. 2015. *Police Body-Mounted Cameras: With Right Policies in Place, A Win for All*. New York: American Civil Liberties Union.

[16] Cynthia Lum, Megan Stoltz, Christopher Koper, and Amber Scherer. 2019. "Research on Body-Worn Cameras: What We Know, What We Need to Know." *Criminology & Public Policy*, 18: 93–118.

stop, frisk, and search described in the narratives in the stop reports. A description of the sampling methodology and the Monitor Team's assessment process is included in Appendix A.

C. Description of NYPD PSAs in 2017, Prior to Deployment of BWCs

There are a number of different characteristics among the NYPD PSAs, such as the size of housing developments, surrounding neighborhood poverty, officer staffing, and crime rates, that needed to be controlled for the analyses in the Housing Bureau BWC evaluation. This section of the Report describes those characteristics, based on data as of 2017, prior to the deployment of BWCs in the PSAs.

Table 2 below presents the total number of developments and buildings, the land area (in square miles) covered by these buildings, and the total number of apartments and residents living in NYCHA housing served by each of the nine PSAs in 2017, the year prior to BWC deployment. In total, NYPD Housing Bureau officers provided policing services to some 333,977 residents in 149,684 apartments across 467 buildings, comprising 258 housing developments, covering just over three square miles of land area. PSA 7, located in the Bronx, served the largest number of people with more than 49,000 residents in 49 NYCHA developments. By contrast, PSA 9 in Queens served the smallest number of people with roughly 25,000 residents in 12 NYCHA developments. In 2017, the overall NYCHA resident population was 54.9 percent Hispanic, 36.6 percent Black, 3.1 percent Asian, 43.1 percent White, and 2.3 percent other races.[17]

[17] https://furmancenter.org/files/NYCHA_Diversity_Brief_Final-04-30-2019.pdf (accessed 6/24/2022).

Table 2. Developments, Buildings, Land Areas, Total Apartments, and Total Residents by PSA in 2017[18]

	Total Developments	Total Buildings	Land Area (sq. mi.)	Total Apartments	Total Residents
PSA 1	20	35	.483	17,219	36,764
PSA 2	43	92	.458	19,537	43,906
PSA 3	28	56	.379	19,420	45,186
PSA 4	32	46	.227	16,399	35,243
PSA 5	29	49	.257	17,176	37,834
PSA 6	26	52	.183	13,953	29,185
PSA 7	49	75	.307	20,128	49,248
PSA 8	19	35	.445	14,133	31,278
PSA 9	12	27	.334	11,719	25,333
PSA Total	258	467	3.073	149,684	333,977

Table 3 presents summary data for the nine PSAs reflecting the average (mean) number of NYPD officers assigned, 911 emergency calls for service dispatched, major crime complaints (murder, rape, robbery, felony assault, burglary, grand larceny, and grand larceny auto), arrests (including arrests for felonies, misdemeanors, violations, and infractions), arrests where force was used by the officer(s) making the arrest(s), and number of stop reports between 2015 and 2017. These data are presented as means per PSA and means per officer in the PSAs. As Table 3 reveals, there are some notable variations in these six measures across the nine PSAs. For example, the average number of major crimes in PSA2 was 696.0, while the average number of major crimes in PSA 9 was only 284.3. PSA 7 had an average of 15.1 arrests per officer while PSA 9 had only an average of 5.8 arrests per officer.

[18] Following the structure of the data file provided to the Monitor Team by the NYPD on December 27, 2017, the development counts were based on unique Tenant Data System (TDS) numbers that NYCHA uses to assign tenant applications to specific housing developments. As such, these counts included housing developments with related names as distinct developments. For instance, in PSA 6, Douglass I (TDS 082), Douglass II (TDS 582), and Douglass Addition (TDS 148) represented three distinct housing developments in Table 1 rather than being aggregated into one larger housing development that shared the "Douglass" name attribution. We supplemented the NYPD data with the "NYCHA Data Development Book File," publicly available at https://data.cityofnewyork.us/Housing-Development/NYCHA-Development-Data-Book/evjd-dqpz (downloaded 12/28/2017).

Table 3. NYPD Officer Staffing and Work Activities in Nine PSAs, 2015 – 2017[19]

Mean Per PSA

	Avg. Officers	Avg. 911 Calls	Avg. Major Crimes	Avg. Arrests	Avg. Arrests w/ Force	Average Stop Reports
PSA 1	246.3	29,280.3	512.0	1,790.7	38.3	241.7
PSA 2	328.0	17,979.3	696.0	2,350.7	65.0	121.7
PSA 3	262.0	22,117.0	638.7	2,089.3	53.7	124.3
PSA 4	166.3	12,284.0	388.0	1,759.7	22.0	204.7
PSA 5	245.7	21,489.3	567.7	3,125.0	51.3	204.3
PSA 6	158.3	14,743.0	432.3	1,497.0	15.3	153.0
PSA 7	241.3	22,109.7	677.3	3,651.7	43.7	313.7
PSA 8	193.0	16,803.7	605.0	2,046.0	27.3	178.7
PSA 9	217.3	21,037.7	284.3	1,264.0	16.3	65.7
PSA total	2,058.3	177,844.0	4,801.3	19,574.0	333.0	1607.7

Mean Per Officer in PSA

	Avg. Officers	Avg. 911 Calls Per Officer	Avg. Major Crimes Per Officer	Avg. Arrests Per Officer	Avg. Arrests w/ Force Per Officer	Average Stop Reports Per Officer
PSA 1	246.3	118.9	2.1	7.3	0.2	1.0
PSA 2	328.0	54.8	2.1	7.2	0.2	0.4
PSA 3	262.0	84.4	2.4	6.4	0.2	0.5
PSA 4	166.3	73.9	2.3	10.6	0.1	1.2
PSA 5	245.7	87.5	2.3	12.7	0.2	0.8
PSA 6	158.3	93.1	2.7	9.5	0.1	1.0
PSA 7	241.3	91.6	2.8	15.1	0.2	1.3
PSA 8	193.0	87.1	3.1	10.6	0.1	0.9
PSA 9	217.3	96.8	1.3	5.8	0.1	0.3
PSA total	2,058.3	86.4	2.3	9.5	0.2	0.8

Table 4 presents descriptive characteristics and CCRB complaint histories for the Housing Bureau members assigned to patrol in the PSA commands as of December 31, 2017 (N=1,916). The vast majority of Housing Bureau members assigned to patrol held the rank of police officer (88.7%), while a much smaller share held the rank of sergeant (11.3%). Housing Bureau patrol

[19] The NYPD provided the aggregate PSA data and individual PSA patrol officer data to the Monitor Team on March 27, 2018, and April 24, 2018, respectively.

officers in the PSA commands were primarily male (81.9%), had an average age of 33.6 years, and averaged 6.8 years on the job. Housing Bureau patrol officers were racially diverse: 36.2 percent were Hispanic, 34.3 percent were white, 19.2 percent were Black, and 10.3 percent were Asian/Pacific Islander. Housing Bureau patrol officers rarely generated CCRB complaints. Fifty-one percent of Housing Bureau patrol officers did not generate a single CCRB complaint over the course of their career as of December 31, 2017. Housing Bureau patrol officers generated, on average, 0.175 CCRB complaints per year on the job (or less than one complaint every five years).

Table 4. Descriptive Characteristics and CCRB Complaint History of Housing Bureau Officers Assigned to Patrol in PSA Commands (December 31, 2017 snapshot)

Officers Assigned		
	Number	**Percent**
PSA 1	236	12.3
PSA 2	311	16.2
PSA 3	243	12.7
PSA 4	150	7.8
PSA 5	215	11.2
PSA 6	147	7.7
PSA 7	218	11.4
PSA 8	178	9.3
PSA 9	218	11.4
Total	1,916	100
Rank		
Police Officer	1,700	88.7
Sergeant	216	11.3
Gender		
Male	1,569	81.9
Female	347	18.1
Race		
Hispanic	693	36.2
White	658	34.3
Black	367	19.2
Asian / Pacific Islander	198	10.3
Age		
Mean	33.6	
Standard deviation	6.8	

Range	21 – 59	
Years on the Job		
Mean	6.8	
Standard deviation	5.5	
Range	<1 – 31	
CCRB Complaint Rate per Year on the Job		
Mean	0.175	
Standard deviation	0.273	
Range	0 – 3.2	
CCRB Complaints during Career		
	Number	**Percent**
No complaints	978	51.0
One or more complaints	938	49.0

V. Estimating the Impact of BWCs on the Civility of Police-Civilian Encounters and Policing Activity

This section describes the research methodology and statistical models used to estimate the impact of equipping Housing Bureau officers with BWCs on the civility of police encounters with civilians, and on the level of enforcement activities of those officers. The methodology and models are described first at the PSA command level, and then at the officer level, as are the results of the analyses. This section includes technical language describing the Housing Bureau BWC evaluation.[20]

A. Statistical Models for the PSA Command-Level Analysis

Time series analysis, involving before and after measurements for a particular dependent variable, represents a common type of evaluation research found in the fields of criminology and criminal justice.[21] The units of analysis in this evaluation are "PSA-months" over a five year

[20] It is important to include the technical details of the research so that outside experts can closely review the research methods and statistical models used in the evaluation.

[21] One intended purpose for doing this time-series quasi-experimental research is to capture longer time periods and a sufficient number of different events to control for various threats to validity and reliability. Shadish, W., Cook,

period. Monthly counts of outcome measures in each PSA were collected between January 1, 2015 and December 31, 2019 (N = 9 PSAs * 12 months * 5 years = 540 PSA-months). Count data represent the number of times that an event occurs within a specific time frame, e.g., the number of stops conducted by NYPD Housing Bureau officers in PSA 1 during a one-month time period.[22]

Regression models were used to analyze the monthly change in outcome counts for each PSA command before and after officers in the PSA command were equipped with BWCs, controlling for other covariates. A simplified version of the panel regression model was estimated as follows:

$$(1) \quad Y_{it} = \beta_0 + \beta_1 BWC_{it} + \beta_2 Trend_{it} + \beta X_{it} + \alpha_i + \delta_t + \varepsilon_{it}$$

Where $i = 1 \ldots 9$ PSA commands, with PSA i consisting of $t = 1, \ldots, n_i$ monthly observations, and Y_{it} is the outcome variable indicating the monthly count of an outcome in a specific PSA i during month-year t. The regressor BWC is a dummy variable identifying whether a PSA command adopted BWCs (1) or not (0) during the study period. The coefficient β_1 is the estimate of the

T., & Campbell, D. (1979). *Experimental and quasi-experimental designs for generalized causal inference*. Boston: Houghton Mifflin Company. The established trend found before the treatment to be studied allows researchers to predict what might have happened without the intervention. The difference between what actually happened after the intervention and the predicted outcome based on the trend before treatment helps to determine the actual impact of the treatment. However, in this study, the estimation of BWC impacts in PSAs will be limited by the absence of a no-treatment control group to account for confounding factors not specified in the models.

[22] There are well-documented problems associated with treating event count variables, which are discrete, as continuous realizations of a normal data-generating process. Gary King. 1989. "Event Count Models for International Relations: Generalizations and Applications." *International Studies Quarterly*, 33: 123–47. As such, methods such as standard mean difference tests and ordinary least squares regression that assume conditional normality of the dependent variable may generate biased estimates with count data. William Gardner, Edward Mulvey, and Esther Shaw. 1995. "Regression Analyses of Counts and Rates: Poisson, Overdispersed Poisson, and Negative Binomial Models." *Psychological Bulletin*, 118: 392–404. Thus, Poisson regression is generally used to estimate models of the event counts. J. Scott Long. 1997. *Regression Models for Categorical and Limited Dependent Variables*. Advanced Quantitative Techniques in the Social Sciences, Volume 7. Thousand Oaks, CA: Sage Publications. The Poisson regression model has the defining characteristic that the conditional mean of the outcome is equal to the conditional variance. However, in practice, the conditional variance often exceeds the conditional mean. We used robust standard errors to adjust for overdispersion in our Poisson regression models. 22 Richard Berk and John MacDonald. 2008. "Overdispersion and Poisson Regression." *Journal of Quantitative Criminology*, 24: 269–84. As a robustness check, all models were also estimated with negative binomial regressions. The results presented do not change.

direct effect of BWC treatment on the selected outcome measures. *Trend* represents a monthly (linear) covariate used to control for the steep decline in specific outcomes, such as arrests, stops, and summonses in the PSAs during this time period.[23] The model also controlled for factors that change each month within PSAs that may be correlated with the outcome measures, such as the number of officers assigned to the PSAs monthly, citizen calls for service received in the PSAs, and major crime incidents reported in the PSAs, and β represents the vector of estimates of these attributes (X_{it}). Fixed effects for PSA (α_i) and month (δ_t) were included to control for common trends to all PSAs in a given month and unmeasured differences between PSAs that were stable over the five-year time period.

Robust standard errors were clustered by PSA to assure that estimates account for heteroscedasticity and unmeasured dependence at the PSA level over time. The parameter estimates were expressed as incidence rate ratios (IRR) (i.e., exponentiated coefficients), or the ratio of change in the count.[24] Following convention, a two-tailed 5% level of significance was selected as the benchmark to reject the null hypothesis of "no difference."

In alternative specifications of equation (1), we include the term $\gamma(BWC_i * Month_t)$, which represents PSA-specific monthly time trends. The inclusion of PSA-specific monthly time trends enables an assessment of whether the findings hold after taking into account that each PSA is implementing cameras at different time points and may have diverging trends.[25]

---

[23] *See* https://www1.nyc.gov/assets/nypd/downloads/pdf/monitor-reports/federal-monitor-15th-report.pdf.

[24] An IRR is easy to convert into a percentage change in expected count. For instance, an IRR = 1.10 would represent a ten percent increase in the outcome counts and an IRR = 0.90 would represent a ten percent decrease in the outcome counts for the PSA BWC intervention months relative the outcome counts for the PSA BWC pre-intervention months, controlling for the other covariates included in the model.

[25] Angrist, J.D., Pischke, J.S., 2009. *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton, NJ: Princeton University Press.

To assess how outcomes evolved in the months leading up to and after PSAs adopted BWCs, we estimated an event study model as an expansion of the panel regression model as follows:

$$(2)\ Y_{it} = \sum_{\substack{j=-13 \\ j\neq -1}}^{12} \theta_j 1(t=j) + \beta_2 Trend_t + \beta X_{it} + \alpha_i + \varepsilon_{it}$$

The first set of parameters θ denotes indicators for each month relative to the implementation of body worn cameras in each PSA. We exclude the month before the adoption of cameras to serve as the reference period. The event study model controls for PSA fixed effects and other covariates reflected in the vector of X attributes.[26] Again, robust standard errors were clustered by PSA to assure that estimates were adjusted to heteroscedasticity and unmeasured dependence at the PSA level over time.

B. Results of PSA Command-Level Analysis

Table 5 below presents the results of Poisson panel regressions of the command-level effects of deploying BWCs on NYPD Housing Bureau officers on the following outcome measures over the course of the study period: CCRB complaints; stops; arrests; arrests with force; summonses; interior patrols; and officer-initiated calls to dispatch.

The results suggest that the BWC intervention enhanced the civility of encounters between NYPD Housing Bureau officers and civilians. Controlling for the other variables,[27] the BWC

[26] Stevenson, B., Wolfers, J., 2006. "Bargaining in the shadow of the law: divorce laws and family distress." *Quarterly Journal of Economics*, 121: 267–88; Steinberg, M.P., B. Ukert, and J.M. MacDonald. 2019. "Schools as places of crime? Evidence from closing chronically underperforming schools." *Regional Science and Urban Economics*, 77: 125–40.

[27] It is worth noting here that the fixed effects PSA dummy variables included the Poisson regression models in Table 5 show some statistically significant differences in various outcomes across PSAs over time. For instance, the coefficient for PSA 7 (IRR = 1.681) in the first model indicates that this PSA had 68.1 percent more CCRB complaints during the study period relative to PSA 1 (the reference category).

intervention was associated with a statistically significant 42.9 percent decrease ($p<.05$) in the monthly counts of CCRB complaints against officers. The BWC intervention was also associated with a statistically significant 15.6 percent decrease ($p<.05$) in the monthly number of arrests in which officers reported using force during the intervention period relative to the pre-intervention period.

The results further suggest that the Housing Bureau officers increased the number of stop reports they filed and generally decreased their enforcement activities after being equipped with BWCs. The BWC intervention was associated with a statistically significant 47.5 percent increase ($p<.01$) in the monthly numbers of stop reports submitted by Housing Bureau officers during the intervention period relative to the pre-intervention period, holding other variables constant. Controlling for pre-existing decreases in enforcement actions in NYCHA developments, (particularly trespass enforcement, noted in the Monitor's Fifteenth Report[28]) and other covariates, the analyses found that the BWC intervention was associated with statistically significant decreases in monthly counts of total arrests (-34.3%, $p<.01$), trespass arrests (-30.9%, $p<.05$), interior patrols of NYCHA buildings (-30.4%, $p<.01$), and issuance of summonses for disorderly behavior (-60.7%, $p<.01$) during the intervention period relative to the pre-intervention period. However, the BWC intervention did not yield statistically significant changes in the monthly numbers of total summonses issued, officer-initiated calls for service, and disorder/obstruction arrests over the course of the study time-period.

[28] https://www.nypdmonitor.org/wp-content/uploads/2022/09/15-Fifteenth-Report.pdf.

Table 5. Poisson Regressions Estimating the Impact of the BWC Intervention on Monthly Counts of Selected Outcomes in NYPD PSAs

	CCRB IRR (RSE)	Stops IRR (RSE)	Arrests IRR (RSE)	Arrests w/Force IRR (RSE)	Summonses IRR (RSE)	Interior Patrols IRR (RSE)	Officer-initiated Calls IRR (RSE)
BWC Impact	.574 (.152) *	1.475 (.186) **	.657 (.047) **	.844 (.067) *	1.001 (.162)	.696 (.015) **	.943 (.052)
PSA 2	.677 (.209)	.881 (.288)	.949 (.150)	1.018 (.263)	.363 (.153) *	.691 (.108) *	.858 (.102)
PSA 3	.462 (.049) **	.963 (.092)	1.049 (.045)	1.133 (.093)	1.390 (.152) **	.960 (.065)	.951 (.065)
PSA 4	1.845 (.804)	.949 (.342)	1.943 (.313) **	1.019 (.289)	1.125 (.481)	1.140 (.157)	.746 (.127)
PSA 5	.871 (.074)	1.322 (.119) **	1.898 (.048) **	1.267 (.055) **	1.377 (.053) **	.970 (.058)	.924 (.095)
PSA 6	1.240 (.567)	.820 (.312)	1.407 (.247)	.821 (.253)	1.134 (.522)	1.211 (.181)	.771 (.132)
PSA 7	1.681 (.095) *	2.039 (.161) **	2.273 (.052) **	1.141 (.055) **	2.567 (.086) **	1.005 (.053)	.884 (.076)
PSA 8	1.175 (.352)	1.454 (.371)	1.841 (.201) **	1.087 (.206)	2.280 (.656) **	1.129 (.112)	.802 (.113)
PSA 9	.750 (.243)	.5669 (.101) **	.918 (.049)	.599 (.079) **	1.895 (.328) **	.975 (.067)	1.124 (.147)
Trend	1.007 (.006)	.969 (.006) **	.989 (.002) **	.984 (.004) **	.956 (.006) **	1.018 (.001) **	.997 (.002)
Officers	1.009 (.004) *	.997 (.004)	1.003 (.002)	1.003 (.003)	1.005 (.005)	1.004 (.002) *	.999 (.001)
Calls for service	.999 (.001)	1.001 (.001) *	1.001 (.001) **	1.001 (.001) **	1.001 (.001)	1.001 (.001) **	1.001 (.001) **
Major Crime Incidents	1.003 (.009)	.994 (.002) *	1.006 (.001) **	1.007 (.003)	1.012 (.002)	.999 (.001)	1.001 (.001)
Constant	1.004 (1.143)	42.594 (42.573)**	40.374 (18.297)**	.937 (.686)	44.557 (55.287)**	436.522 (172.046)**	830.64(265.128)**
Log pseudolikelihood	-2231.331	-2498.676	-3587.439	-991.353	-9099.457	-62584.118	-10807.417
Pseudo R^2	.106	.365	.668	.118	.689	.756	.910
N	540	540	540	540	540	540	540

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

Notes: PSA 1 is the reference category for the PSA fixed effects dummy variables. IRR = Incidence Rate Ratio. RSE = Robust Standard Error. Robust standard errors were clustered by PSA.

Table 5. Poisson Regressions Estimating the Impact of the BWC Intervention on Monthly Counts of Selected Outcomes in NYPD PSAs (cont'd)

	Trespass Arrests IRR (RSE)	Disorder / Obstruct Arrests IRR (RSE)	Disorder Summonses IRR (RSE)
BWC Impact	.691 (.127) *	.977 (.279)	.393 (.118) **
PSA 2	.492 (.135) *	.803 (.474)	.439 (.192)
PSA 3	.846 (.070) *	.991 (.185)	1.076 (.129)
PSA 4	1.774 (.334) **	.989 (.510)	.332 (.132) **
PSA 5	1.312 (.101) **	1.117 (.096)	.416 (.022) **
PSA 6	.717 (.143)	.507 (.291)	.216 (.093) **
PSA 7	1.497 (.098) **	.771 (.076) **	2.091 (.129) **
PSA 8	.791 (.105)	.662 (.225)	1.049 (.275)
PSA 9	.269 (.023) **	.509 (.087) **	.837 (.163)
Trend	.979 (.003) **	.987 (.006)	.968 (.007) **
Officers	.999 (.003)	.998 (.007)	.995 (.005)
Calls for service	1.001 (.001)	1.001 (.001) **	.999 (.001)
Major crime incidents	.997 (.002)	1.005 (.005)	1.014 (.006) *
Constant	40.438 (20.441) **	1.365 (2.102)	44.658 (54.734) **
Log pseudolikelihood	-2640.481	-823.983	-2179.812
Pseudo R^2	.489	.061	.592
N	540	540	540

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

Notes: PSA 1 is the reference category for the PSA fixed effects dummy variables. IRR = Incidence Rate Ratio. RSE = Robust Standard Error. Robust standard errors were clustered by PSA. Month fixed effects variables included but not shown.

Alternative specifications of equation (1) includes the term $\gamma(PSA_i*Month_t)$, which represents PSA-specific linear time trends analysis. The inclusion of PSA-specific linear time trends enables one to assess if the main effects of BWCs hold after controlling for the potential for divergent trends across each PSA. Table 6 presents the results of the alternative specification of the main regression models that included monthly PSA-specific trends. The results generally confirmed the findings of the main effects models. After the deployment of BWCs, PSA-specific linear trends show statistically significant declines in monthly numbers of arrests, arrests with force, interior patrols, trespass arrests, and disorder summonses, as well as a statistically significant increase in monthly number of stop reports (all $p<.01$). The PSA-specific linear trends suggest monthly numbers of CCRB complaints declined by a similar amount ($p = .015$) but were not statistically significant at the threshold value of $p<.01$.

Table 6. Results of Poisson Regressions Estimating PSA Specific Linear Time Trends

Outcome Variables	**IRR (RSE)**
CCRB	0.554 (0.134)
Stops	1.358 (.142) **
Arrests	0.631 (0.041) **
Arrests w/ Force	.804 (.060) **
Summons	0.848 (0.106)
Interior Patrols	0.693 (0.016) **
Officer-Initiated Calls	0.933 (0.054)
Trespass Arrests	0.648 (0.124)
Disorder Arrests	0.624 (0.327)
Obstruction Arrests	1.087(0.276)
Disorder Summonses	0.364 (0.106) **

N = 540 for each model

** = $p<0.01$

Notes: IRR = Incidence Rate Ratio. RSE = Robust Standard Error. Robust standard errors were clustered by PSA. PSA fixed effects, month fixed effects, monthly linear trends, officers, calls for service, and major crime incidents included but not shown.

Using monthly numbers of arrests as an exemplar outcome measure, Figure 2 presents graphs of the month-specific coefficients and 95 percent confidence intervals estimating the effect of deploying BWCs on arrests in the months prior to and following implementation for the entire study period, and for the 12 months before and after BWC implementation, respectively. The figure shows that in the six months prior to BWC implementation, the arrest trends are parallel, while in the more distant past they were trending downward for all PSAs. Table 7 presents the event study model month-specific coefficients for arrest outcomes during the 12 months before and after BWC deployment on Housing Bureau officers. The coefficients generally show that the monthly counts of arrests before BWC deployment were associated with increased numbers of arrests (IRR>1), while the monthly counts of arrests after BWC implementation were associated with decreased numbers of arrests (IRR<1).

Figure 2. Event Study Model Graph, Arrests 12 Months Before and After BWC Deployment

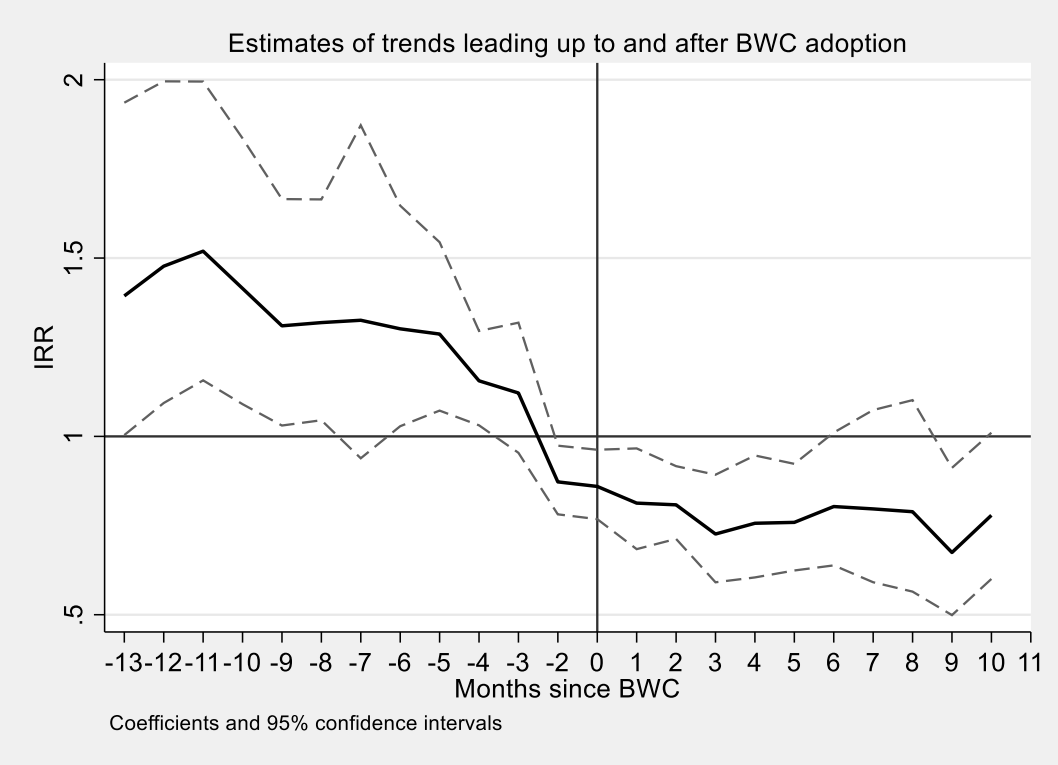


Table 7. Event Study Model Month Arrest Count Change Estimates, 12 Months Before and After BWC Deployment

	IRR (RSE)
Month -12	1.394
	(0.234)
Month -11	1.477
	(0.227)
Month -10	1.519**
	(0.211)
Month -9	1.415**
	(0.188)
Month -8	1.310
	(0.160)
Month -7	1.319
	(0.157)
Month -6	1.326
	(0.233)
Month -5	1.302
	(0.156)
Month -4	1.287**
	(0.120)
Month -3	1.156

	(0.067)
Month -2	1.122
	(0.093)
Month 0	0.872
	(0.049)
Month +1	0.860**
	(0.050)
Month +2	0.813
	(0.072)
Month +3	0.808**
	(0.052)
Month +4	0.726**
	(0.076)
Month +5	0.756
	(0.086)
Month +6	0.759**
	(0.076)
Month +7	0.803
	(0.094)
Month +8	0.797
	(0.121)
Month +9	0.789
	(0.134)
Month +10	0.674
	(0.104)
Month +11	0.779
	(0.103)

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

Notes: IRR = Incidence Rate Ratio. RSE = Robust Standard Error. Robust standard errors were clustered by PSA. Each row reflects the monthly estimate prior to and after implementation of BWC (-1 month is reference). PSA fixed effects, month fixed effects, officers, calls for service, and major crime incidents included but not shown.

C. Statistical Models for the PSA Officer-Level Analysis

For the officer-level analysis, the units of analysis in this evaluation were "officer-years" over a four-year period. The NYPD provided the Monitor Team with data on Housing Bureau officers who worked in the PSAs at the time BWCs were deployed in each of the nine PSAs (N = 1,903). Given temporal variations in BWC adoption dates across the nine PSAs, four one-year outcome observation periods were created for each officer, normalized by the deployment date in

each PSA (i.e., intervention year, -1 pre-intervention year, -2 pre-intervention year, -3 pre-intervention year).[29] However, given varying lengths of service, not all officers were observed for the entire four-year study period (e.g., 62 officers, or 3.3 percent, were rookies with less than one year of service before BWC deployment; 190 officers, or 10.0 percent, had one prior service year, 190 officers, or 14.7 percent, had two prior service years; and the remainder, 1,371 officers or 72.0 percent, had three or more prior service years). As such, the effective number of observations included in our evaluation was 6,766 "officer-years."

Panel regression models were then used to analyze the annual change in CCRB complaint counts (and other outcomes) for each Housing Bureau officer before and after the BWCs were deployed in their respective PSA commands, controlling for other covariates. A simplified version of a panel regression model was estimated as follows:

$$(3) \quad Y_{ijt} = \beta_0 + \beta_1 BWC_{ijt} + \sum_{t=-3}^{-1} \theta_{ij} Year_t + \beta X_{it} + \alpha_j + \varepsilon_{it}$$

Where $i = 1 \ldots n_i$ officers in j = 1…9 PSA commands, $t = 1, \ldots, n_t$ yearly observations, and Y_{ijt} is the outcome variable indicating the count of CCRB complaints for an individual officer in a specific PSA i during year t. The regressor *BWC* is a dummy variable identifying whether a PSA command adopted BWCs (1) or not (0) in given a year ($t = 1$), while *Year* are fixed effects dummy variables (= 1) identifying whether the officer-year CCRB complaint outcome was in the PSA in the three pre-intervention years t. The coefficient β_1 is the officer-specific effect of BWC during the treatment year, while θ are the officer-specific estimates from the three pre-intervention years. In this model, β represents the vector of estimates for covariates for each officer (X), such as age,

[29] Shorter observation periods were considered (e.g., quarters). However, the sparseness of outcome data in these shorter observation periods yielded excessive zero counts. For instance, as Table 4 shows, Housing Bureau officers generated only .175 CCRB complaints per year on the job (or less than one complaint every five years).

sex, race, rank, and years on the job. Fixed effects for each PSA (α) are included to control for common trends to individual officers, PSAs, and time. Standard errors (robust) are clustered at the individual-officer level.

The Housing Bureau BWC evaluation also had to account for the fact that circumstances of PSAs (contextual effects) on Housing Bureau officers could have varied over the course of the four-year study period. For instance, Patrol Services Bureau officers in precincts surrounding and intersecting with the PSAs were outfitted with BWCs at varying times during the study period. As such, NYPD Housing Bureau officers could have been impacted (receive indirect treatment) via their presence within a precinct that had already deployed BWCs for its officers prior to the deployment of BWCs in the Housing Bureau officers' PSAs. The presence of precinct officers with BWCs responding to calls for service in NYCHA housing developments, such as for providing backup for Housing Bureau officers, could also influence Housing Bureau officer and citizen behavior during these encounters. Thus, we ran the same panel regression models with PSAs represented as random effects rather than fixed effects over the four-year study period.[30] In these random-effects models, standard errors were clustered by PSA.

D. Results of Housing Bureau Officer-Level Analysis

Table 8 below presents the results of the Housing Bureau officer-level panel regression of outcomes, specified as fixed effects, controlling for individual officer characteristics.[31] The results suggest that outfitting Housing Bureau officers with BWCs improved the civility of police-civilian encounters and changed the officers' policing activities. Controlling for study year, PSA assigned,

[30] For a discussion of random effects model, see Stephen W. Raudenbush and Anthony S. Bryk. 2002. *Hierarchical Linear Models: Applications and Data Analysis Methods* (2nd edition). Thousand Oaks, CA: Sage.

[31] The results from a regression model without and with control variables are shown in Appendix C.

and officer characteristics, Housing Bureau officers generated 42.3 percent fewer CCRB complaints ($p<.01$) and reported using force in 20.3 percent fewer arrests ($p<.01$) during the BWC year relative to pre-intervention years. Housing Bureau officers made 23.3 percent fewer total arrests ($p<.01$) and 23.2 percent fewer trespassing arrests ($p<.01$) during the year they were equipped with BWCs relative to prior years. Summonses issued by Housing Bureau officers increased by 8 percent ($p<.01$) during the BWC year when compared to pre-intervention years, controlling for other covariates. However, disorder summonses issued by Housing Bureau officers dropped by 13.1 percent ($p<.01$) during the year they wore BWCs relative to prior years, holding other variables constant. Finally, the number of stop reports submitted by Housing Bureau officers increased sharply by 68.1 percent ($p<.01$) during the BWC year relative to pre-intervention years.[32]

Table 8. Panel Regression of Study Outcomes, by Year, PSA, and Officer Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables	**CCRB**	**Stops**	**Arrests**	**Force**	**Summonses**	**Trespass**	**Disorder Summonses**
BWC	0.557**	1.681**	0.767**	0.797**	1.080**	0.768**	0.869**
	(0.024)	(0.048)	(0.005)	(0.061)	(0.010)	(0.018)	(0.021)
Year 1	1.039	1.503**	1.824**	2.779**	4.628**	3.200**	4.244**
	(0.061)	(0.055)	(0.016)	(0.175)	(0.049)	(0.104)	(0.128)
Year 2	1.020	1.254**	1.615**	1.601**	2.359**	1.934**	1.697**
	(0.046)	(0.041)	(0.015)	(0.135)	(0.027)	(0.050)	(0.051)
PSA 2	1.245	0.478**	0.888**	0.658	0.884**	1.015	0.839*
	(0.162)	(0.051)	(0.034)	(0.162)	(0.034)	(0.084)	(0.063)
PSA 3	0.620**	0.833*	1.034	1.227	1.039	1.228*	1.057
	(0.102)	(0.074)	(0.043)	(0.289)	(0.043)	(0.110)	(0.085)
PSA 4	1.831**	1.315**	1.903**	2.952**	1.869**	2.673**	2.596**
	(0.237)	(0.109)	(0.087)	(0.683)	(0.087)	(0.256)	(0.188)
PSA 5	1.110	1.095	1.379**	1.355	1.350**	1.746**	1.658**
	(0.157)	(0.088)	(0.056)	(0.336)	(0.057)	(0.158)	(0.126)

[32] The command-level analysis estimated an effect on reported stops (+48%) that was similarly large, statistically significant, and in the same direction as the Housing Bureau officer-level analysis (+68%). The larger impact estimated by the Housing Bureau officer-level analysis was primarily due to observed reporting changes that occurred in the context of very sparse data. In the year prior to BWC implementation, 64.7 percent of Housing Bureau officers included in the analysis did not submit a single stop report (mean = .39 stop reports per officer). During the first BWC intervention year, 43.0 percent of Housing Bureau officers included in the analysis did not submit a single stop report (mean = .67 stop reports per officer).

PSA 6	1.113	1.446**	1.869**	3.446**	1.842**	2.478**	2.554**
	(0.184)	(0.116)	(0.095)	(0.771)	(0.095)	(0.259)	(0.206)
PSA 7	1.361*	0.939	1.860**	2.440**	1.822**	2.318**	2.553**
	(0.193)	(0.084)	(0.078)	(0.503)	(0.078)	(0.198)	(0.182)
PSA 8	1.020	1.150	1.392**	1.410	1.392**	1.763**	1.720**
	(0.158)	(0.091)	(0.065)	(0.340)	(0.065)	(0.170)	(0.138)
PSA 9	0.843	0.768**	0.862**	0.884	0.873**	0.951	0.837*
	(0.125)	(0.071)	(0.036)	(0.242)	(0.037)	(0.089)	(0.071)
Age	0.969**	1.001	0.994*	0.965*	0.994*	0.999	0.995
	(0.009)	(0.006)	(0.003)	(0.016)	(0.003)	(0.006)	(0.005)
Years on the job	1.024	0.939**	0.972**	0.915**	0.971**	0.952**	0.954**
	(0.013)	(0.008)	(0.003)	(0.021)	(0.003)	(0.007)	(0.006)
Sergeant	1.108	1.187*	0.922*	0.772	0.919*	1.071	0.889*
	(0.135)	(0.098)	(0.032)	(0.209)	(0.032)	(0.077)	(0.052)
Black	1.043	0.947	1.021	1.324	1.021	0.971	0.980
	(0.106)	(0.066)	(0.032)	(0.234)	(0.032)	(0.061)	(0.052)
Hispanic	1.074	0.970	0.986	0.959	0.987	1.064	0.968
	(0.091)	(0.053)	(0.026)	(0.125)	(0.026)	(0.059)	(0.042)
Asian/Other	0.972	0.989	1.029	1.277	1.020	1.125	1.025
	(0.125)	(0.074)	(0.041)	(0.241)	(0.041)	(0.094)	(0.068)
Female	0.763**	0.957	0.959	0.829	0.952	0.928	0.905*
	(0.073)	(0.059)	(0.026)	(0.138)	(0.025)	(0.057)	(0.041)
Observations	6,766	6,766	6,766	6,766	6,766	6,766	6,766
Number of Officers	1,903	1,903	1,903	1,903	1,903	1,903	1,903
Clusters	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* $p<0.05$
** $p<0.01$

Notes: Incident rate ratios displayed. Robust standard errors in parentheses. Year 3 is the reference year. PSA 1 is the reference PSA.

Table 9 presents the results of the Housing Bureau officer-level panel regression of outcomes, with PSA covariates specified as random effects as opposed to fixed effects, controlling for individual officer characteristics. The PSA random effects specification did not change the findings that equipping Housing Bureau officers with BWCs improved the civility of police-citizen outcomes and decreased their policing enforcement activities. Indeed, the direction, size, and statistical significance of all BWC intervention-year outcomes remained almost the same as the effects estimated in the officer-level regression models with PSA fixed effects.

Table 9. Panel Regression of Study Outcomes by Year and Officer Characteristics, PSA; Random Effects Models

Outcome	IRR (RSE)
CCRB	.552 (.024) **
Stops	1.677 (.038) **
Arrests	.764 (.015) **
Arrests w/Force	.778 (.094) *
Summonses	1.077 (.027) **
Trespass Arrests	.768 (.036) **
Disorder Summonses	.865 (.063) *

* $p<0.05$
** $p<0.01$

Notes: IRR = Incidence Rate Ratio. RSE = Robust Standard Error clustered by PSA. Pre-intervention year and officer covariates included but not shown.

VI. Estimating the Impact of BWCs on the Lawfulness of Stops by Housing Bureau Officers

A. Analytical Approach

As noted in Section I.B above, the Housing Bureau BWC evaluation also examined whether equipping Housing Bureau officers with BWCs would impact the lawfulness of their stops. The evaluation compared stop reports of officer before they were outfitted with BWCs to stop reports of officers after they were outfitted with BWCs. Each stop report had been evaluated by the Monitor Team for the lawfulness of the stop, frisk, and search described in the narratives in the stop report. The sampling methodology and the Monitor Team's process for assessing the legality of the stops, frisks and searches is described in Appendix A. The sampling methodology generated 684 stop reports made by Housing Bureau officers between January 1, 2017, and December 31, 2019, including 298 stops made by officers without BWCs and 350 stop reports made by Housing Bureau officers equipped with BWCs.[33]

[33] It is noteworthy that the sampling procedure yielded 17.4 percent more stop reports (+52) for review when officers wore BWCs relative to when officers did not have BWCs. Given that the sampling procedure allowed all stop reports within each command to have an equal probability of selection over time, this suggests that Housing

A series of bivariate chi-square and standardized mean difference statistical tests were used to explore differences in stop characteristics, officer actions, and lawfulness assessment outcomes in treatment (with BWCs) and control (without BWCs) groups. Multivariate logistic regressions also were used to estimate the impact of the BWCs on officer actions and lawfulness assessments of those actions, controlling for characteristics of the stops.[34] To ensure that the coefficient variances accounted for violations of the homoscedastic errors assumption of linear regression models, robust standard errors clustered by PSA were used. Parameter estimates were expressed as odds ratios (OR).[35]

B. Results for Legality Outcomes, Stop Report Characteristics

Table 10 compares the gender, race/ethnicity, suspected crime, officer actions, how the encounter was generated (radio run, officer-initiated, or direct contact from victim/witness) and the lawfulness of the officer's actions for stop reports of officers without BWCs relative to the stop reports of officers with BWCs. In both groups, stopped civilians tended to be younger Black and Hispanic males suspected of a range of crimes. Stopped individuals were more likely to be Black non-Hispanic subjects (70.9% v. 61.7%, respectively, $p<.05$) and less likely to be white Hispanic subjects (15.4% v. 25.8%, respectively, $p<.05$) during the BWC intervention period relative to the period before BWC deployment. Housing Bureau officers in the stop reports were more likely to

Bureau officers outfitted with BWCs during the study period filled out more stop reports than Housing Bureau officers without cameras.

[34] Multivariate logistic regressions model binary outcome variables, in which the log odds of the probability of the outcomes occurring versus not occurring are modeled as a linear combination of the predictor variables. John H. Aldrich and Forrest D. Nelson. 1984. *Linear Probability, Logit, and Probit Models.* Quantitative Applications in the Social Sciences, Paper 45. Newbury Park, CA: Sage Publications.

[35] The OR is defined as the ratio of the odds of A in the presence of B and the odds of A in the absence of B. OR greater than 1 suggests a positive relationship between the occurrence of A and the presence of B, while OR less than 1 suggests a negative relationship. *See* Magdalena Szumilas. 2010. "Explaining Odds Ratios." *Journal of the Canadian Academy of Child and Adolescent Psychiatry*, 19: 227–29.

be mobilized through radio runs (50.0% v. 28.9%, respectively, $p<.05$) and less likely to conduct officer-initiated encounters (41.1% v. 63.4%, respectively, $p<.05$) during the BWC intervention period relative to the period before BWC deployment. Moreover, Housing Bureau officers were more likely to stop civilians on suspected violent crimes (27.4% v. 16.4%, respectively, $p<.05$) and other/unknown crimes (5.4% v. 2.3%, respectively, $p<.05$), and less likely to stop civilians based on disorder offenses (22.6% v. 41.3%, respectively, $p<.05$) and drug offenses (4.6% v. 8.4%, respectively, $p<.05$) during the BWC intervention period relative to the period before BWC deployment.

Individuals were frisked and issued summonses in similar percentages of pre-intervention and BWC-intervention stops made by Housing Bureau officers. However, civilians in BWC intervention stops relative to pre-intervention stops were much less likely to be searched (30.6% v. 47.0%, respectively, $p<.05$) and arrested (33.4% v. 49.3%, respectively, $p<.05$). Table 10 also presents the results of the stop report lawfulness audits conducted by the Monitor Team. Overall, the justifications reported for stopping civilians were a little more likely to be regarded as lawful in the BWC intervention stop reports relative to the pre-intervention stop reports (67.1% v. 58.1%, respectively, $p<.05$). In stop reports that involved a frisk (N = 362 of 648), the justifications reported for frisking civilians in Housing Bureau officer stop reports during the BWC intervention period were not significantly more or less likely to be regarded as constitutional when compared to Housing Bureau officer stop reports during the pre-intervention period. In approximately the one-third of stop reports that reported a search (N = 247 of 648), the justifications reported for searching civilians in stop reports by Housing Bureau officers equipped with BWCs were less likely to be lawful when compared to stop reports by Housing Bureau officers who were not equipped with BWCs (90.7% v. 97.1%, respectively, $p<.05$).

Table 10. Comparison of Stop Report Characteristics During Pre-Intervention and BWC Intervention Periods

	Pre-Int.		Intervention		Std. Mean Difference
	N	%	N	%	
Male	251	84.2%	311	88.9%	.068
Female	46	15.4%	36	10.3%	
Missing	1	0.3%	3	0.8%	
Black non-Hispanic	184	61.7%	248	70.9%	.096*
White Hispanic	77	25.8%	54	15.4%	-.129*
Black Hispanic	21	7.0%	28	8.0%	.018
White non-Hispanic	10	3.4%	11	3.1%	-.006
Asian / other / missing	6	2.0%	9	2.6%	.018
Mean age (SD)	273	26.5 (11.7)	316	28.0 (12.1)	.065
Mobilization					
Radio run	86	28.9%	175	50.0%	.215*
Officer-initiated	189	63.4%	144	41.1%	-.222*
Complainant / witness	23	7.7%	31	8.9%	.021
Suspected crime					
Violent	49	16.4%	96	27.4%	.131*
Weapon	78	26.2%	114	32.6%	.070
Property	16	5.4%	26	7.4%	.042
Disorder	123	41.3%	79	22.6%	-.201*
Drug	25	8.4%	16	4.6%	-.078*
Other / unknown	7	2.3%	19	5.4%	.078*
Officer action outcomes					
Monitor – Lawful stop	173	58.1%	235	67.1%	.094*
Frisked suspect	156	52.3%	206	58.9%	.065
Monitor – Lawful frisk	139	89.1%	179	86.9%	-.033
Searched suspect	140	47.0%	107	30.6%	-.168*
Monitor – Lawful search	136	97.1%	97	90.7%	-.146*
Arrested suspect	147	49.3%	117	33.4%	-.161*
Issued summons	10	3.4%	13	3.7%	.010

Total N = 648 (Pre-intervention N = 298, Intervention N = 350)

* $p<0.05$

Table 11 presents the results of the multivariate logistic regressions of Housing Bureau officer action outcomes on whether the stop report was completed during the BWC intervention period relative to the pre-intervention period, controlling for stop characteristics. The results show that for four out of the five outcomes there was a significant reduction in the percent of stops with searches and arrests or summonses during the BWC intervention period relative to the pre-period. Similar to the bivariate analyses, subjects stopped by Housing Bureau officers outfitted with BWCs were not more or less likely to be frisked, but were much less likely to be searched and arrested/summonsed relative to subjects stopped by control officers before they were outfitted with cameras.[36] Controlling for stop characteristics, BWC intervention period stop reports were associated with a statistically significant 46.6 percent decrease ($p<.05$) in the odds that a search was conducted relative to the odds that a search was not conducted when compared to pre-intervention stop reports. Holding the other covariates constant, the predicted marginal effects of the equipping officers with BWCs suggests that 30.0 percent of BWC intervention stops involved a search, while 50.3 percent of the pre-intervention stops involved a search. Further, relative to pre-intervention stop reports, stop reports prepared after BWC deployment were associated with a statistically significant 39.8 percent decrease ($p<.05$) in the odds that a subject was arrested/summonsed relative to the odds that a subject was arrested/summonsed in a stop report submitted during the pre-intervention period, holding the other covariates constant. The predicted marginal effects of equipping officers with BWCs suggests that 38.4 percent of BWC intervention stops resulted in an arrest/issued summons, while 60.1 percent of pre-intervention stops resulted in an arrest/issued summons, controlling for the other variables.

[36] Due to the small number of stop reports involving the issuance of a summonses, the arrest and summons officer action outcomes were collapsed into one binary variable (1 = arrested / summonsed, 0 = not arrested / summonsed).

Table 11 also presents the results of the multivariate logistic regressions of the impact of BWC intervention on Monitor Team assessments of the lawfulness of stops, frisks, and searches in the stops, controlling for stop characteristics.[37] While the effects of BWCs on whether the stop was judged to be lawful by the Monitor Team were in the same positive direction as the bivariate comparison in Table 10, the results were no longer statistically significant when stop characteristics were considered in the multivariate analysis. However, relative to pre-intervention stop reports, stop reports prepared after BWC deployment were associated with a statistically significant 41.9 percent decrease ($p<.05$) in the odds that a frisk/search conducted during a stop was assessed as constitutional relative to the odds that a pre-intervention frisk/search conducted during a stop was assessed as constitutional, holding the other covariates constant. The predicted marginal effects of the placement of BWCs on Housing Bureau officers suggests that 70.3 percent of BWC intervention frisks and searches met the appropriate lawfulness standard while 78.1 percent of the pre-intervention frisks and searches met the appropriate lawfulness standard controlling for the other covariates.

[37] As a result of the small number of stop reports involving searches, the frisk and search officer action outcomes were collapsed into one binary variable (1 = frisked/searched, 0 = not frisked/searched).

Table 11. Multivariate Logistic Regressions of BWC Impact on Officer Action Outcomes, Controlling for Stop Characteristics

	Frisked	Searched	Arrested/ Summonsed	Lawful Stop	Lawful Frisk/Search
Covariate	**OR (RSE)**	**OR (RSE)**	**OR (RSE)**	**OR (RSE)**	**OR (RSE)**
BWC treatment	.890 (.203)	.534 (.124)*	.602 (.111)*	1.442 (.337)	.581 (.118)*
Marginal effect:					
BWC Intervention	.570	.300	.384	.683	.703
Pre-period	.659	.503	.601	.571	.781
Male	2.165 (1.287)	.953 (.299)	.989 (.337)	1.562 (.483)	1.379 (.584)
Black non-Hispanic	1.878 (.853)	.834 (.375)	.478 (.292)	1.974 (.899)	1.151 (.326)
White Hispanic	1.623 (.822)	.870 (.437)	.561 (.287)	1.237 (.563)	.955 (.467)
Black Hispanic	1.278 (.791)	1.345 (.648)	1.390 (.767)	1.257 (.530)	.567 (.334)
Asian / other	.932(.682)	.380 (.208)	.647 (.378)	.552 (.533)	---
Age	.984 (.008)*	1.028 (.012)*	1.033 (.015)*	1.026 (.007)*	1.022 (.008)*
Radio run	1.245 (.539)	2.596 (.723)*	1.610 (.835)	.538 (.305)	1.078 (.501)
Officer-initiated	.682 (.209)	2.536 (1.118)*	1.759 (1.001)	.425 (.251)	.802 (.388)
Violent	.855 (.369)	.365 (.277)	.963 (.614)	3.071 (1.755)*	1.236 (.709)
Property	.702 (.297)	.660 (.449)	1.024 (.619)	1.971 (.948)	1.092 (.814)
Drug	.997 (.637)	.531 (.435)	1.354 (1.306)	11.472 (9.454)*	1.091 (.645)
Weapon	12.824 (5.408)*	1.832 (1.337)	7.317 (5.544)*	1.294 (.769)	9.653 (5.274)*
Disorder	.417 (.155)*	.383 (.272)	.571 (.401)	1.948 (1.157)	2.415 (1.844)
Constant	.608 (.618)	.298 (.296)	.595 (.382)	.280 (.204)	.579 (.690)
Log pseudolikelihood	-293.885	-338.981	-305.032	-361.239	-317.242
Pseudo R^2	0.2718	0.147	0.251	.079	.113
N	589	589	589	589	438

* $p<0.05$.

Notes: OR = Odds Ratio. RSE = Robust Standard Error. Robust standard errors were clustered by PSA. Female was reference category for the male covariate. White non-Hispanic suspect was the reference category for the other race covariates. Complainant/witness-initiated stop was the reference category for the mobilization covariates. Other and unknown suspected crime was the reference category for the suspected crime type categories. The Asian dummy variable was omitted from the Lawful Frisk / Search regression because there were zero applicable cases.

VII. Conclusion

The NYPD deployed BWCs on its officers working in nine PSAs between February 2018 and December 2018. It is important to note that the BWC implementation occurred when there were large declines in NYPD enforcement activity in and around NYCHA housing buildings. The quasi-experimental designs measured the impact of BWC deployment on Housing Bureau officers controlling for these existing trends and other factors. The findings from the PSA command-level and officer-level quasi-experiments generally complemented each other and suggest that the BWC deployment produced significant changes in the civility of police-civilian encounters, police enforcement activity, and the reporting of civilian stops. However, the results presented here are limited by the completeness and quality of the data provided by the NYPD.[38] Further, this evaluation was not a randomized controlled trial.[39] As such, the quasi-experimental results present limited causal evidence of the impact of BWC deployment on Housing Bureau officer activities.

The Housing Bureau BWC evaluation documented significant reductions in CCRB complaints (-43% PSA command-level, -42% officer-level) and arrests that involved the use of force (-16% PSA command-level, -20% officer-level) when the BWC deployment year was compared to pre-deployment years. The citywide randomized controlled trial measuring the impact of BWCs on Patrol Services Bureau officer behaviors reported reductions in CCRB

[38] Two examples reported here help to illustrate some of these limitations. The NYPD Housing Bureau officer roster data provided did not include information on work assignments prior to and following their immediate PSA assignment. As such, we were not able to look at BWC treatment contamination issues, nor were we able to look at possible treatment spillover effects into other commands. With respect to use of force, NYPD use of force data was limited to officer reports of any force used during an arrest and do not indicate whether the force applied was excessive or not.

[39] Former Mayor De Blasio and NYPD decided to adopt BWCs on a citywide basis starting in December 2017 and completed in December 2019. While the citywide deployment had benefits for City residents and the NYPD, citywide deployment of BWCs in the PSAs meant that meant that it was not possible to randomize allocation of BWCs to treatment and control units within PSAs.

complaints but not arrests with use of force.[40] The available evaluation literature suggests that BWCs do seem to reduce civilian complaints against officers;[41] however, the evidence assessing the impacts of BWCs on officers' use of force is more mixed, with a recent review suggesting significant reductions in officer force.[42] Nevertheless, for the NYPD and other police departments, increased civility could generate considerable collateral benefits, such as fewer injuries to civilians and officers and reduced civil litigation. Civil police-citizen interactions are less likely to escalate into unfortunate outcomes, such as police shootings. These findings support the continued use of BWCs by NYPD Housing Bureau officers.

The Housing Bureau BWC evaluation also documented significant reductions in arrests (-34% PSA command-level, -23% officer-level), trespass arrests (-31% PSA command-level, -23% officer-level), and disorder summonses (-60% PSA command-level, -13% officer-level) during the BWC implementation year relative to pre-implementation years. It is unclear why there was a larger reduction in disorder summonses at the PSA command-level when compared to the officer-level.[43] The officer-level analysis also suggested a small eight percent increase in total

[40] *See* Monitor's Twelfth Report – Results and Evaluation of NYPD's Body Worn Camera Pilot Program (11/30/20), *available at* https://www.nypdmonitor.org/wp-content/uploads/2022/09/12th-Report.pdf.

[41] Lum, C., Stoltz, M., Koper, C. S., & Scherer, J. A. (2019). "The research on body-worn cameras: What we know, what we need to know." *Criminology & Public Policy*, *18(1)*, 93–118.

[42] Morgan C. Williams Jr., Nathan Weil, Elizabeth A. Rasich, Jens Ludwig, Hye Chang, and Sophia Egrari. (2021). *Body-Worn Cameras in Policing: Benefits and Costs.* NBER Working Paper 28622. Cambridge, MA: National Bureau of Economic Research.

[43] The PSA command-level and Housing Bureau officer-level analyses represent two different units of analysis with varying observation periods. The disorder summons counts were aggregated at the PSA command-level and include some unknown share of disorder summonses made by non-Housing Bureau officers during the 60-month observation period. The NYPD provided the rosters of Housing Bureau officers who were assigned to the PSAs at the time when BWCs were implemented in each PSA. Housing Bureau officer-level outcomes were compared for a BWC intervention year relative to three pre-intervention year Housing Bureau officer-level outcomes. As noted in the methods section, some Housing Bureau officers were observed for less than three pre-intervention years given when they were appointed NYPD officers. It is also unknown whether officers spent some or all of the pre-intervention years in other commands. Given these differences, it is noteworthy that the estimated BWC effects of

summonses after BWC implementation, while the PSA command-level analysis reported no difference in total summonses after the BWC implementation. Given the overall 90 percent decrease in summonses in NYCHA housing between 2013 and 2019, it is possible that particular Housing Bureau officers may have been marginally more likely to issue summonses when outfitted with BWCs relative to an aggregate PSA count that could include some summonses made by non-Housing Bureau officers. In the command-level analysis, the Housing Bureau BWC evaluation also found that officers conducted 30 percent fewer interior patrols after the deployment of BWCs. Equipping Housing Bureau officers with BWCs did not alter the number of officer-initiated calls for service in the PSAs.[44] The overall weight of the evidence on policing activity outcomes suggests that equipping Housing Bureau officers with BWCs further decreased the downward trajectory of enforcement activity in NYCHA housing.

The number of stop reports submitted by Housing Bureau officers, however, increased dramatically in the year after BWC deployment relative to pre-deployment years (+48% PSA command-level analysis, +68% officer-level analysis). Further, the analysis of randomly selected stop reports suggest that stops were more likely to involve Black subjects, less likely to originate from officer-initiated activities, and more likely to involve violent crimes during the BWC deployment year. The results suggest a possible shift in stop report activity—increased stop reports were more focused on substantive crime activities and not generated by officer-initiated enforcement of more subjective crime categories such as disorderly behavior and drug sales. Stops made by Housing Bureau officers after BWC deployment were also less likely to involve an arrest

---

the command-level and Housing Bureau officer-level analyses are in the same direction and statistically significant ($p<0.01$).

[44] Calls for service and interior patrol data were only available at the PSA level of aggregation and not available for individual Housing Bureau officers.

or summons, less likely to involve a search, and those stops with searches were less likely to be judged as lawful relative to stops with searches made before BWC implementation. Finally, it is worth noting that the increase in the number of stop reports did not correspond to a decrease in the proportion of those stops found to be legally sufficient.

Consistent with the randomized controlled experimental findings reported in the Monitor's Twelfth Report, the increased number of stop reports seems to be an artifact of the surveillance potential of the BWC technology. The analyses of NYPD stops support the position that the increase in stop reports made following BWC deployment may be influenced by a heightened willingness on the part of NYPD officers to file such reports given the associated video documentation of stops created by the BWCs. The stops made by Housing Bureau officers after BWC deployment were also less likely to produce stops reports that involved full searches, the issuance of a summonses, or the arrest of suspects, when compared to stops made by Housing Bureau officers without BWCs. The increased share of stop reports without additional enforcement actions (searches, summonses, or arrests) implies that Housing Bureau officers outfitted with BWCs increased their documentation of less intrusive encounters that might not have resulted in stop reports in the absence of the BWC technology. The presence of the BWCs seems to enhance Housing Bureau officer compliance with NYPD policy directives requiring the documentation of civilian stops.

The increased documentation of stops involving less serious encounters with citizens suggests that BWCs deter officers from failing to document stops. NYPD policy requires officers to activate BWCs during all pedestrian stops, and officers must document these encounters by filing stop reports. The availability of BWC video for specific encounters increases the likelihood that precinct commanders will detect unreported stops. While it is not known whether officers

were in fact disciplined for failing to submit a stop report, either before or after officers were equipped with BWCs, the failure to submit a stop report can subject an officer to a disciplinary violation. The presence of revealing video therefore likely increases officer perceptions that policy violations would be detected, given that the video decreases the need for supervisors to locate and interview people involved in the encounter.

It is obviously concerning that NYPD officers continue to make some stops that involve unlawful searches of citizens. However, the finding that the deployment of BWCs on Housing Bureau officers resulted in the documentation of larger numbers of stops relative to pre-deployment is fundamentally good news. Put simply, if the NYPD is not aware that a problem exists, they are not able to remedy the underlying conditions that cause the problem to persist. The deployment of BWCs on Housing Bureau officers not only appears to increase their compliance with NYPD directives to document all stops, but it also provides the Department with an important opportunity to intervene and monitor their progress toward ensuring constitutional policing.

Appendix A

Monitor Team Methodology for Sampling and Reviewing Stop Reports

The stop reports (and the Monitor Team's assessments) used for this evaluation were taken from the stop reports reviewed by the Monitor Team as part of its general monitorship compliance reviews. The Monitor Team developed a two-prong sampling methodology that involved cluster random sampling and simple random sampling to select representative samples of NYPD stop reports beginning January 1, 2017, and continuing through December 31, 2019.

The first sampling methodology involves stop reports audited by the NYPD's Quality Assurance Division, which is the NYPD unit that conducts audits of the Department's activities. Each quarter, QAD sends the Monitor Team a list of the 131 commands in which QAD had audited the stop reports during the previous quarter. These commands include the nine PSAs. The Monitor Team then would randomly select a smaller number of commands from which it would review all the stop reports audited by QAD in those commands.

The selection of commands to be reviewed by the Monitor Team was guided by the steps below. First, a statistical power analysis determined that a yearly sample of at least 1,200 stop reports was required to ensure a representative annual group of stop encounters. These 1,200 stop reports were selected on a rolling quarterly basis with a sample of at least 300 stop reports selected each quarter. This approach allowed sufficient numbers of stops to inform decision-making on compliance and to provide timely feedback to the NYPD and QAD auditors on the accuracy of their own internal assessments. The Monitor Team was able to evaluate the overall stop reporting process from report preparation, through the command's self-inspection process, to the final audits done by QAD by selecting and reviewing stop reports in aggregate by command (cluster random sampling). Using the command as the sampling unit allowed the Monitor Team to identify a

representative sample of stop reports and command self-inspections, permitting a more global view of the entire NYPD stop reporting procedure and process.

In addition to the sampling of stop reports audited by QAD by command, the Monitor Team also reviewed a random sample of stop reports selected from all stops recorded in NYCHA housing developments. Officer completing stop reports record whether the stop was made in Housing (NYCHA developments), Transit (NYC subways), or Patrol (street stops). A simple random sample of stop reports was selected from those listed as being conducted in Housing.

For each stop report selected, the NYPD produced the stop report and a printout of the communications between the NYPD dispatcher and the officers responding. This printout is from the NYPD's Intergraph Computer Aided Dispatch System (ICAD). These documents were used by the Monitor Team to evaluate the lawfulness of the stop, as well as that of any frisk and/or search if they were conducted.

The steps below describe the multi-stage evaluation process used by the Monitor Team:

1. Two Monitor Team reviewers (Reviewer 1 and Reviewer 2) independently assessed the lawfulness of the stop, frisk, and search described in the narratives provided in each stop report.

2. Each reviewer examined the stop report and any associated ICAD printout (the radio dispatch). The reviewer would examine the officer's narrative in the stop report describing the circumstances that led to the stop, as well as what was listed by the officer as the crime suspected, to determine whether the officer articulated reasonable suspicion of a felony or Penal Law misdemeanor. If a frisk and/or a search was conducted, the reviewer would review the officer's narrative describing the circumstances of the frisk and/or the search to assess whether the officer had reasonable suspicion that the person stopped was armed and dangerous, justifying a frisk, or if the officer had a justifiable legal basis for the search.

3. Reviewer 1 and 2 shared their independent assessments with each other and discussed their findings, resulting in a list of stops where: (1) both reviewers disagreed with the assessment of lawfulness made by QAD on the stop, frisk, or search; and (2) the reviewers disagreed with each other on the lawfulness of the police actions described in the stop report.

4. A third reviewer independently assessed the lawfulness of the stop, frisk, and search described in the stop reports for which there was disagreement, and then shared those

results with Reviewers 1 and 2. All three Reviewers then discussed their views, resulting in a list of stop reports with disagreement (either with QAD or among the team).

5. The Monitor and Deputy Monitor would review the stop reports with disagreements and then all five (Monitor, Deputy Monitor, and Reviewers 1, 2, and 3) discussed the stop reports and reached a conclusion on the lawfulness of the encounter described in each stop report.

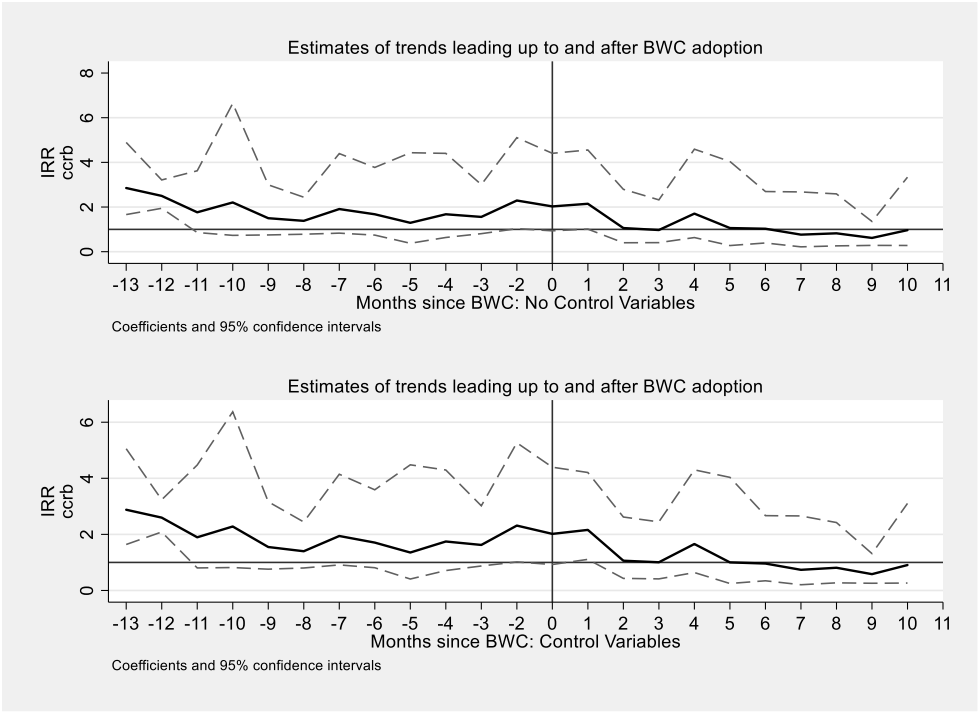
6. The Monitor Team's list of legally insufficient stop reports was subsequently sent to the NYPD for review.

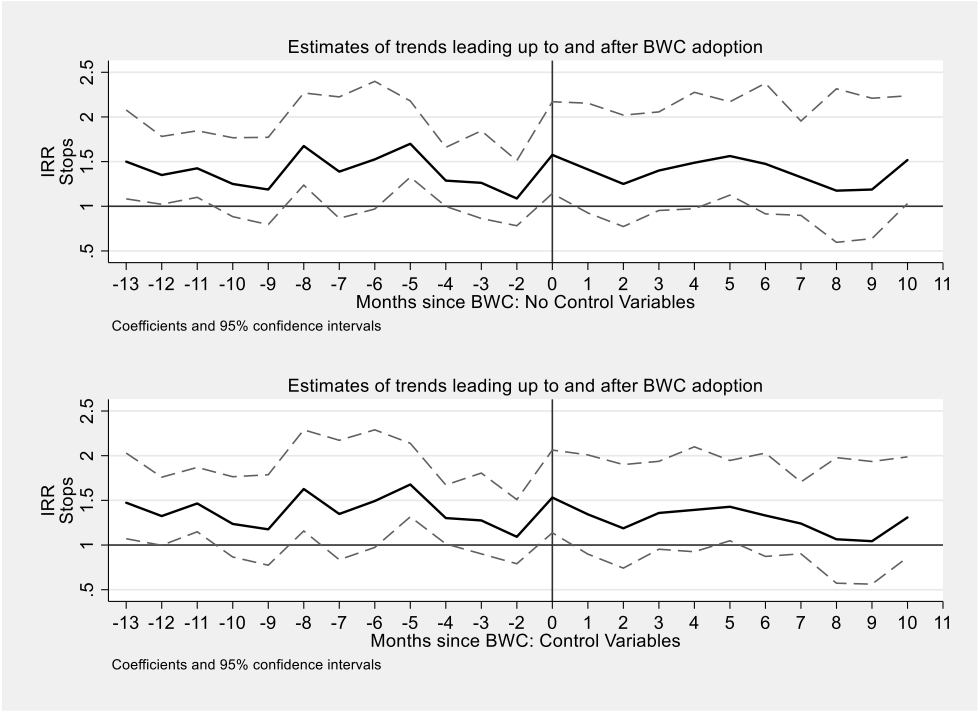
7. The Monitor Team and representatives from the NYPD Risk Management Bureau (RMB) and QAD then met to discuss the stop reports identified by the Monitor Team that did not articulate a legal basis for either the stop, frisk, or search.

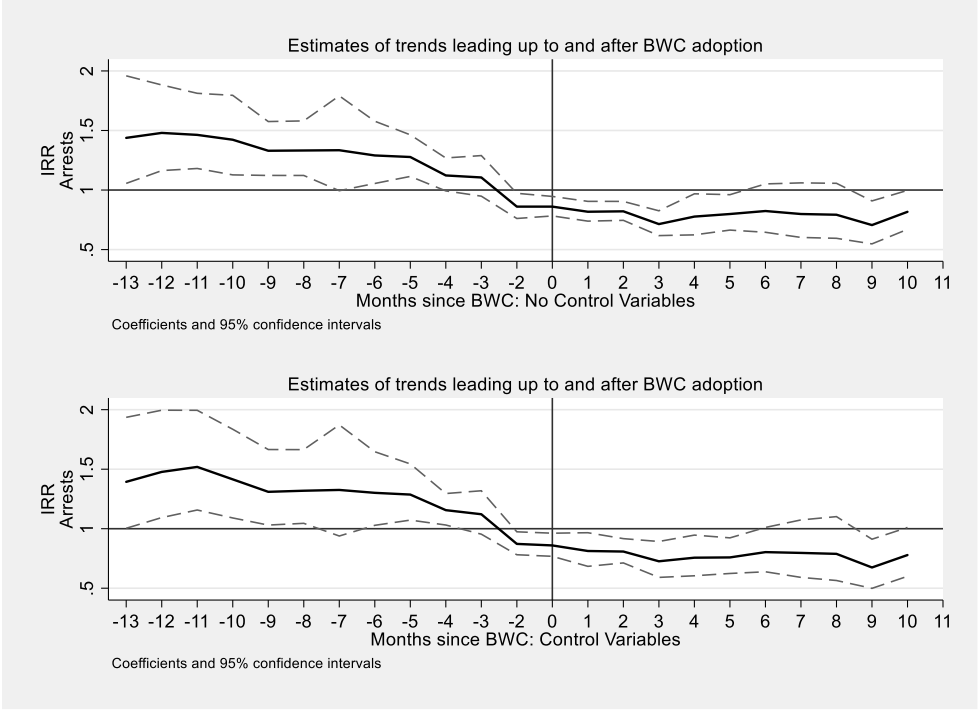
8. After the Monitor Team meeting with RMB and QAD, the Monitor Team made a final assessment of the lawfulness of police actions in the selected stop reports for that quarter.

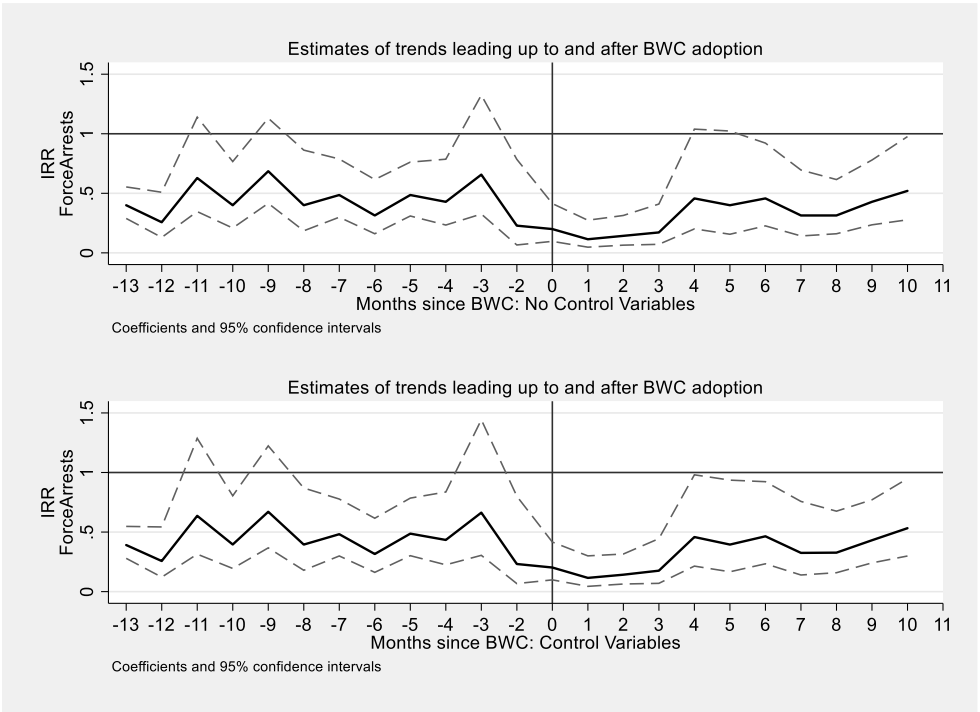
Appendix B

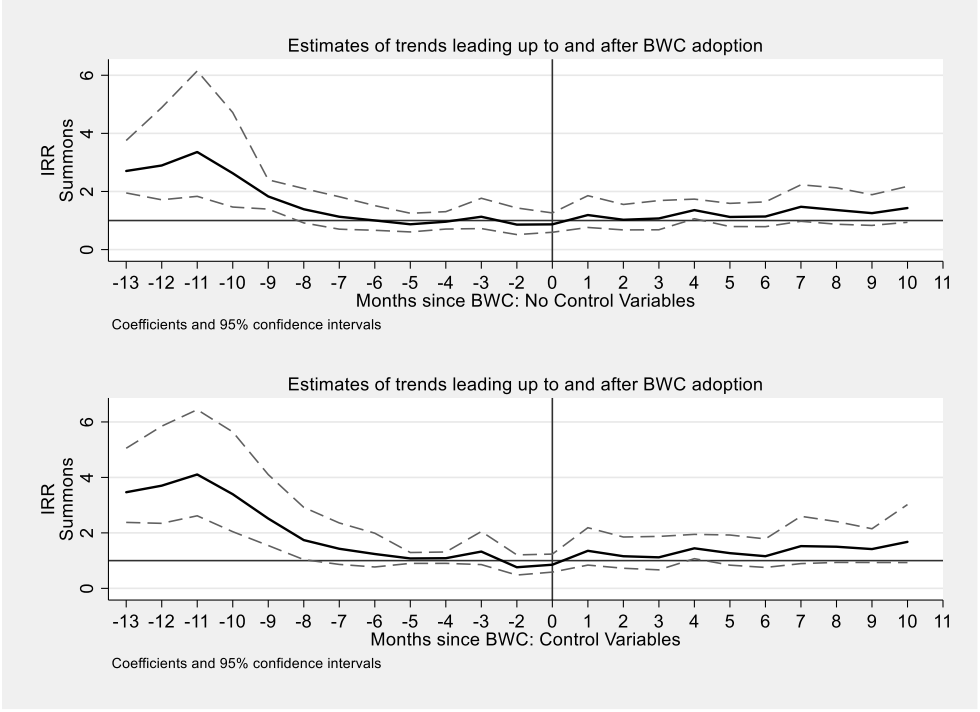
Graph of Trend in Aggregate Outcomes With and Without Control Variables

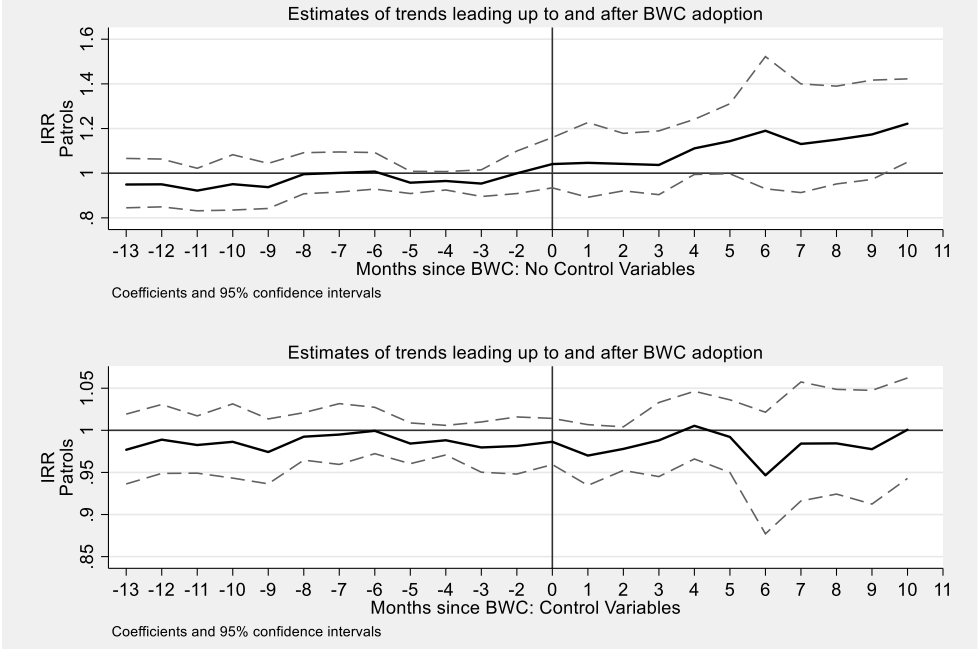


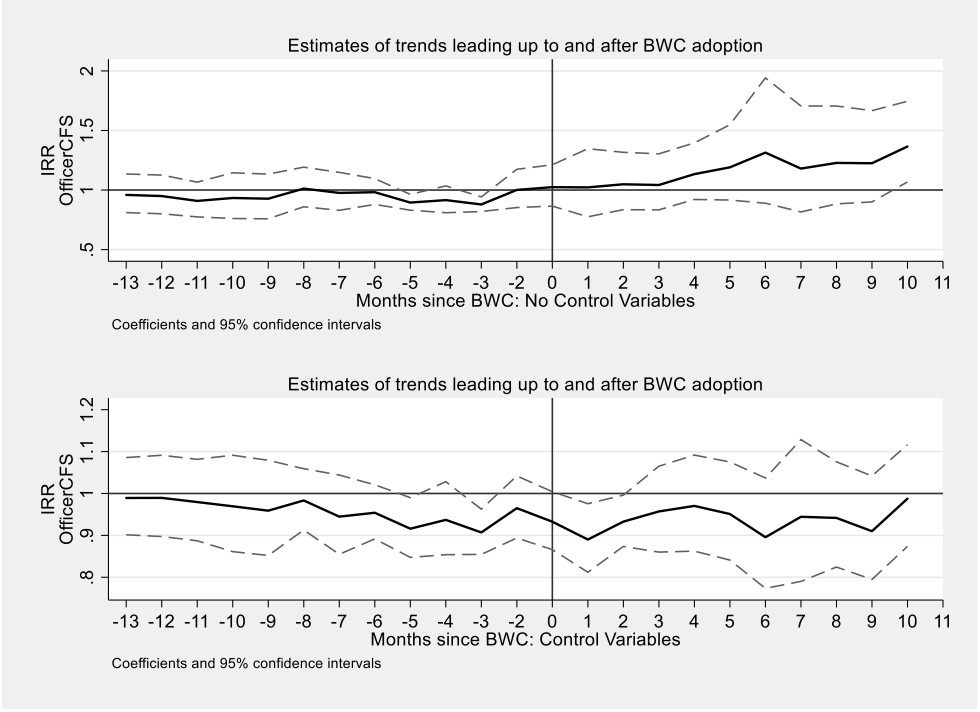


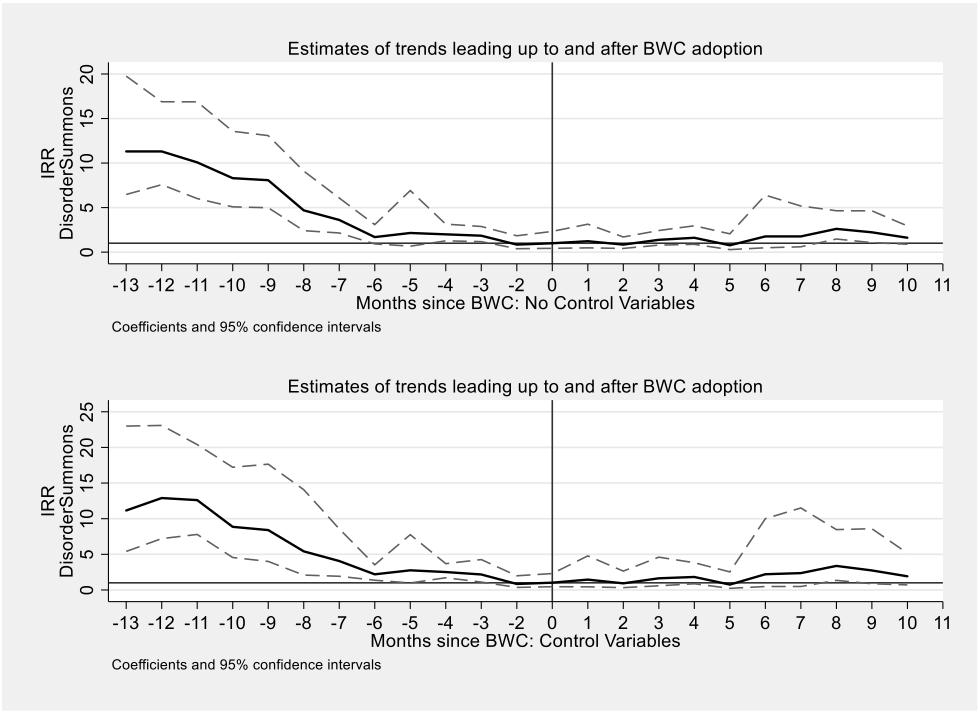

Estimates of trends leading up to and after BWC adoption
IRR Arrests
.5
1
1.5
2
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: No Control Variables
Coefficients and 95% confidence intervals
Estimates of trends leading up to and after BWC adoption
IRR Arrests
.5
1
1.5
2
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: Control Variables
Coefficients and 95% confidence intervals


Estimates of trends leading up to and after BWC adoption
IRR ForceArrests
0
.5
1
1.5
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: No Control Variables
Coefficients and 95% confidence intervals
Estimates of trends leading up to and after BWC adoption
IRR ForceArrests
0
.5
1
1.5
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: Control Variables
Coefficients and 95% confidence intervals


Estimates of trends leading up to and after BWC adoption
IRR
Summons
0
2
4
6
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: No Control Variables
Coefficients and 95% confidence intervals
Estimates of trends leading up to and after BWC adoption
IRR
Summons
0
2
4
6
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: Control Variables
Coefficients and 95% confidence intervals


Estimates of trends leading up to and after BWC adoption
IRR
Patrols
.8
1
1.2
1.4
1.6
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: No Control Variables
Coefficients and 95% confidence intervals
Estimates of trends leading up to and after BWC adoption
IRR
Patrols
.85
.9
.95
1
1.05
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: Control Variables
Coefficients and 95% confidence intervals


Estimates of trends leading up to and after BWC adoption
IRR
OfficerCFS
.5
1
1.5
2
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: No Control Variables
Coefficients and 95% confidence intervals
Estimates of trends leading up to and after BWC adoption
IRR
OfficerCFS
.8
.9
1
1.1
1.2
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: Control Variables
Coefficients and 95% confidence intervals


Estimates of trends leading up to and after BWC adoption
IRR
DisorderSummons
0
5
10
15
20
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: No Control Variables
Coefficients and 95% confidence intervals
Estimates of trends leading up to and after BWC adoption
IRR
DisorderSummons
0
5
10
15
20
25
-13 -12 -11 -10 -9 -8 -7 -6 -5 -4 -3 -2 0 1 2 3 4 5 6 7 8 9 10 11
Months since BWC: Control Variables
Coefficients and 95% confidence intervals

Appendix C

Panel Regression of Study Outcomes on Year, PSA, and Officer Characteristics With and Without Control Variables

Variables	(1) CCRB	(2) CCRB	(3) Stops	(4) Stops	(5) Arrests	(6) Arrests	(7) Force	(8) Force	(9) Summons	(10) Summons	(11) Trespass	(12) Trespass Arrests	(13) Disorder Summons	(14) Disorder Summons
Year 1	1.035	1.039	1.412**	1.503**	1.788**	1.824**	2.544**	2.779**	4.475**	4.628**	3.056**	3.200**	3.908**	4.244**
	(0.061)	(0.061)	(0.051)	(0.055)	(0.015)	(0.016)	(0.152)	(0.175)	(0.049)	(0.049)	(0.092)	(0.104)	(0.109)	(0.128)
Year 2	1.019	1.020	1.223**	1.254**	1.600**	1.615**	1.549**	1.601**	2.323**	2.359**	1.895**	1.934**	1.641**	1.697**
	(0.046)	(0.046)	(0.040)	(0.041)	(0.015)	(0.015)	(0.127)	(0.135)	(0.026)	(0.027)	(0.047)	(0.050)	(0.048)	(0.051)
Year 4	0.558**	0.557**	1.697**	1.681**	0.771**	0.767**	0.809**	0.797**	1.087**	1.080**	0.775**	0.768**	0.880**	0.869**
	(0.024)	(0.024)	(0.048)	(0.048)	(0.005)	(0.005)	(0.061)	(0.061)	(0.010)	(0.010)	(0.018)	(0.018)	(0.021)	(0.021)
PSA 2	1.257	1.245	0.500**	0.478**	0.924	0.888**	0.683	0.658	0.921*	0.884**	1.070	1.015	0.884	0.839*
	(0.164)	(0.162)	(0.053)	(0.051)	(0.039)	(0.034)	(0.184)	(0.162)	(0.039)	(0.034)	(0.096)	(0.084)	(0.071)	(0.063)
PSA 3	0.612**	0.620**	0.862	0.833*	1.062	1.034	1.191	1.227	1.066	1.039	1.273*	1.228*	1.086	1.057
	(0.100)	(0.102)	(0.078)	(0.074)	(0.049)	(0.043)	(0.301)	(0.289)	(0.049)	(0.043)	(0.124)	(0.110)	(0.095)	(0.085)
PSA 4	1.776**	1.831**	1.300**	1.315**	1.867**	1.903**	1.967**	2.952**	1.841**	1.869**	2.640**	2.673**	2.550**	2.596**
	(0.232)	(0.237)	(0.112)	(0.109)	(0.091)	(0.087)	(0.460)	(0.683)	(0.092)	(0.087)	(0.263)	(0.256)	(0.200)	(0.188)
PSA 5	1.109	1.110	1.149	1.095	1.416**	1.379**	1.290	1.355	1.383**	1.350**	1.876**	1.746**	1.698**	1.658**
	(0.157)	(0.157)	(0.096)	(0.088)	(0.067)	(0.056)	(0.332)	(0.336)	(0.067)	(0.057)	(0.191)	(0.158)	(0.145)	(0.126)
PSA 6	1.122	1.113	1.367**	1.446**	1.760**	1.869**	2.120**	3.446**	1.744**	1.842**	2.324**	2.478**	2.365**	2.554**
	(0.184)	(0.184)	(0.114)	(0.116)	(0.090)	(0.095)	(0.485)	(0.771)	(0.091)	(0.095)	(0.249)	(0.259)	(0.197)	(0.206)
PSA 7	1.376*	1.361*	1.002	0.939	1.930**	1.860**	2.245**	2.440**	1.897**	1.822**	2.470**	2.318**	2.678**	2.553**
	(0.191)	(0.193)	(0.092)	(0.084)	(0.089)	(0.078)	(0.500)	(0.503)	(0.090)	(0.078)	(0.218)	(0.198)	(0.206)	(0.182)
PSA 8	1.060	1.020	1.196*	1.150	1.399**	1.392**	1.306	1.410	1.403**	1.392**	1.812**	1.763**	1.723**	1.720**
	(0.166)	(0.158)	(0.099)	(0.091)	(0.071)	(0.065)	(0.333)	(0.340)	(0.072)	(0.065)	(0.187)	(0.170)	(0.150)	(0.138)
PSA 9	0.840	0.843	0.719**	0.768**	0.818**	0.862**	0.696	0.884	0.828**	0.873**	0.880	0.951	0.779**	0.837*
	(0.125)	(0.125)	(0.068)	(0.071)	(0.038)	(0.036)	(0.200)	(0.242)	(0.038)	(0.037)	(0.089)	(0.089)	(0.070)	(0.071)
Age		0.969**		1.001		0.994*		0.965*		0.994*		0.999		0.995
		(0.009)		(0.006)		(0.003)		(0.016)		(0.003)		(0.006)		(0.005)
Years on Job		1.024		0.939**		0.972**		0.915**		0.971**		0.952**		0.954**
		(0.013)		(0.008)		(0.003)		(0.021)		(0.003)		(0.007)		(0.006)
Sgt		1.108		1.187*		0.922*		0.772		0.919*		1.071		0.889*
		(0.135)		(0.098)		(0.032)		(0.209)		(0.032)		(0.077)		(0.052)
Black		1.043		0.947		1.021		1.324		1.021		0.971		0.980
		(0.106)		(0.066)		(0.032)		(0.234)		(0.032)		(0.061)		(0.052)

Hispanic		1.074		0.970		0.986		0.959		0.987		1.064		0.968
		(0.091)		(0.053)		(0.026)		(0.125)		(0.026)		(0.059)		(0.042)
Asian/Other		0.972		0.989		1.029		1.277		1.020		1.125		1.025
		(0.125)		(0.074)		(0.041)		(0.241)		(0.041)		(0.094)		(0.068)
Female		0.763**		0.957		0.959		0.829		0.952		0.928		0.905*
		(0.073)		(0.059)		(0.026)		(0.138)		(0.025)		(0.057)		(0.041)
/lnalpha	1.544	1.483	0.591	0.456*	0.178**	0.142**	4.910**	3.949**	0.156**	0.119**	0.472**	0.414**	0.205**	0.122**
	(0.432)	(0.428)	(0.201)	(0.177)	(0.030)	(0.024)	(1.416)	(1.237)	(0.032)	(0.026)	(0.099)	(0.095)	(0.071)	(0.059)
Observations	6,766	6,766	6,766	6,766	6,766	6,766	6,766	6,766	6,766	6,766	6,766	6,766	6,766	6,766
Number of Officers	1,903	1,903	1,903	1,903	1,903	1,903	1,903	1,903	1,903	1,903	1,903	1,903	1,903	1,903
Cluster	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* p<0.05

** p<0.01

Notes: Incident rate ratios displayed. Robust standard errors in parentheses.