From: Rebecca Kemble <rebecca@renaissanceresourcesllc.com>
Sent: Saturday, December 4, 2021 1:42 PM
To: All Alders <allalders@cityofmadison.com>
Cc: Haas, Michael R <MHaas@cityofmadison.com>; Edgerton, Sarah <SEdgerton@cityofmadison.com>; keetrascob@gmail.com
Subject: Legistar #68625 Implementing Body-Worn Camera Pilot Program

Dear Alders and Mayor -

I'm writing with concerns about the resolution to be introduced at next Tuesday's meeting regarding the implementation of a body-worn camera pilot program, and the apparent oversight of GMO 23.63, Use of Surveillance Policy located in Chapter 23, Offenses Against Public Policy. This ordinance clearly spells out a pubic approval process before any new surveillance technology, or additional pieces of surveillance technology already in use, can be used. Since this ordinance was passed in June, 2020, I have spoken with prior and current Council Presidents about the need for Council to develop and implement these processes. I know that hadn't happened by the time of my departure from Council in April of this year.

Since the surveillance ordinance was put in place prior to the Police Civilian Oversight Board being established in law, it did not contemplate a role for the PCOB in this approval process. However, as co-author on both GMO 23.63 and GMO 5.20 which established the PCOB as a body that is "...to review and make recommendations regarding police discipline, use of force, and other policies and activities, including related to rules, hiring, training, community relations, and complaint processes", I strongly recommend codifying a role for the PCOB in the approval process required by 23.63. I've copied Chair Burnette on this email for that reason.

Thousands of hours of staff, committee and Alder time over several years have gone into creating these two ordinances. I hope you will review and discuss how best to move forward with an approval process, and follow up on the requirement for annual departmental reports on surveillance technology and the Annual Surveillance Technology Report.

Thank you for your consideration. Please let me know if any of you would like to discuss this further.

Rebecca Kemble
4217 School Rd.
608 347-8097

From: jxd@athletics.wisc.edu <jxd@athletics.wisc.edu>
Sent: Sunday, December 5, 2021 1:45 PM
To: All Alders <allalders@cityofmadison.com>
Subject: [All Alders] Police body cameras

Recipient: All Alders

Name: jerry darda
Address: 15 sturbridge circle, Madison, WI 53717
Phone: 608-831-9420
Email: jxd@athletics.wisc.edu

Would you like us to contact you? Yes, by email

Message:

Please provide our good police officers with needed body cameras which will tell the truth when violent controversial encounters occur. Folks in our neighborhood think this is no brainer.

Jerry & Bonnie Darda
&
Isabella Marina Rocha

From: annewalker@homelandgarden.com <annewalker@homelandgarden.com>
Sent: Monday, December 6, 2021 1:40 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Body cams

Dear Alders

I am uncomfortable with the idea of police officers wearing body cams. Specifically, I am worried about some of the unintended consequences of body cams.

Years ago, I needed to file a restraining order and domestic stalking ordinance against my ex-husband. I worked with rock star police officer Jean Pappalia. Had Officer Pappalia been wearing a body cam, I doubt I would have been as forthcoming about the issues I was facing. In turn, not being as forthcoming with an officer can result in receiving less help from the very people who are qualified to provide it.

Respectfully,

Anne Walker

From: Gregory Gelembiuk <gwgelemb@wisc.edu>
Sent: Monday, December 6, 2021 3:37 PM
To: All Alders <allalders@cityofmadison.com>; Rhodes-Conway, Satya V. <SRhodes-Conway@cityofmadison.com>
Subject: Regarding Legistar #68625 Implementing Body-Worn Camera Pilot Program

Dear Alders and Mayor,

I am writing regarding Legistar #68625 "Implementing Body-Worn Camera Pilot Program".

This resolution cites the Body-Worn Camera Feasibility Review Committee report, but appears to ignore all the preconditions specified in the committee report, that the report states should be satisfied before any use of bodycams here (e.g., requiring a rigorous randomized controlled trial; requiring that the D.A. first make specific firm commitments to prevent adverse effects; requiring that MPD adopt the policies in the report with at most minor modifications; etc.). See page 8 of the report:

> "While the Committee struggled to come to consensus on whether to recommend for or against BWCs, the Committee was unanimous that BWCs should *only* be implemented if done so in a context that includes good policies and procedures as part of an overall package of reforms that enhances the potential for desired effects and minimizes the potential for unintended harms as much as possible. Thus, the following should be strict preconditions for implementation of BWCs.
>
> Madison should adopt a BWC program only if:"

None of that is in place, and there's nothing in the text of the resolution that suggests these conditions will be required before proceeding with a pilot.

For example, the Dane County District Attorney has not "*formally enacted a policy to review any relevant BWC video before making a charging decision in any case referred by MPD where BWC video is available*", nor has the D.A. "*firmly committed to measures sufficient to prevent an overall increase in charging rates and criminalization in low-level offenses caused by MPD BWC implementation.*" Nor is there in place "*a system and or process for sharing BWC video footage files – preferably an electronic file sharing system if feasible – with the Dane County District Attorney's Office and the Public Defender's Office in time for informing charging decisions for cases referred by MPD for potential criminal charges.*"

Likewise, the report specifies as a precondition that "*MPD has formally adopted the BWC policies recommended by the Body-Worn Camera Feasibility Review Committee with, at most, minor modifications that do not alter the essential substance and principles outlined in this Report and in the Model Policy*." These policies are extensive and contain many unusual features - for example, the committee report specifies that a new additional discovery process be in place, giving individuals or their attorneys immediate access to BWC video whenever someone is charged (see policy provision 8f). Instead, the proposed resolution says "*Police Chief Barnes should have input into the specific policies and procedures governing the use*" and that after the pilot, MPD must provide a report "*describing… policies and procedures governing their use*". In terms of policy, the resolution appears to give MPD the latitude to do whatever it wants.

Importantly, the resolution and fiscal note for this item appear not to take into account that the report specifies a requirement for "*a rigorous, randomized controlled trial as a pilot program, with tracking and analysis of data on key outcomes, and particularly prosecutorial charging rates.*" The full cost of such a trial, including analysis, is not estimated in the report and thus is not provided in the fiscal note (which appears to assume a cost of only $138,000, as MPD projected for its original North Side Pilot Project concept). In contrast, here is a grant application for an initial randomized controlled trial of BWCs in Milwaukee. The grant specifies 50 cameras (similar to the number of cameras proposed in the resolution for the Madison pilot). The total cost includes a $399,746 contract with the Urban Institute to administer/analyze the randomized controlled trial.

For operating costs, the proposed resolution specifies only: "*$55,000 in operating funds to cover overtime for processing the video. MPD has indicated that the Department intends to absorb these costs within their existing 2022 adopted operating budget*". Basically, this appears to be saying the cost will be absorbed into departmental overtime. But the $55,000 only covers the cost of the specific additional positions required for the pilot BWC program (per MPD, 250 hours each for a FSU Lab Technician, a Management Information Specialist 2, a Clerk Typist, and a Program Assistant 1). Besides not covering the cost of a rigorous randomized controlled trial, this figure doesn't cover the cost in time for training all North District patrol officers, sergeants, and Community Policing Team officers to use BWCs (police departments generally provide 1-3 full days of training) and even more importantly, it doesn't cover the cost of officer workload time required for BWCs.

Data from a large number of surveys, worklog analyses, etc. across multiple police departments provide pretty consistent estimates, that officers wearing BWCs require roughly 30 minutes a shift for BWC-related tasks (e.g., reviewing videos for case reports or court testimony, cataloguing videos, tagging videos with meta-data, specifying individuals or features that need to be redacted, uploading videos, etc.). For extensive documentation of BWC impact on workload, see Appendix 3 (page 11) here. These are new BWC-related tasks that officers have to spend administrative time on, with this additional workload reducing time available for patrol and other core policing functions. Thus, either time spent on existing core policing functions will need to be reduced, or a commensurate increase in overtime will be required to maintain the same level of service. None of this is accounted for in the fiscal note.

In addition, I doubt that the $83,000 specified for BWC purchase covers the cost of BWC features the committee report says must be included. For example, the report specifies that "*technologies shall be adopted that automatically engage the recording equipment whenever squad car lights are activated, squad car doors are opened, officers are dispatched to an incident by the dispatch center, the camera system detects that an officer is running, or other similar automatic engagement systems offered by the technology.*" Such automatic triggering systems are expensive. But the $83,000 specified in this proposed resolution is the same amount that MPD originally specified for equipment purchase and maintenance for a North District pilot project, without any such automatic triggers. The cost of fulfilling the report's mandate, for extensive automatic trigger systems, appears not to be covered.

MPD and the alders sponsoring this resolution appear to be proceeding as though the Body-Worn Camera Feasibility Review Committee report, with its specific content, didn't exist – ignoring its actual recommendations and, as far as I can see, just using it as a green light/fig leaf to justify enacting MPD's original pre-committee BWC plan without substantive modification.

The preconditions and policies in the committee report are designed to ameliorate known adverse effects of implementing BWCs. As I've noted previously, I don't believe that harms and costs can be sufficiently ameliorated for BWCs to provide, on balance, a net benefit. The case of BWCs may bear a resemblance to some other technological "solutions to problems" that were widely implemented and almost universally lauded before their adverse consequences or lack of efficacy came to be adequately recognized (e.g., nuclear power, asbestos insulation, the insecticide DTT, lobotomies, etc.). With good reason, I fear that BWCs will exacerbate Madison's sky-high racial disparities in arrests and prosecutions (one of the highest among U.S. cities), with particular impact in Madison's most heavily policed communities. But if Madison were to move forward with BWCs, it should at least try to somewhat mitigate the known negative effects, as specified in the report.

Finally, in case anyone might be interested, here's a documentary worth watching, contemplating bodycams and surveillance. It has a 93% rating from film critics on the review aggregator site Rotten Tomatoes and was the winner of the Sundance 2021 Special Jury Prize for Nonfiction Experimentation.
https://superltd.com/films/all-light-everywhere?fbclid=IwAR24lkLMQkchQcG9omfHHjpkoKLmLLqaywgiHpdYXuj30JEcon7lYQfqsS8

All Light, Everywhere | Super LTD

ALL LIGHT, EVERYWHERE is an exploration of the shared histories of cameras, weapons, policing and justice. As surveillance technologies become a fixture in everyday life, the film interrogates the complexity of an objective point of view, probing the biases inherent in both human perception and the lens.

superltd.com

Sincerely,

Dr. Gregory Gelembiuk

From: lbgoodrich@icloud.com <lbgoodrich@icloud.com>
Sent: Monday, December 6, 2021 10:37 PM
To: All Alders <allalders@cityofmadison.com>
Subject: [All Alders] Police cameras

Recipient: All Alders

Name: Lynn Jenkins
Address: 555 S. Midvale Blvd #214, Madison, WI 53711
Phone: 608-575-8797
Email: lbgoodrich@icloud.com

Would you like us to contact you? Yes, by email

Message:

Please count me as one Madison resident who expects to hold every Madison police officer accountable for how they manage difficult situations with all of our Madison residents. I want to see what is happening and then our prosecutors to use that information to keep bad decisions to a minimum. Please vote in favor of the body cams, even if it means increasing my taxes.

From: Mara Eisch <eisch.mara@gmail.com>
Sent: Tuesday, December 7, 2021 10:24 AM
To: All Alders <allalders@cityofmadison.com>
Subject: CC Agenda 104

Please let's get this pilot study done!! What is there to fear from the results of this study?
Mara

From: Jake Winkler <trappedinink@gmail.com>
Sent: Tuesday, December 7, 2021 2:06 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Police body cam pilot

How is next Tuesday's resolution in compliance with GMO 23.63, Use of Surveillance Policy located in Chapter 23, Offenses Against Public Policy? This ordinance requires a public approval process for new surveillance tech.
GMO 23.63 was created prior to the creation of the Police Civilian Oversight Board and so does not mention PCOB, but it would be wise to include PCOB in the process, and indeed this new surveillance resolution is in the purview of the ordinances that created PCOB.
Most activists are not calling for body cams. If body cams are implemented, the details matter a lot as to the success (which is why most activists are not pushing for them). Who has access to the footage? Does MPD have to release footage of an incident on a specific timeline? Not to mention the steep cost to purchase, train staff, and store the video data.

Jake Winkler
5306 Barton Rd

From: Erin Lemley <afuzzybird@gmail.com>
Sent: Tuesday, December 7, 2021 2:38 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Say "No" to body-worn cameras pilot

Dear Madison Alders,

I see that we are once again picking up the issue of body-worn cameras for our police officers, and I am writing in opposition to the pilot program and to future adoption of the cameras. Although it is tempting to believe that the program will solve all of our police department's problems, cameras are not a panacea and in fact will increase money going to our police department that could be going to programs that actually help reduce crime, such as housing (that's a big one for out city right now, isn't it!), education, healthcare, and workforce development. We are looking for a quick fix when there isn't one--the fix is to invest in our people.

In addition, this pilot program is clearly against GMO 23.63, as explained by Rebecca Kemble in her letter to the council.

I am asking you to reject this pilot program and to think of better uses for this money which will positively impact the lives of our community members in need.

Sincerely,
Erin Lemley
1703 Rowland Ave, Madison, WI 53704

From: Erica Ramberg <erica.ramberg@gmail.com>
Sent: Tuesday, December 7, 2021 2:47 PM
To: All Alders <allalders@cityofmadison.com>
Subject: NO to body cameras

Dear Alders and Mayor -

I'm writing with concerns about the resolution being introduced today regarding the implementation of a body-worn camera pilot program, and the apparent oversight of GMO 23.63, Use of Surveillance Policy located in Chapter 23, Offenses Against Public Policy. This ordinance clearly spells out a public approval process before any new surveillance technology, or additional pieces of surveillance technology already in use, can be used. Furthermore, the new Police Civilian Oversight Board should have a critical role in this public approval process.

I ask that you reject item 104 on the agenda. This item erases the work of this council and previous members to be accountable to the community when considering methods for policing them.

My best
Erica Ramberg

515 Briar Hill Rd, Madison, WI 53711

From: Morgan Mayer-Jochimsen <mmayerjochimsen@gmail.com>
Sent: Tuesday, December 7, 2021 3:08 PM
To: All Alders <allalders@cityofmadison.com>; Mayor <Mayor@cityofmadison.com>
Subject: Opposition to body-cam pilot program

Dear Alders and Mayor -
I'm writing in opposition to the implementation of a body-worn camera pilot program. This program has not undergone the required process delineated in GMO 23.63, Use of Surveillance Policy located in Chapter 23, Offenses Against Public Policy. The ordinance clearly spells out a public approval process before any new surveillance technology, or additional pieces of surveillance technology already in use, can be used. Since this ordinance was passed in June, 2020, I am concerned that it has not been implemented and is not being followed. I hope you will review and discuss how best to move forward with a public approval process.
Thank you,
Morgan Mayer-Jochimsen
423 N Pinckney St. Apt 2
Madison, WI 53703

From: Alexandra Wilburn <wilburnalexandra@gmail.com>
Sent: Tuesday, December 7, 2021 4:00 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Opposed to body cams

Hello,

As Kemble pointed out, this violates an ordinance in place.

Also as an activist I've tried to get footage from Portage Police department, and not only did it take weeks of a public awareness campaign to get the footage given to myself as an advocate for John King who was brutally assaulted but we weren't even given all of the footage the department held onto footage even longer making it difficult for John King to fight for his freedom after being assaulted.

Not to mention the files were incredibly difficult to navigate, and software had to be downloaded to view the footage.

This is a program that will continue the illogical system of allowing police to oversee themselves.

For those that believe in reform. Whats the point of writing a check for tech that will only cause more process issues.

Lets get our PCOB, independent monitor FULLY FUNDED and they can have the discussions with community on bodycams

Best,

Alexandra Wilburn

P.s.

Tell Ismael ozanne to never run for office again as he has failed to bring the Derek chauvin of Dane County to Justice. Matt kenny should be in prison for intentional homicide of Tony Robinson Jr.

Inpursuitofjustice.net for more info

From: David Keller <david@kellerrealestategroup.com>
Sent: Tuesday, December 7, 2021 6:10 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Agenda item 104 (68625) Common Council Meeting Tonight

Dear Alders,
I tried to register my support for Agenda item #104 on your Common Council Agenda registration website today but it was not working, hence I am sending you this email so that my support of the Body Worn Camera Pilot program is received by each of you.

I wholeheartedly agree with establishing the Pilot program – something many, many other cities throughout the Country have implemented with success!!!

Thank you and please have the IT department look into why I was not able to register online.

David C. Keller
1102 Winston Drive,
Madison, WI 53711

From: kiramilanich@gmail.com <kiramilanich@gmail.com>
Sent: Tuesday, December 7, 2021 3:08 PM
To: All Alders <allalders@cityofmadison.com>
Subject: oppose the body-worn camera pilot program

I am writing to ask you to oppose item #104; the body-worn camera pilot program. I agree with what former alder Kemble wrote about this:

"I'm writing with concerns about the resolution to be introduced at next Tuesday's meeting regarding the implementation of a body-worn camera pilot program, and the apparent oversight of GMO 23.63, Use of Surveillance Policy located in Chapter 23, Offenses Against Public Policy. This ordinance clearly spells out a pubic approval process before any new surveillance technology, or additional pieces of surveillance technology already in use, can be used. Since this ordinance was passed in June, 2020, I have spoken with prior and current Council Presidents about the need for Council to develop and implement these processes. I know that hadn't happened by the time of my departure from Council in April of this year.
Since the surveillance ordinance was put in place prior to the Police Civilian Oversight Board being established in law, it did not contemplate a role for the PCOB in this approval process. However, as co-author on both GMO 23.63 and GMO 5.20 which established the PCOB as a body that is "...to review and make recommendations regarding police discipline, use of force, and other policies and activities, including related to rules, hiring, training, community relations, and complaint processes", I strongly recommend codifying a role for the PCOB in the approval process required by 23.63. I've copied Chair Burnette on this email for that reason.
Thousands of hours of staff, committee and Alder time over several years have gone into creating these two ordinances. I hope you will review and discuss how best to move forward with an approval process, and follow up on the requirement for annual departmental reports on surveillance technology and the Annual Surveillance Technology Report. "

Thank you,

Kira Milanich
3741 Johns St
Madison, WI
53714

From: Keith Findley <keith.findley@wisc.edu>
Sent: Tuesday, January 4, 2022 1:02 PM
To: All Alders <allalders@cityofmadison.com>; Mayor <Mayor@cityofmadison.com>; Aisha Moe <aishasmoe@gmail.com>
Cc: Tom Brown <tbrown@ulgm.org>; Myadze, Charles <myadzec@gmail.com>; Kapusta-Pofahl, Karen <KKapusta-Pofahl@cityofmadison.com>; Keith Findley <keith.findley@wisc.edu>; Kim Jorgensen (dokithia@gmail.com) <dokithia@gmail.com>; Luke Schieve (lschieve@exactsciences.com) <lschieve@exactsciences.com>; Tom Brown (tbrown@ulgm.org) <tbrown@ulgm.org>; Veronica Figueroa (veronicaf@unidoswi.org) <veronicaf@unidoswi.org>; Haas, Michael R <MHaas@cityofmadison.com>; Austin, Brian <BAustin@cityofmadison.com>
Subject: Body Worn Cameras--New Research

Dear Mayor and Alders,

As former co-chair of the city's Body-Worn Camera Feasibility Review Committee, I wanted to keep you updated on the latest research that has emerged on the effects of Body-Worn Cameras (BWCs). As you know, the research to date has reported wide diversity in effects, depending to a great extent on location and policies under which BWCs are implemented. One of the key concerns opponents in Madison have expressed is that BWCs might have the unintended effect of increasing the rate at which police officers arrest or prosecutors charge individuals with low-level offenses. On that issue, a new randomized, controlled study of the implementation of BWCs in New York City has found that "The BWC intervention was not associated with any statistically significant changes in the number of arrests, arrests with force, summons, domestic incident reports, and citizen crime complaint reports. The BWC intervention, however, was associated with a statistically significant 38.8 percent increase in the count of stop reports submitted by NYPD officers and a statistically significant 21.1 percent reduction in the CCRB complaints made against police officers ($p < .05$ for both outcomes)." In essence, the study found no increase in criminalization, but a reduction in citizen complaints and an increase in officer compliance with report-writing requirements. The study concluded: "this study finds that the placement of BWCs on officers can increase their compliance with department directives to document stops of citizens. These data can then be used to determine whether officers are adhering to the rule of law in their enforcement efforts. In sum, BWCs could be useful in reducing persistent problems with unlawful citizen stops."

A copy of the full study is attached.

Best regards,

Keith A. Findley
Professor
University of Wisconsin Law School
975 Bascom Mall
Madison. WI 53706
Cell: 608-335-4544

Received: 21 November 2020 | Revised: 6 July 2021 | Accepted: 13 July 2021

DOI: 10.1111/1745-9125.12293

ARTICLE

CRIMINOLOGY

Body-worn cameras, lawful police stops, and NYPD officer compliance: A cluster randomized controlled trial*

Anthony A. Braga[1] | **John M. MacDonald**[1] | **James McCabe**[2]

[1] University of Pennsylvania

[2] Sacred Heart University

Correspondence
Anthony A. Braga, Department of Criminology, University of Pennsylvania, 558 McNeil Building, 3718 Locust Walk, Philadelphia, PA 19104-6286.
Email: abraga@upenn.edu

*The authors of this article currently serve on the federal monitoring committee on the settlement agreements in *Floyd, et al. v. City of New York, et al.*, 08 Civ. 1034 (AT), *Ligon, et al., v. City of New York, et al.*, 12-CV-2274 (AT), and *Davis et al., v. City of New York*, et al., 10-CV-00699 (AT). The analyses and opinions expressed in the article reflect those of the authors only and not any other entity.

The work reported in this article was funded by the City of New York as part of the settlement agreements.

The authors would like to thank Peter Zimroth, Richard Jerome, other monitor team members, and the NYPD for their assistance with this research.

Abstract

The federal court settlement of Floyd, et al. v. City of New York, et al. (2013) mandated that the New York City Police Department (NYPD) implement a series of reforms to address unlawful stop, question, and frisk patterns and practices. Among other changes, the remedial order required the NYPD to implement and evaluate a pilot body-worn camera program to determine whether outfitting officers with the technology led to more lawful and civil police–citizen encounters. A cluster randomized controlled trial involving 40 police precincts and 3,889 NYPD officers was used to evaluate the effects of body-worn cameras on a series of police work activity, civility, and lawfulness outcomes. Relative to control officers, citizen complaints against treatment officers outfitted with body-worn cameras were reduced by 21 percent. Treatment officers, however, also filed nearly 39 percent more stop reports when compared with control officers. Treatment stop reports tended to involve minority subjects, were less likely to involve arrests and summons, and were significantly more likely to be rated as not meeting constitutional justifications for stops, frisks, and searches. These results suggest that body-worn cameras improved NYPD officer compliance with mandates to document all stops and could be used to

address unlawful policing through better detection of problematic police–citizen encounters.

KEYWORDS
body-worn cameras, cluster randomized controlled trial, lawful policing, New York City Police Department (NYPD), stop-and-frisk

1 | INTRODUCTION

The presence of body-worn cameras (BWCs) during police–citizen encounters has become a prominent feature of urban policing around the world. In 2013, approximately 24 percent of local police departments serving U.S. cities with populations of 250,000 residents or larger reported the deployment of BWCs on their officers (Reaves, 2015). By 2016, ~80 percent of large local police departments (agencies with 500 or more full-time sworn officers) had acquired BWCs, and 70 percent had started outfitting active duty officers with the technology (Hyland, 2018). The growth in the adoption of BWCs in the United States was fueled in response to persistent problems with police–community relations, as well as in response to concerns with police shootings of unarmed Black citizens (see, e.g., Lum et al., 2015; White, 2014). President Obama's Task Force on 21st Century Policing (2015) considered the use of BWCs as a technological innovation that could help build community trust and police legitimacy and noted the need to provide financial support to offset the considerable costs to local police departments to implement BWC programs. The U.S. Department of Justice (2015) initiated a $20 million grant program to support BWC adoptions by interested law enforcement agencies. The program remains active today and has made 421 grant awards totaling $82 million through FY 2019 (CNA, 2020).

The available program evaluation evidence suggests police officers and citizens alike support BWC use, but BWCs have not generated consistent effects on a range of police and citizen behaviors (Lum et al., 2019). For instance, in terms of the civility of police–citizen encounters, existing program evaluations suggest that BWCs do reduce citizen complaints against police officers (e.g., see Ariel et al., 2015; Jennings et al., 2015), but their effects on police use of force are unclear (e.g., see Braga et al., 2018; White et al., 2018). It is notable that the May 25, 2020 death of George Floyd involved Minneapolis Police Department officers who were outfitted with BWCs but still used force that violated department policies, led to their dismissal from the department, and a series of criminal charges including third-degree murder. This event provoked a wave of Black Lives Matter protests across the United States during the 2020 summer that demanded various police reforms, including more lawful policing. Competing police reform bills were also introduced in the U.S. House of Representatives and U.S. Senate, both of which include provisions to expand BWC use to improve police lawfulness and transparency. Some have argued that BWCs could enhance the constitutionality of police officer encounters with the public (Stanley, 2015). A recent systematic review of 70 empirical studies, however, concluded that little is known about the impact of BWCs on the constitutionality of police officer actions (Lum et al., 2019).

The 2013 federal court remedial order in the New York City stop, question, and frisk case, *Floyd, et al. v. City of New York, et al.*, noted the potential benefits of outfitting New York City Police Department (NYPD) officers with BWCs. Those possible benefits included creating objective records of stop-and-frisk encounters, encouraging lawful and respectful police–citizen

interactions, alleviating mistrust between the NYPD and the public, and offering a way to help determine the validity of accusations of police misconduct. The court order directed the NYPD to work with the court-appointed independent monitor to conduct a one-year pilot program and randomized experimental evaluation to determine whether the BWCs yielded the desired impacts, and whether the program should be expanded or terminated. The monitor was charged with measuring the effectiveness of BWCs on a range of outcomes measures including the reduction of unconstitutional stops and frisks. The court-ordered BWC pilot program and accompanying evaluation was initiated in April 2017. Soon thereafter, the NYPD started a citywide effort to outfit all uniform patrol officers and officers working selected specialized units with BWCs.

This article presents the results of the evaluation of the court-ordered evaluation of the NYPD BWC pilot program. Drawing on the findings of a recently completed Campbell review of the impacts of BWCs on a range of outcomes, our evaluation is the first to consider how the deployment of BWCs on police officers impacts the lawfulness of their actions. It is also the largest single-site randomized field experiment conducted to date of BWCs and the first to explicitly control for treatment contamination effects by design. We begin by briefly reviewing the existing theoretical literature considering the placement of BWCs on police officers and the available empirical evidence on the effects of BWCs on police–citizen encounter civility, officer work activities, and police lawfulness. We then summarize the changes to NYPD stop, question, and frisk policies and practices resulting from the *Floyd* (2013) settlement and describe the implementation of the BWC pilot program, cluster randomized controlled trial methods, and analytical models. Our analyses suggest that the placement of BWCs on treatment officers decreased the number of citizen complaints but increased the number of stop reports relative to control officers. Furthermore, we find that treatment stop reports were more likely to document unlawful stops, frisks, and searches of younger minority males when compared to control stop reports. As discussed in the concluding section, these findings suggest BWCs may increase officer compliance with NYPD directives to document all citizen stops.

2 | LITERATURE REVIEW

2.1 | Theoretical background

Studies consistently document that citizens support the placement of BWCs on officers to hold them more accountable and enhance community trust (e.g., see Crow et al., 2017; Goodison et al., 2017; Todak et al., 2018). This solid community support exists in a context where video surveillance has become ubiquitous in modern society with the saturation of smartphones, drones, closed-circuit television, and other devices in public places (Evans, 2015; Haggerty et al., 2011). High-quality videos of interactions between the police and the public can be effortlessly and instantly shared with countless viewers. The proliferation of BWCs, coupled with these more general societal changes, have created a "new visibility" of policing as officer activities are routinely recorded on video (Brown, 2016; Goldsmith, 2010). The prominent presence of cameras in public places has developed into a key police accountability system. Some observers suggest that this new visibility creates a reciprocal support system or "synopticon" where the few are watched by many (Doyle, 2011; Mathiesen, 1997). Beyond satisfying community desires to outfit police officers with BWCs, police departments also have a more practical reason for the rapid implementation of this technology. BWC recordings capture the officers' gaze, and these videos can be used to limit

organizational liability by countering the narratives developed by citizens who capture police behavior on smartphones.

Foucault's (1977) competing gazes paradigm is compatible with the use of BWCs as a prophylactic technology to counter the gazes of citizen smartphone videos. Community members who support law enforcement generally suggest this same justification for the deployment of BWCs on officers. For instance, a survey of residents in two Florida counties concluded, "community members who believe the police are doing a good job and treating people fairly are more likely to support BWCs, not as a mechanism to correct bad police behavior, but as a tool to combat negative views of the police that result from the rare bad act caught on cell phone video" (Crow et al., 2017, p. 605). Police departments and community supporters alike recognize the importance of recording encounters that capture events, more or less, from the officers' perspectives that can be used to protect them from unjustified allegations of officer wrongdoing. BWC videos can counter false claims of misconduct or incomplete recordings that do not completely capture earlier events that may have shaped officer decisions to use force. As such, the new visibility provided by BWC videos serves as an accountability mechanism for both officers and citizens during encounters.

Deterrence is often suggested as a main theoretical justification for outfitting police officers with BWCs to improve the civility of police–citizen encounters (see, e.g., Ariel et al., 2015; Braga et al., 2018). Deterrence theory posits that potential offenders decide not to commit crimes when the perceived costs of the criminal act outweigh the perceived benefits of the act (Zimring & Hawkins, 1973). A large body of research suggests that deterrent impacts are determined by whether offenders perceive their risk of apprehension and certainty of punishment are enhanced by specific interventions (see, e.g., Nagin, 2013). BWCs stimulate deterrence through the threat of inappropriate and illegal behaviors captured on video and, in turn, modify officer and citizen perceptions of sanction risks (Ariel et al., 2017). As a result, police officers outfitted with BWCs should be deterred from taking actions that violate the law and departmental policy. Deterrent impacts on antisocial behavior during police–citizen encounters may be generated via the threat of formal punishment by the criminal justice system, as well as via concerns over informal punishment by family, friends, and the general public through shaming on social media if a captured video "goes viral" (Braga et al., 2020).

Public awareness theory provides another theoretical explanation of the potential benefits of placing BWCs on police officers (Farrar & Ariel, 2013; Ariel et al., 2015). When people know they are being watched, they are more apt to compare established social norms with their current behaviors, conform to set rules, and behave in socially desirable ways (Duval & Wicklund, 1972). Research suggests that self-awareness can be stimulated by the presence of mirrors, audiences, video and audio recordings, and other environmental cues (Silvia & Duval, 2001). BWCs may inspire police officers and citizens to consider societal norms and standards during their interactions and adjust their immediate behaviors in socially desirable ways. Indeed, a recent controlled evaluation suggests that BWCs may stimulate officers to treat motorists in a respectful and procedurally just manner during traffic stops (Demir et al., 2020).

Consistent with the key outcome measures in our evaluation of the NYPD BWC program, the available program evaluation evidence on the effects of BWCs on the civility of police–citizen encounters, police work activities, and police lawfulness is briefly reviewed here. It is important to note that some of the variations in the presented program evaluation findings may be a result of divergent local implementation contexts, including the policies and training sessions that guide BWC use, the frequency and rigor of supervisory review of captured videos, police–community relations at the time of adoption, and other factors (White & Malm, 2020).

2.2 | Effects on police–citizen encounter civility

Several randomized controlled trials and quasi-experimental evaluations have estimated the impacts of BWCs on citizen complaints of inappropriate police behavior and misconduct during encounters with officers. Many evaluations report large reductions in citizen complaints when officers are outfitted with BWCs relative to non-BWC wearing officers (e.g., Ariel et al., 2015; Braga et al., 2018; Hedberg et al., 2017; Jennings et al., 2015; Mesa Police Department, 2013), whereas a smaller number do not find any noteworthy reductions (e.g., Grossmith et al., 2015; White et al., 2018; Yokum et al., 2017). The overall empirical evidence to date suggests that BWCs reduce citizen complaints against officers; however, it remains unclear whether these reductions represent bona fide improvements in the civility of police–citizen encounters or changes in citizen reporting behavior prompted by a diminished tendency to file frivolous complaints against officers (Lum et al., 2019).

Program evaluations considering the effects of BWCs on police officer use of force during interactions with citizens is much more mixed (Lum et al., 2019). Several controlled evaluations have reported no differences in various police use-of-force outcomes when BWC officers are compared with control non-BWC officers (Headley et al., 2017; Peterson et al., 2018; White et al., 2018; Yokum et al., 2017). In contrast, there is a noteworthy group of controlled evaluations that do find reductions in police use-of-force outcomes for BWC officers relative to control non-BWC officers (Ariel et al., 2015; Braga et al., 2018; Hedberg et al., 2017; Jennings et al., 2015; Mesa Police Department, 2013). A multisite randomized controlled trial involving eight police departments in England and Wales and 2,122 officers suggests that divergent findings on the effect of BWCs on police use of force may be explained by variation in policies governing cameras and the willingness of officers to comply with established camera activation guidelines (Ariel et al., 2016).

2.3 | Effects on police officer work behaviors and lawfulness

The existing program evaluation evidence is also mixed on the effects of BWCs on police officer work behaviors, such as their inclination to use proactive policing tactics and their propensity to use discretion when resolving crime incidents through arrests (Katz et al., 2014; Lum et al., 2019; Rowe et al., 2018; Wallace et al., 2018). Surveys of police officers suggest that BWCs are viewed as potentially important tools to create video evidence that captures events in police–citizen encounters that support the arrest and prosecution of offenders (Goodall, 2007; ODS Consulting, 2011). Other studies suggest that officers are concerned that they will be unfairly disciplined for not making arrests or issuing citations to civilians when these actions are found to be legally justified after supervisors review videos (Police Executive Research Forum, 2014; Ready & Young, 2015).

The conflicting results of a series of recent controlled studies highlights the unclear effects of the BWC technology of police officer work behaviors. In Boston, a randomized controlled trial found no differences between BWC treatment and non-BWC control police officers in responses to dispatched calls for service, self-initiated calls to report proactive interventions, the number of crime incidents handled, arrest reports made to resolve crime incidents, and street stops completed (Braga et al., 2020). Yet, a randomized experiment in Spokane found that the cameras increased self-initiated calls (Wallace et al., 2018), whereas a randomized experiment in Las Vegas found cameras increased arrests and citations (Braga et al., 2018). A quasi-experimental evaluation in Phoenix found that BWCs increased arrests (Hedberg et al., 2017). When compared with

their control counterparts, BWC officers were no more likely to initiate traffic stops of citizens in a randomized experiment in Milwaukee (Peterson et al., 2018), and no more likely to conduct stop-and-frisks in a cluster randomized trial in London (Grossmith et al., 2015). In Mesa, Arizona, however, a controlled evaluation reported that BWC officers were more likely to initiate encounters with citizens and give them citations but less likely to conduct stop-and-frisks and make arrests relative to non-BWC comparison officers (Ready & Young, 2015).

Community concerns over police accountability, such as the lawfulness of police actions during encounters with citizens, was a key factor motivating the adoption of BWCs (Stanley, 2015; Todak et al., 2018). This particularly salient aspect of changing police lawfulness has been surprisingly absent from BWC program evaluation research (see Lum et al., 2019). Police compliance with the law is an important element of democratic societies (Rawls, 1971; Skogan & Meares, 2004). Law enforcement officers are expected to comply with federal and state laws, local ordinances, and the policies and standards of their departments. Important legal constraints on policing include the U.S. Constitution's Fourth Amendment offering citizens protections against illegal searches and seizures, and the Equal Protection Clause of the Fourteenth Amendment protecting citizens against policing actions that discriminate against individuals on the basis of race, religion, country of origin, and other characteristics (Cohen & Kaplan, 1982).

The Fourth Amendment provides the legal basis for civilian stops, arrests, frisks, searches, and use of force by police officers (Sklansky, 1997). For instance, stops are brief detentions of citizens that require officers to have "reasonable suspicion" that a person is currently involved in, has just committed, or is about to commit a criminal offense; frisks, involving pat-downs above the clothes to determine whether a person is carrying a weapon, requires officers to reasonably suspect that an individual is armed and dangerous (see *Terry v. Ohio*, 392 U.S. 1, 1968). In practice, officers need to satisfy a low burden of proof to meet the reasonable suspicion requirement to conduct a stop and frisk (e.g., see White & Fradella, 2016). Some legal scholars and criminologists argue that the reasonable suspicion standard invites racial disparities as explicit and implicit biases can cause minority group members to be stopped and frisked by simply living or working in a high crime area (Capers, 2011; Jones-Brown et al., 2013; Richardson & Goff, 2012), even if the area is not high crime in any measurable sense (Grunwald & Fagan, 2019).

The U.S. Supreme Court has ruled that law enforcement officers must have "probable cause" or adequate reason, which is a more stringent requirement than reasonable suspicion, to believe that a person has committed a crime and make a lawful arrest (e.g., see *Draper v. United States*, 1959). Since these actions are more intrusive than pat-down frisks of individuals, searches also require probable cause that an officer believes a crime suspect or evidence of a crime will be found in the location searched (Cohen & Kaplan, 1982). Searches sometimes require the police to acquire a warrant from a neutral magistrate to ensure that probable cause has been established. Extensive case law exists to determine whether police searches and seizures, including use of force, meets reasonableness standards and balances the rights of the individual against government interests (e.g., *Mapp v. Ohio*, 1961; *Graham v. Connor*, 1989).

2.4 | Stop reports and BWCs in New York City

Former New York City Mayor Michael Bloomberg (2013) and former NYPD Commissioner Raymond Kelly (2015) claimed that increased use of stop, question, and frisk practices (now known simply as "stop reports") was the primary driver of a citywide 37 percent reduction in violent and property index crimes between 2000 and 2010. The number of stop reports increased

dramatically from 97,296 in 2002 to a peak of 685,724 in 2011. Empirical research on the relationship between increased stop reports and subsequent crime reductions in New York City is mixed (see, e.g., Rosenfeld & Fornango, 2014; Smith & Purtell, 2008; Weisburd et al., 2014) with the more rigorous studies suggesting small crime reductions associated with increased stops of citizens (MacDonald et al., 2016; Weisburd et al., 2016). Nevertheless, the NYPD's increased use of stops was strongly criticized for generating unlawful stops that violated Fourth Amendment protections against illegal searches and seizures, as well as for producing racial disparities in who was stopped (Fagan & Davies, 2000), with some arguing that Black and Hispanic residents were being explicitly targeted by the practice in violation the Equal Protection Clause of the Fourteenth Amendment (Jones-Brown et al., 2013).

The Center for Constitutional Rights filed a federal class action lawsuit, known as *Floyd, et al. v. City of New York, et al.* (2013), that alleged the NYPD's use of stop, question, and frisk was unconstitutional as the plaintiffs noted that a substantial share of justifications for stops did not meet reasonable suspicion standards and that unwarranted racial disparities were being generated by NYPD stop activities. In 2013, the federal judge hearing the lawsuit ruled that the NYPD was liable for a pattern and practice of unconstitutional stops of citizens that violated the Fourth and Fourteenth Amendments. To ensure that officer stops of citizens conformed with federal and New York State law,[1] the federal court ordered modifications to NYPD policies, training, and auditing; the handling of civilian complaints and officer discipline; and the measurement and evaluation of organizational performance objectives. The court also mandated that the NYPD establish and evaluate a pilot BWC program. An independent monitor was appointed by the federal court to oversee the execution of the settlement reforms.

The number of stop reports filed by NYPD officers started to decline between 2011 and 2012 before the settlement was reached. The number of NYPD stop reports, however, plummeted by nearly 98 percent in the years following the 2013 settlement—from 532,911 in 2012 to 11,008 in 2018 (New York Civil Liberties Union, 2021). In 2017, however, federal monitor analyses of computer-aided dispatch and arrest data suggested that NYPD were no longer documenting all stop encounters (Zimroth, 2017). In response, the NYPD made an aggressive department-wide effort to increase the documentation of stop encounters through bi-annual meetings of all commands to address the under-reporting of stops and reinforce the requirement that every stop be documented, quarterly analyses of arrest data and daily analyses of calls for service data to identify officer encounters that may have involved a stop followed by subsequent requests to precinct commanders for supporting documentation. The NYPD BWC policy requires the video recording of all citizen stop encounters.[2]

The court-ordered evaluation used a cluster randomized controlled trial to measure the impact of BWCs on the civility of police–citizen encounters (as measured by civilian complaints and arrest reports listing officer use of force) and policing activity (as measured by crime incident reports, domestic incident reports, arrests, summons issued, and stop reports made by police officers). Consistent with the reforms mandated by the settlement, the cluster randomized controlled trial

[1] New York State law regulates police conduct more rigorously than federal law. For instance, see New York State Criminal Procedure Law section 140.50 (1) and section 140.50 (3); *People v. DeBour* (1976); *People v. Holmes* (1996).

[2] NYPD BWC policy specifies a variety of situations where the BWC must be activated including crime-in-progress assignments, interior patrols of NYCHA buildings, pedestrian stops, vehicle stops, personal interactions that escalate, interactions with emotionally disturbed persons, arrests, and other law enforcement duties. The NYPD policy instructs officers to notify members of the public that an interaction is being recorded as soon as it is "reasonably practical." See https://www1.nyc.gov/assets/nypd/downloads/pdf/public_information/body-worn-cameras-patrol-guide.pdf (accessed July 19, 2020).

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TABLE 1 Comparison of treatment and control precinct characteristics

Variable	Treatment Mean	Control Mean	Standardized Mean Difference	Combined K–S *D*
Population	115,557.2	102,705.1	.143	.250
NYCHA population	4,986.2	4,609.2	.031	.200
Concentrated disadvantage	.247	.409	–.077	.250
Officers	228.5	215.9	.118	.200
Major crimes	1,567.6	1,413.4	.161	.250
Arrests	4,878.1	4,820.9	.016	.300
Arrests with force	63.6	66.9	–.046	.250
911 calls	72,730.4	70,269.5	.064	.250
CCRB	67.4	67.5	–.001	.350

Notes: $N = 40$ (20 treatment precincts, 20 control precincts). Joint F test = .348. $p > F = .946$. The standardized mean differences are Beta coefficients generated by ordinary least-squares regressions of each precinct characteristic on group assignment. Meaningful differences between treatment and control groups would be noted by standardized mean differences in excess of |.20|. The distributions of treatment and control precinct characteristics were also compared using two-sample Kolmogorov–Smirnov (K–S) tests for equality of distribution functions. Values closer to 0 indicate more comparability between distributions. The p value associated with the F test was estimated by using a re-randomization procedure in which we randomly assigned the treatment variable 1,000 times and re-calculated the F statistic each time. The empirical p value was generated by the relative position of the F statistic for the model that used the actual data among the distribution of randomized placebo treatments.

the control precincts and non-third-platoon uniformed patrol officers in the treatment precincts were then eligible to be outfitted with BWCs during the larger deployment of cameras.

3.1.2 | Assessment of balance in treatment and control groups

Treatment and control precincts were compared using Cohen's *d* standardized mean difference metrics (Cohen, 1988) and two-sample Kolmogorov–Smirnov (K–S) tests for equality of cumulative distribution functions to determine whether the groups were substantively different from each other based on selected police, crime, and neighborhood characteristics for 2015–2017 (table 1). An F statistic generated by a linear regression of a dummy variable indicator for treatment status on a vector of pretreatment controls was used as an omnibus test of covariate balance.[5] The results of these comparisons showed that the treatment and control precincts were alike, with all contrasts producing small mean differences and similar joint distributions.[6] This suggests that the matching and randomization procedure generated balanced treatment and control clusters. Although conditions varied within precincts, the balanced clusters helped to ensure that the

[5] Since the number of clusters is small, the asymptotic critical value of our omnibus F statistic may provide a poor approximation of the true sampling distribution. As such, we estimated its p value by using a re-randomization procedure in which we randomly assigned the treatment variable and re-estimated the F statistics 1,000 times. The empirical p value was generated by the relative position of the F statistic for the model that used the actual data amongst the distribution of placebo randomizations.

[6] We follow convention in referring to small mean differences as those that are less than .20 standard deviations. Although randomization by design should mean that treatment and control groups are on average comparable, there is always the chance that some differences will emerge. Randomization does not ensure perfect comparability in a single experiment. What matters is how different the groups are on preexisting differences. As such, statisticians recommend using a comparison of average differences, like a standardized effect size, rather than a test statistic and p value (Imai et al., 2008).

with the yearly number of stop reports made in those precincts.[3] As such, all 77 NYPD precincts were ranked according to the 2012–2015 mean yearly counts of CCRB complaints. The top-ranked 40 precincts were then matched into 20 pairs based on simple, but deliberate, matching exercises to ensure that any peculiarities found in one group would most likely occur in the other as well (Blalock, 1979; Rossi et al., 2006). Research also shows that matching (clusters) in community-level trials prior to random assignment provides efficiency gains by shrinking the a priori differences between the treatment and control groups (Freedman et al., 1990).

Precincts were matched into pairs within boroughs by first comparing mean yearly CCRB counts to ensure that treatment and control groups would be balanced on this key outcome measure. Other relevant variables were then considered. These variables included 2012–2015 mean yearly counts of arrests, uses of force, major reported crimes, sworn officers, and 2014–2015 mean 911 calls for service counts, 2015 New York City Housing Authority resident population data, and an overall concentrated disadvantage index for the neighborhoods that comprised the precincts, based on census block data from the 2015 U.S. Census Bureau's American Community Survey (see, e.g., Sampson et al., 1997). This information was supplemented by information regarding neighborhood characteristics and dynamics that might not appear in official data. With the support of the NYPD, the lead author visited all 77 precincts to develop qualitative information relevant to the matching. The matching process yielded 20 similar pairs.[4] A randomization algorithm was then used to assign which precinct within each pair would receive the BWCs.

Implementing a BWC program in a large police department that provides services to citizens in a diverse set of communities across a sprawling metropolitan area was a complex process. For the NYPD, it required coordinating the training of line-level officers and supervisors in the policies governing the BWC program and technological operations, ensuring the appropriate information technology resources were available in each precinct to facilitate uploading acquired video from BWCs, and addressing other matters of implementation. To accommodate these needs, the NYPD used a staggered roll-out of the BWCs over seven months beginning in April and ending in November 2017. Despite varying start dates, all treatment precincts used the BWCs for one full year with the experimental intervention period ending in November 2018.

This evaluation was implemented in the context of a larger effort by the NYPD to outfit all uniformed patrol officers and all officers assigned to specific specialized units with BWCs between December 2017 and August 2019. This larger BWC implementation was explicitly designed not to threaten the integrity of the treatment and control groups during the one-year intervention period. The citywide effort to place BWCs on all uniformed NYPD officers commenced with the 37 precincts not included in the cluster randomized experiment. The placement of BWCs on eligible specialized unit officers did not begin until March 2019, after the last matched pair of precincts completed the one-year experimental intervention period (November 2018). As matched pairs completed the one-year intervention period, all uniformed patrol and Anti-Crime Unit officers in

[3] For all NYPD precincts, 2012 precinct CCRB counts and 2012 precinct stop counts were highly correlated (Pearson's $r = .84$, $p < .000$). Moreover, a 2012 study by the CCRB showed that the police precincts with the highest number of civilian complaints against officers had the highest stop rates. See http://www.nydailynews.com/new-york/brooklyn/complaints-cops-mirror-stop-and-frisk-numbers-article-1.1388735 (accessed May 30, 2020).

[4] Precincts were initially grouped based on similar mean yearly CCRB counts. Within the initial groups, the precincts were further parsed into subgroups based on the goal of maximizing balance on the other quantitative measures. Precincts in these subgroups were initially paired based on simple mean comparisons of all quantitative metrics that suggested the most parsimonious match. Qualitative information on physical features, such as the presence of a large park or a robust entertainment district, and neighborhood dynamics were then considered to confirm each matched pair (see Braga & Bond, 2008; Braga et al., 1999).

was explicitly designed to determine whether the presence of BWCs affected the lawfulness of police–citizen encounters through independent reviews of officer justifications for stops and their subsequent frisks and searches of subjects. As will be discussed further below, the 40 precincts included in the court-ordered evaluation were independent of and separate from a larger citywide effort to outfit NYPD officers with BWCs.

3 | METHODOLOGY

3.1 | Cluster randomized controlled trial design

Randomized experimental designs provide the strongest method in observational research to assess the effect of an intervention, such as the presence of BWCs, on outcomes (Campbell & Stanley, 1966; Shadish et al., 2002). This evaluation used cluster randomization, a variation of the classic design in which clusters (groups) of subjects, rather than individual subjects, are randomly allocated to treatment and control conditions (Mosteller & Boruch, 2002; Murray, 1998). BWCs were randomly allocated by precinct to treatment or control groups for a one-year intervention period in 2017. In treatment precincts, BWCs were provided to all uniformed patrol officers working the third platoon (3:00 pm to midnight shift) and plainclothes Anti-Crime Unit officers working a majority of their shift on the third platoon in the treatment precincts. The comparison group in the control precincts was composed of uniformed patrol officers working the third platoon and plainclothes officers in Anti-Crime Units working the majority of their shift on the third platoon.

The cluster randomized controlled trial design helped to guard against treatment "contamination" across individual officers and civilians. A growing body of evaluation evidence finds that officers with BWCs influence the behavior of officers without cameras if they work simultaneously in the same area and interact with the same people (Ariel et al., 2019; Braga et al., 2020). Similarly, the exposure to BWCs through a subset of officers in an area could influence how civilians in that area interact with the police more broadly. Such contamination undermines the ability to detect intervention effects because both treatment and control officers (and civilians) could be modifying their behaviors as a result of the presence of BWCs. The diffusion of treatment into control conditions threatens the internal validity of the experiment by violating the stable unit treatment value assumption (SUTVA) that the effect of a treatment on a given observational unit is independent of the intervention assignments to other observational units (Rubin, 1980). Randomly allocating groups of officers, rather than individual officers, who work in distinct precincts to have BWCs or not limits the contamination problem. NYPD precincts mainly function as smaller independent police departments with strict guidelines in the department-wide patrol guide that ensure precinct border integrity and prohibit officers from crossing precincts except in emergency circumstances.

3.1.1 | Matching precincts and subsequent randomization

The cluster randomized experiment was designed by the NYPD monitor team over the course of 2016. Consistent with the court's remedial order, the experiment needed to evaluate the impacts of BWCs in precincts that had the highest numbers of police–citizen encounters as measured by NYPD stop reports. As described earlier, stop reports no longer seemed to be a reliable measure of police–citizen encounters post-2012. The yearly number of citizen complaints handled by New York City's Civilian Complaint Review Board (CCRB) in NYPD precincts was highly correlated

with the yearly number of stop reports made in those precincts.[3] As such, all 77 NYPD precincts were ranked according to the 2012–2015 mean yearly counts of CCRB complaints. The top-ranked 40 precincts were then matched into 20 pairs based on simple, but deliberate, matching exercises to ensure that any peculiarities found in one group would most likely occur in the other as well (Blalock, 1979; Rossi et al., 2006). Research also shows that matching (clusters) in community-level trials prior to random assignment provides efficiency gains by shrinking the a priori differences between the treatment and control groups (Freedman et al., 1990).

Precincts were matched into pairs within boroughs by first comparing mean yearly CCRB counts to ensure that treatment and control groups would be balanced on this key outcome measure. Other relevant variables were then considered. These variables included 2012–2015 mean yearly counts of arrests, uses of force, major reported crimes, sworn officers, and 2014–2015 mean 911 calls for service counts, 2015 New York City Housing Authority resident population data, and an overall concentrated disadvantage index for the neighborhoods that comprised the precincts, based on census block data from the 2015 U.S. Census Bureau's American Community Survey (see, e.g., Sampson et al., 1997). This information was supplemented by information regarding neighborhood characteristics and dynamics that might not appear in official data. With the support of the NYPD, the lead author visited all 77 precincts to develop qualitative information relevant to the matching. The matching process yielded 20 similar pairs.[4] A randomization algorithm was then used to assign which precinct within each pair would receive the BWCs.

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[4] Precincts were initially grouped based on similar mean yearly CCRB counts. Within the initial groups, the precincts were further parsed into subgroups based on the goal of maximizing balance on the other quantitative measures. Precincts in these subgroups were initially paired based on simple mean comparisons of all quantitative metrics that suggested the most parsimonious match. Qualitative information on physical features, such as the presence of a large park or a robust entertainment district, and neighborhood dynamics were then considered to confirm each matched pair (see Braga & Bond, 2008; Braga et al., 1999).

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911 calls	72,730.4	70,269.5	.064	.250
CCRB	67.4	67.5	−.001	.350

Notes: $N = 40$ (20 treatment precincts, 20 control precincts). Joint F test = .348. $p > F = .946$. The standardized mean differences are Beta coefficients generated by ordinary least-squares regressions of each precinct characteristic on group assignment. Meaningful differences between treatment and control groups would be noted by standardized mean differences in excess of |.20|. The distributions of treatment and control precinct characteristics were also compared using two-sample Kolmogorov–Smirnov (K–S) tests for equality of distribution functions. Values closer to 0 indicate more comparability between distributions. The p value associated with the F test was estimated by using a re-randomization procedure in which we randomly assigned the treatment variable 1,000 times and re-calculated the F statistic each time. The empirical p value was generated by the relative position of the F statistic for the model that used the actual data among the distribution of randomized placebo treatments.

the control precincts and non-third-platoon uniformed patrol officers in the treatment precincts were then eligible to be outfitted with BWCs during the larger deployment of cameras.

3.1.2 | Assessment of balance in treatment and control groups

Treatment and control precincts were compared using Cohen's *d* standardized mean difference metrics (Cohen, 1988) and two-sample Kolmogorov–Smirnov (K–S) tests for equality of cumulative distribution functions to determine whether the groups were substantively different from each other based on selected police, crime, and neighborhood characteristics for 2015–2017 (table 1). An F statistic generated by a linear regression of a dummy variable indicator for treatment status on a vector of pretreatment controls was used as an omnibus test of covariate balance.[5] The results of these comparisons showed that the treatment and control precincts were alike, with all contrasts producing small mean differences and similar joint distributions.[6] This suggests that the matching and randomization procedure generated balanced treatment and control clusters. Although conditions varied within precincts, the balanced clusters helped to ensure that the

[5] Since the number of clusters is small, the asymptotic critical value of our omnibus F statistic may provide a poor approximation of the true sampling distribution. As such, we estimated its p value by using a re-randomization procedure in which we randomly assigned the treatment variable and re-estimated the F statistics 1,000 times. The empirical p value was generated by the relative position of the F statistic for the model that used the actual data amongst the distribution of placebo randomizations.

[6] We follow convention in referring to small mean differences as those that are less than .20 standard deviations. Although randomization by design should mean that treatment and control groups are on average comparable, there is always the chance that some differences will emerge. Randomization does not ensure perfect comparability in a single experiment. What matters is how different the groups are on preexisting differences. As such, statisticians recommend using a comparison of average differences, like a standardized effect size, rather than a test statistic and p value (Imai et al., 2008).

treatment officers and control officers worked in broadly similar neighborhood, crime, and policing contexts.

The NYPD maintained records of the monthly rosters of all third platoon patrol officers and Anti-Crime Unit officers in the 20 pairs of treatment and control precincts during their respective one-year intervention periods. At the end of the intervention period, these rosters were provided to the monitor team. It is important to note here that NYPD precincts experience natural shifts over time in the stock and flow of officers into and out of third platoon and Anti-Crime Unit assignments for a variety of reasons, including changing to other work shifts within precincts, earning promotions, moving to other precincts, going on leave for injury or health reasons, retiring from service, and being terminated. The NYPD provided data on $N = 3{,}889$ officers who worked uniformed third platoon (3,495; 89.9 percent) or applicable plainclothes Anti-Crime (394; 10.1 percent) assignments and appeared on the monthly precinct rosters during the one-year intervention period in 20 matched precincts. The treatment group comprised $N = 1{,}991$ officers (51.2 percent), and the control group comprised $N = 1{,}898$ (48.8 percent) officers.

Attrition represents a threat to the internal validity of randomized experiments if it occurs differentially between the treatment and control groups (Shadish et al., 2002). Fortunately, there were nearly identical attrition rates for the treatment and control groups, and no systematic differences in the individual characteristics of officers in the treatment and control groups who were not observed for the full 12-month intervention period.[7] Officers in the treatment and control groups remained in these assignments, on average, for more than 8 months. Nearly 72 percent of the officers in each group were observed for at least six months, and some 46 percent of the officers in each group were observed on NYPD precinct rosters for the full 12-month intervention period. The NYPD provided termination dates for the officers but did not note the reason for termination (e.g., retirement, fired). Only 8 percent of officers who were not observed for the full 12 months were terminated (167 out of 2,092; 84 control and 83 treatment). Other officers who were not observed for the full 12 months were most likely to have received a new assignment, went off active duty for an injury, or some other reason. NYPD did not provide data on work assignment changes for this small group of officers.

As anticipated, the precinct roster data suggested that there was little contamination between treatment and control conditions. Only 18 officers (0.5 percent of 3,889) appeared in both the treatment group (third platoon or Anti-Crime assignment in treatment precinct) and control group (third platoon or Anti-Crime assignment in control precinct). During the intervention period, the movement observed included 12 officers moving from control to treatment conditions and 6 officers moving from treatment to control conditions.

Comparisons of group characteristics did not reveal any meaningful average differences in officer rank, sex, race/ethnicity, age, and years on the job, for officers included in the treatment and control groups (table 2). Officers in the treatment and control groups also did not substantively differ in their average work activities, arrests that involved use of force, and CCRB complaints during the 12 months prior to their inclusion in the cluster randomized controlled trial. Taken as a whole, these analyses suggest that randomization was achieved as the process generated balanced clusters and units of analysis.

[7] Cohen's d standardized mean difference in observation time between the treatment and control groups was –.0006. A linear probability model was estimated to test whether there were any systematic differences in the characteristics of officers in the treatment and control groups who were not observed for the full 12-month intervention period ended were in the treatment or control group (robust standard errors clustered by command). The F statistic was not statistically significant ($p = .2866$), and none of the individual covariates were statistically significant at the $p < .05$ level.

TABLE 2 Comparison of preintervention outcome measures and characteristics of NYPD officers working the third platoon and anti-crime units in treatment and control precincts

Variable	Treatment	Control	Standardized Mean Difference	Combined K–S *D*
Officer sex				
% Male	84.2%	84.0%	–.003	.002
% Female	15.8%	16.0%		
Officer race/ethnicity				
% White	47.2%	47.0%	.002	.002
% Hispanic	30.7%	28.9%	.020	
% Black	13.2%	14.8%	–.024	
% Asian/other	8.9%	9.3%	–.007	
Officer assignment				
% Third platoon patrol	89.7%	90.1%	.007	.004
% Anti-crime	10.3%	9.9%		
Officer rank				
% Police officer	90.9%	90.3%	–.010	.006
% Sergeant	9.1%	9.7%		
	Mean (SD)	**Mean (SD)**		
Officer age	29.8 (6.6)	29.9 (6.6)	–.002	.011
Officer years on the job	4.3 (4.0)	4.4 (5.2)	–.007	.028
Stop reports	.7 (1.8)	.7 (1.8)	–.003	.006
Arrests	10.4 (11.6)	10.7 (12.4)	–.012	.021
Arrests with force	.1 (.5)	.2 (.5)	–.022	.017
Summons	8.9 (17.1)	8.2 (14.1)	.021	.033
Domestic incident reports	.3 (2.3)	.2 (1.8)	.020	.024
Crime complaint reports	34.4 (29.9)	33.4 (29.7)	.017	.024
CCRB complaints	.3 (.6)	.3 (.6)	–.011	.009

Notes: $N = 3{,}889$ (1,991 treatment officers, 1,898 control officers). Joint F test = .51; $p > F$, degrees of freedom = .915 (14, 39). SD = standard deviation. The standardized mean differences are Beta coefficients generated by ordinary least-squares regressions of each precinct characteristic on group assignment. The preintervention outcome measures are based on 12 month counts. Meaningful differences between treatment and control groups would be noted by standardized mean differences in excess of |.20|. The distributions of treatment and control precinct characteristics were also compared using two-sample Kolmogorov–Smirnov (K–S) tests for equality of distribution functions. Values closer to 0 indicate more comparability between distributions.

3.1.3 | Sampling stop reports to assess police lawfulness outcomes

A stratified random cluster sampling methodology was used to select representative samples of NYPD stop reports made by third platoon officers and Anti-Crime Unit officers during the 12-month intervention periods in the treatment and control precincts. As part of its broader compliance measurement duties, the NYPD monitor team routinely reviewed the lawfulness of more than 1,200 stop reports per year via quarterly acquisition of all stop reports in randomly selected NYPD commands.[8] Each quarter, the NYPD Quality Assurance Division (QAD) sent the

[8] Reviewers were lawyers with constitutional law expertise and former police executives with extensive experience in training officers in the rule of law.

monitor team a list of all audits conducted on all commands' stop reports during the previous quarter. The monitor team then randomly selected commands until reaching at least 300 stop reports to be independently audited by team members (usually about 20 commands generate at least 300 stop reports in a quarter). Beginning in the first quarter of 2017, the monitor team stratified the ongoing random cluster sample to ensure that the first 10 commands selected were from the 40 precincts included in the experiment. The first strata included five experimental precincts randomly selected from these 40 commands, and the accompanying matched precinct within their pair were included to ensure balanced representation of stop reports in treatment and control precincts in subsequent analyses (10 precincts total comprising 5 matched pairs of treatment and control precincts). The second strata included the random selection of nonexperimental commands until the desired 300 stop reports required for citywide compliance monitoring was reached.

The stratified cluster sampling process yielded an audit of 474 stop reports made by treatment and control officers during the one-year intervention period. The random cluster sample of stop reports by precinct allowed the monitor team to evaluate the overall stop reporting process from report preparation, through the command's self-inspection process, to the final audits made by QAD. This facilitated a more global view of the stop reporting and internal NYPD auditing process. A simple random selection of stop reports within precincts was considered by the NYPD and the monitor team but regarded as overly burdensome.

Once the quarterly stop report data from randomly selected experimental precincts were provided to the monitor team, a structured process was followed to assess the lawfulness of the stops, frisks, and searches in the included reports. As part of the court-mandated *Floyd* (2013) reforms, the NYPD provided mandatory training to all officers on the federal and New York State requirements for lawful stops, and proper documentation of those stops. QAD developed an auditing plan to determine the lawfulness of stops, frisks, and searches that was based on the training materials; this auditing plan was subsequently approved by the independent monitor and the federal court. QAD and monitor team reviewers of the sampled stop reports assessed the narratives that described specific officer actions during these encounters relative to the legal standards in the training. The steps below describe the multistage process used:

1. Two monitor team reviewers (Reviewer 1 and Reviewer 2) independently assessed the lawfulness of the stop, frisk, and search described in the narrative provided in each included stop report.
2. Reviewers 1 and 2 shared their independent assessments with each other and discussed their findings. This discussion generated a list of stops where disagreements existed. There were two types of disagreement: a) both monitor team reviewers disagreed with the assessment of lawfulness made by QAD on either the stop, frisk, or search; and b) the monitor team reviewers disagreed with each other on the lawfulness of the police actions described in each report. The two monitor team reviewers then considered each other's assessments and subsequently made any rating changes they deemed appropriate.[9]

[9] There were few disagreements in this initial stage of review. For instance, the Cronbach's Alpha metric assessing agreement between the initial shared assessment by the monitor reviewers and the QAD review was .868 for stop lawfulness ratings, .806 for frisk lawfulness ratings, and .730 for search lawfulness ratings. Alpha varies from zero to one with higher values indicating a greater degree of reliability in measurement (Cronbach, 1951). These statistics suggest good internal consistency in lawfulness ratings within the monitor team and when the monitor ratings were compared with the QAD ratings.

3. The list of both types of disagreements was then sent to Reviewer 3. Reviewer 3 assessed the lawfulness of the stop, frisk, and search of the stop reports under disagreement and then shared those views with Reviewers 1 and 2. All three Reviewers discussed their views and a final list of stop reports with disagreement (either with QAD or among the team) was created.
4. The final list of stop reports with disagreements was sent to the Monitor and Deputy Monitor for their review and assessment of lawfulness.
5. All five monitor team members (Monitor, Deputy Monitor, and Reviewers 1, 2, and 3) then discussed all stop reports with disagreement and made a final decision on the lawfulness of the encounter described in the narrative of each stop report.
6. The final list of disagreements was subsequently sent to the NYPD for review.
7. The monitor team and representative 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 lawfulness in either the stop, frisk, or search.
8. After the monitor team meeting with RMB and QAD, a final assessment was made of the lawfulness of police actions in the included stop reports for that quarter.

3.2 | Analytical approach

3.2.1 | Encounter civility and policing activity outcomes

Given the observed attrition in the treatment and control groups, we used intention-to-treat (ITT) analyses based on the initial random assignment to treatment rather than analyses of the treatment as received. ITT analyses provide fair comparisons between treatment and control groups because it avoids the bias associated with the nonrandom loss of study participants (Hollis & Campbell, 1999). As such, all $N = 1{,}991$ treatment officers and $N = 1{,}898$ control officers were included in our analyses.

Encounter civility and policing activity outcomes were measured for officers in both treatment and control groups during one-year preintervention (prior to the adoption of cameras by treatment officers) and intervention (treatment officers wearing cameras) periods. As table 2 suggests, these outcome measures were distributed in the form of event counts. There are well-documented problems associated with treating event count variables as continuous realizations of a normally distributed data generating process (King, 1989). Rather, Poisson regression is generally used to estimate models of the event counts (Long, 1997). The Poisson regression model assumes that the conditional mean of the outcome is equal to the conditional variance; however, in practice, this assumption is rarely met. As discussed further below, we estimated negative binomial models that included overdispersion parameters (alpha). Postestimation likelihood ratio tests confirmed that the overdispersion parameters were not equal to zero and overdispersion adjustments were required. As such, we used robust standard errors to adjust for overdispersion in our Poisson regressions (Berk & MacDonald, 2008) and report the results of negative binomial regressions in appendix B.

The impact of BWCs on treatment officer outcomes relative to control officer outcomes was estimated via difference-in-differences (DID) methods. The DID estimates the difference in treatment officers' postintervention outcomes at time t compared with their preintervention outcomes, relative to the same difference for the control officers (Card & Krueger, 1994). As such, our Poisson regression model was as follows:

(Equations 1 and 2)

$$\Pr(Y_{it} = y_{it} \mid \lambda_{it}) = \frac{e^{-\lambda_{it}}(\lambda_{it})^{y_{it}}}{y_{it}!}$$

where

$$Log(\lambda_{it}) = \beta_0 + \beta_1 Group_i + \beta_2 Period_t + \beta_3 Group_i \times Period_t$$

In this model, variation in the officer outcome variable (Y_{it}) is a function of a series of predictor variables (λ_{it}). The regressor $Group_i$ is a dummy variable identifying whether an officer was in the treatment group (1) or not (0). The omitted group comprises control officers in the experiment. The regressor $Period_t$ is a dummy variable for whether the month is during the intervention period (1) or during the pre-intervention period (0). The coefficient β_3, conforming to the product of the group and period dummy variables, is the DID estimate of the effect of BWCs on selected officer outcomes for treatment officers relative to control officers. By taking the exponent of estimates, one obtains the expected change in rate (or counts). The primary parameter of interest exp(β_3) is expressed as a ratio of incident rate (IRR) ratios, a form of a DID estimator.

The units of analysis in the DID panel design were officer-observation periods. As described above, outcomes for each officer were observed for one-year preintervention and one-year intervention periods, bringing the total sample included in our statistical models to $N = 3{,}982$ treatment (1,991 treatment officers × 2 observations) and $N = 3{,}796$ control (1,898 control officers × 2 observations) units. Precinct pair fixed effects were included, and standard errors were clustered by officer to address heteroskedasticity and overdispersion. All parameter estimates were expressed as incidence rate ratios (i.e., exponentiated coefficients), or the percentage change in the expected count. Following convention, a two-tailed 5 percent level of significance was selected as the benchmark to reject the null hypothesis of "no difference."

The statistical power of an experimental design represents the probability that a statistical test will reject the null hypothesis when a specific alternative hypothesis is true (Lipsey, 1990). As statistical power increases, the likelihood of failing to reject a true null hypothesis of no difference between treatment and control groups (type II error) decreases. Statistical power estimates range from 0 to 1, with a .80 power level to detect a small effect size (Cohen's $d = .20$) generally recognized as a desirable level of design sensitivity to detect program impacts in experimental research (Cohen, 1988). In cluster randomized controlled trials, statistical power is a function of the number of clusters and the cluster size; the degree to which outcomes are correlated within clusters also needs to be considered (known as the intraclass correlation coefficient, ICC; see Hemming et al., 2017). With some $N = 7{,}778$ observations nested within 20 precinct pairs, our cluster randomized controlled trial had more than adequate statistical power to detect small group differences in preintervention and intervention outcomes. For instance, our design had a statistical power of .89 (alpha = .05, ICC = .019) to detect small differences in stop reports and statistical power of .92 (alpha = .05. ICC = .015) to detect small differences in CCRB complaints.[10]

[10] All power calculations were conducted with the design effect taken into consideration. The effective sample size formula considered the number of observations per unit (m), the number of clusters (k), and the intra-class correlation (ICC) for each cluster. Effective sample size = (n) / (1 + (m − 1) + (k − m) × r). Where n is the number of observations for each cluster, m is the number of observations per officer ($m = 2$), k is the number clusters (pairs) ($k = 20$), and r is the (ICC) within each cluster. When $r = 0$, the effective sample size becomes the number of observations for each cluster. When $r = 1$ the effective sample size becomes the number of clusters.

3.2.2 | Police lawfulness outcomes

The random selection of stop reports from pairs of experimental precincts started with the commencement of the cluster randomized controlled trial in April 2017. Stop reports sampled in experimental precincts prior to April 2017 were selected for review by the monitor team with a different sampling methodology. As such, DID analyses were not possible. Statistical analyses of police lawfulness outcomes focused on the differences in stop reports in the treatment precincts relative to control precincts during the intervention period for each pair. The monitor team assessments of lawfulness of stops, frisks, and searches served as the key outcome variables. The NYPD provided the results of its internal QAD assessments of the lawfulness of these police actions, which were also analyzed as a parallel set of outcomes.

The sampling frame for stop reports was not designed to follow the cluster randomized controlled trial design. As such, these analyses are exploratory. Since there is a modest number of cases ($N_1 = 185$, $N_2 = 289$), the statistical power to detect an effect in our stop legality analysis is somewhat diminished relative to our larger cluster randomized controlled trial. For instance, using monitor team assessments of the lawfulness of the stop as an outcome measure, this design has statistical power = .654 to detect a small difference between the treatment and control (d = .20). Statistical power exceeds the desirable .80 level when a slightly larger but still small difference ($d = .27$) between the treatment and control groups is tested. The design had statistical power = .996 to detect a medium difference ($d = .50$) and statistical power = 1.00 to detect a large difference ($d = .80$) between the treatment and control groups.

Cohen's d standardized mean difference statistical tests were used to explore prima facie differences in stop characteristics, officer actions, and lawfulness assessment outcomes in treatment and control groups. Multivariate logistic regressions were then used to estimate the impact of the BWCs on officer actions and lawfulness assessments of those actions controlling for characteristics of the stops (Aldrich & Nelson, 1984). Robust standard errors clustered by precinct pair were used to correct for unmeasured dependence within matched precincts in violation of the assumption of independent normally distributed errors. Parameter estimates were expressed as odds ratios. Once again, the conventional two-tailed 5 percent level of significance was selected as the benchmark to reject the null hypothesis of "no difference."

4 | RESULTS

Table 3 presents the DID estimator results of the Poisson regression models comparing change in counts of civility of police–citizen encounters and policing activities outcomes for treatment officers relative to that for control officers. The BWC intervention was not associated with any statistically significant changes in the number of arrests, arrests with force, summons, domestic incident reports, and citizen crime complaint reports. The BWC intervention, however, was associated with a statistically significant 38.8 percent increase in the count of stop reports submitted by NYPD officers and a statistically significant 21.1 percent reduction in the CCRB complaints made against police officers ($p < .05$ for both outcomes).[11]

[11] Statistical analyses that involve multiple comparisons run the risk of reporting "false discoveries" as multiple simultaneous statistical tests are conducted (Miller, 1981). The conservative Bonferroni method was used to correct for family-wise error in our multiple comparisons. With seven tests and a critical value $p = .05$, the Bonferroni correction = .0073 to reject the null hypothesis. Both the p value < .001 for the DID estimator for stops ($t = 4.04$) and the p value = .006 for the DID estimator for CCRB complaints ($t = -2.74$) exceed the Bonferroni correction benchmark.

TABLE 3 Effects of body-worn cameras on selected policing activity and encounter civility outcomes: Matched pair fixed effects and robust standard errors clustered by officers

Variable	Stop Reports IRR (RSE)	Arrests IRR (RSE)	Arrests with Force IRR (RSE)	Summons IRR (RSE)	Domestic Incidents IRR (RSE)	Crime Complaints IRR (RSE)	CCRB Complaints IRR (RSE)
Impact (Group × Intervention)	1.388 (.112)*	1.052 (.034)	1.019 (.134)	.931 (.059)	.630 (.162)	.997 (.029)	.789 (.068)*
Group (1 = Treatment)	.985 (.077)	.969 (.035)	.869 (.091)	1.061 (.061)	1.353 (.319)	1.030 (.028)	.930 (.070)
Period (1 = Intervention)	.950 (.056)	1.053 (.023)*	1.149 (.102)	.661 (.028)*	2.383 (.484)*	1.146 (.024)*	1.349 (.081)*
Constant	.560 (.091)*	8.992 (.852)*	.193 (.042)*	3.927 (.633)*	.053 (.025)*	36.031 (2.217)*	.094 (.020)*
N	7,778	7,778	7,778	7,778	7,778	7,778	7,778
Log pseudolikelihood	−11,455.649	−54,898.736	−3,596.593	−58,394.508	−10,158.248	−123,967.490	−5,374.887
Wald X^2 (df = 22)	210.140*	125.640*	68.910*	649.160*	173.110*	238.380*	172.020*

Notes: IRR = incidence rate ratio. RSE = robust standard error. Robust standard errors clustered by officers. Fixed effects for matched pairs included but not shown.
* $p < .05$.

Statistically significant period effects shown in table 3 were caused by secular changes in crime trends during the third platoon shifts in the 40 treatment and control precincts and by the attrition of officers to new assignments that remained in our ITT analyses. The two largest period effects in table 3 were produced by assignment changes. During the intervention period, 29 (16 control and 13 treatment) NYPD domestic violence prevention officers generated large numbers of domestic incident reports. When these newly minted domestic violence prevention officers are excluded from the main effects models, the statistically significant period effect for domestic incident reports disappeared (IRR = 1.048, RSE = .147, $p = .739$) and the DID estimator for that outcome remained a null effect (IRR = .768, RSE = .305, $p = .508$). Similarly, 254 officers (134 control and 120 treatment) transferred into administrative assignments that were not likely to generate summons during the intervention period. When these administrative officers are excluded from the main effects models, the statistically significant period effect for summons diminished (IRR = .940, RSE = .046, $p = .214$) and the DID estimator for that outcome remained a null finding (IRR = .959, RSE = .039, $p = .317$). In summary, period effects exist in both treatment and control groups and do not explain the observed differences between BWC and non-BWC officers.

The robustness of these findings was further assessed by using several alternative model specifications. First, the DID panel Poisson regression model was reestimated with robust standard errors clustered by precinct pair (see appendix A).[12] Second, we estimated a DID panel negative binomial regression model with standard errors clustered by precinct pair, a hierarchical Poisson regression with level precinct matching covariates included and standard errors clustered by precinct pair, and a two-stage, least-squares instrumental variables regression to estimate the effect of the BWC treatment on the treated (TOT). In the two-stage least square estimate, the treatment assignment was used as an instrument for treatment received (addressing some of the limits of the ITT design described above) and standard errors were bootstrapped (1,000 repetitions) (Angrist & Imbens, 1995) (see appendix B).[13] Third, ordinary least-squares regressions were used to calculate Cohen's *d* standardized mean differences for treatment officer outcomes relative to control officer outcomes during the intervention period, with associated *t* tests and *p* values estimated using robust standard errors clustered by precinct (see appendix C). Fourth, we reestimated all models using randomization inference, also known as a permutation inference, which does not require assumptions to be made about error distributions (see appendix D). The evaluation findings did not change with these alternative specifications: The placement of BWCs generated a statistically significant increase in stop reports submitted by NYPD officers and a statistically significant reduction in CCRB complaints against NYPD officers. Null findings were reported for the other outcome measures using these differing analytical approaches.

Table 4 presents DID Poisson regressions with matched pair fixed effects and robust standard errors clustered by officers that explore the effects of body-worn cameras on selected policing

[12] We also clustered standard errors on groups (matched pairs) using STATA statistical software (Rogers, 1993). This approach is advantageous because it allows the errors to vary differently between clusters rather than assume they are fixed. An alternative approach would be to estimate with the model with a group-level random effect. This alternative approach, however, assumes that the clusters are random draws of the population of precincts in NYC when in fact the study was set up to provide a balanced comparison of the impact of BWCs on outcomes in precincts with the highest levels of interactions between the NYPD and civilians (see Campbell et al., 2007).

[13] To estimate the TOT, we used a method known as the "contamination adjusted intention to treat" (CA ITT) that uses two-stage, least-squares instrumental variables (IV) regression analysis to adjust for the bias created by contamination (see Sussman & Hayward, 2010). In this analysis, the randomized controlled trial is treated as an IV with treatment assignment as the "instrument." The effect of treatment assignment on outcome observed (intention to treat analysis) is adjusted by the percentage of assigned participants who ultimately receive the treatment.

TABLE 4 Effects of body-worn cameras on selected policing activity and encounter civility outcomes by officer assignment: Matched pair fixed effects and robust standard errors clustered by officers

Variable	Stop Reports IRR (RSE)	Arrests IRR (RSE)	Arrests with Force IRR (RSE)	Summons IRR (RSE)	Domestic Incidents IRR (RSE)	Crime Complaints IRR (RSE)	CCRB Complaints IRR (RSE)
Patrol Officers							
Impact (Group × Intervention)	1.393 (.130)*	1.062 (.037)	1.103 (.160)	.945 (.061)	.641 (.170)	1.003 (.031)	.751 (.073)*
Group (1 = Treatment)	.973 (.074)	.968 (.036)	.897 (.091)	1.042 (.062)	1.359 (.322)	1.042 (.030)	.976 (.083)
Period (1 = Intervention)	1.038 (.071)	1.134 (.028)*	1.146 (.109)	.709 (.031)*	2.348 (.486)*	1.174 (.026)*	1.402 (1.040)*
Constant	.424 (.056)*	7.558 (.621)*	.137 (.027)*	3.055 (.437)*	.054 (.027)*	36.563 (2.287)*	.069 (.017)*
N	6,990	6,990	6,990	6,990	6,990	6,990	6,990
Log pseudolikelihood	−8,489.126	−4,171.021	−2,992.946	−49,834.042	−9,339.018	−11,0178.480	−4,377.698
Wald X^2 (df = 22)	189.030*	235.170*	72.050*	619.310*	183.880*	299.550*	152.850*
Anti-Crime Officers							
Impact (Group × Intervention)	1.390 (.212)*	.989 (.069)	.745 (.240)	.902 (.158)	.444 (.461)	.848 (.093)	.715 (.147)
Group (1 = Treatment)	1.065 (.171)	.945 (.067)	1.063 (.275)	1.054 (.198)	3.201 (3.026)	.880 (.079)	.906 (.161)
Period (1 = Intervention)	.763 (.083)*	.711 (.034)*	1.167 (.270)	.693 (.029)*	3.312 (3.262)	.762 (.069)*	.957 (.113)
Constant	1.908 (.633)*	28.296 (7.991)*	.814 (.175)*	15.111 (6.329)*	.024 (.025)*	31.695 (7.267)*	.434 (.146)*
N	788	788	788	788	788	788	788
Log pseudolikelihood	−1,995.764	−6,212.219	−520.489	−6,776.466	−430.622	−7,659.989	−808.649
Wald X^2 (df = 22)	103.480*	188.980*	55.900*	205.870*	570.530*	151.190*	81.790

Notes: IRR = incidence rate ratio. RSE = robust standard error. Robust standard errors clustered by officers. Fixed effects for matched pairs included but not shown.

* $p < .05$.

activity and encounter civility outcomes by officer assignment. Relative to uniformed patrol officers who handle a broad range of duties, plainclothes anti-crime officers were explicitly tasked with carrying out the NYPD's proactive crime control efforts. Since anti-crime officers are much smaller in number (394, 10.1 percent of the 3,889 experimental officers; see table 2), these results should be interpreted with caution as a result of diminished statistical power to find group differences when comparing treatment and control anti-crime officers. Nevertheless, table 4 suggests that BWCs produced similar impacts on patrol officers and anti-crime officers. Controlling for group and period, the BWC intervention was associated with a statistically significant 39.3 percent increase in the number of stop reports submitted by patrol officers and a statistically significant 24.9 percent reduction in the CCRB complaints made against patrol officers ($p < .05$ for both outcomes); the BWC intervention was associated with a statistically significant 38.8 percent increase in the number of stop reports submitted by anti-crime officers ($p < .05$) and a nonsignificant 28.5 percent reduction in the CCRB complaints made against anti-crime officers ($p = .102$).

The stratified random cluster sampling methodology generated an audit of 474 stops that were made by uniformed officers working the third platoon or by plainclothes Anti-Crime Unit officers in the treatment and control precincts during the intervention period. It is noteworthy that the sampling procedure yielded 56.2 percent additional stop reports (+104) for review in the treatment precincts (289 stop reports) relative to the control precincts (185 stop reports). Given that the sampling procedure allowed all stop reports within each pair to have an equal probability of selection, this suggests that third platoon officers and Anti-Crime Unit officers outfitted with BWCs in the treatment filled out more stop reports. This finding is consistent with the results of the policing activity DID regression analysis suggesting a 38.8 percent increase in the number of stop reports made by the treatment officers relative to control officers over the course of the preintervention and intervention observation periods.

Table 5 compares the gender, race/ethnicity, mobilization modality, suspected crime, officer actions, and the lawfulness of those actions for the treatment stop reports relative to the control stop reports. In both groups, stopped citizens tended to be younger minority males who were suspected of a range of crimes. Stopped individuals were more likely to be non-Hispanic Black subjects (61.6 percent vs. 50.8 percent, respectively, $p < .05$) and less likely to be non-Hispanic White subjects (4.8 percent vs. 9.7 percent, respectively, $p < .05$) in treatment stop reports relative to control stop reports.[14] Treatment and control officers in the stop reports were mobilized through radio runs in more than half of the reports, with self-initiated encounters and complaints by victims and crime witnesses representing equal shares of the other mobilization modalities. Although citizens were frisked in similar shares of treatment and control stops (67.1 percent vs. 63.2 percent, respectively), citizens in BWC stops relative to control stops were much less likely to be searched (26.6 percent vs. 38.9 percent, respectively, $p < .05$), arrested (21.1. percent vs. 31.8 percent, respectively, $p < .05$), and summonsed (1.0 percent vs. 3.9 percent, respectively, $p < .05$).

Table 5 also presents the results of the stop lawfulness audits conducted by the monitor team and the NYPD QAD during the intervention period for stop reports completed by treatment officers outfitted with BWCs and stop reports completed by control officers without the BWCs. The monitor team and QAD assessments show a high degree of agreement that stop report narratives completed by BWC officers were more likely to include descriptions of police actions that were not

[14] Multivariate logistic regressions of the BWC treatment on stop characteristics found that sampled stop reports were more likely to involve Black non-Hispanic subjects relative to White non-Hispanic subjects when made by treatment officers outfitted with BWCs relative to control officers holding the other covariates constant. This difference was statistically significant at the less restrictive $p < .10$ level, however (see appendix E).

TABLE 5 Comparison of stop report characteristics by treatment and control officers

Variable	Control		Treatment		Standardized Mean Difference
	N	%	*N*	%	
Male	173	94.0%	259	89.1%	−.071
Female	11	6.0%	29	10.1%	
Missing	1	<0.1%	1	<0.1%	
Black non-Hispanic	94	50.8%	178	61.6%	.106*
White Hispanic	39	21.1%	56	19.4%	−.021
Black Hispanic	30	16.2%	35	12.1%	−.058
White non-Hispanic	18	9.7%	14	4.8%	−.095*
Asian/other/missing	4	2.2%	6	2.1%	.026
Mean age (SD)	172	28.4 (12.3)	274	26.5 (12.0)	−.077
Mobilization					
Radio run	105	56.8%	167	57.8%	.010
Self-initiated	40	21.6%	61	21.1%	−.006
Complainant/witness	40	21.6%	61	21.1%	−.006
Suspected crime					
Violent	66	35.7%	101	35.0%	−.007
Weapon	56	30.3%	92	31.8%	.016
Property	48	26.0%	66	22.8%	−.035
Disorder	6	3.2%	9	3.1%	−.004
Drug	5	2.7%	17	5.9%	.073
Other/unknown	4	2.1%	4	1.4%	−.029
Officer action outcomes					
Monitor – Lawful stop	146	78.9%	193	66.8%	−.131*
QAD – Lawful stop	155	83.8%	214	74.1%	−.114*
Frisked suspect	117	63.2%	194	67.1%	.039
Monitor – Lawful frisk	113	95.8%	164	85.4%	−.126*
QAD – Lawful frisk	110	94.0%	164	85.4%	−.138*
Searched suspect	72	38.9%	77	26.6%	−.129*
Monitor – Lawful search	68	94.4%	66	85.7%	−.145*
QAD – Lawful search	71	98.6%	68	88.3%	−.157*
Not arrested/summonsed	119	64.3%	225	77.9%	.162*
Arrested suspect	59	31.8%	61	21.1%	
Issued summons	7	3.9%	3	1.0%	

Notes: Total $N = 474$ (Control $N = 185$, Treatment $N = 289$).

* $p < .05$.

constitutional relative to stop report narratives completed by control officers. The monitor team ratings only are described here. Overall, the justifications reported for stopping citizens were less likely to be regarded as lawful in treatment stop reports relative to control stop reports (66.8 percent vs. 78.9 percent, respectively, $p < .05$). In stop reports that involved a frisk ($N = 310$), the justifications reported for frisking citizens in BWC officer reports were less likely to be regarded as constitutional when compared with control officer reports (85.4 percent vs. 95.8 percent,

TABLE 6 Multivariate logistic regressions of officer action outcomes on BWC treatment controlling for stop characteristics

Variable	Frisked OR (RSE)	Searched OR (RSE)	Arrested/ Summonsed OR (RSE)	Lawful Stop OR (RSE)	Lawful Frisk/Search OR (RSE)
BWC treatment	1.109 (.366)	.490 (.094)*	.488 (.120)*	.519 (.133)*	.211 (.108)*
Marginal effect:					
Treatment	.725	.254	.221	.757	.682
Control	.691	.397	.372	.856	.813
Male	2.690 (1.065)*	1.346 (.486)	1.537 (.465)	1.583 (.733)	.272 (.364)
Black non-Hispanic	1.139 (.484)	2.969 (1.539)*	1.161 (.421)	.472 (.269)	.389 (.444)
White Hispanic	.923 (.373)	4.221 (2.527)*	1.617 (.580)	.411 (.264)	.296 (.368)
Black Hispanic	1.064 (.477)	2.032 (1.049)	.950 (.331)	.258 (.239)	.219 (.311)
Asian/other	.602 (.721)	2.299 (2.234)	1.381 (1.413)	.689 (1.067)	—
Age	.992 (.011)	1.011 (.008)	1.001 (.005)	1.013 (.011)	1.014 (.022)
Radio run	1.821 (.760)	1.192 (.380)	.859 (.317)	.724 (.286)	.535 (.196)
Self-initiated	1.442 (.733)	.934 (.504)	1.492 (.647)	.726 (.284)	.526 (.312)
Violent	4.364 (2.101)*	.761 (.412)	.434 (.292)	1.876 (1.001)	.963 (1.408)
Property	2.171 (1.293)	.928 (.380)	.401 (.286)	1.398 (.771)	.390 (.560)
Drug	9.621 (8.965)*	.935 (.591)	.178 (.106)*	2.503 (2.323)	1.417 (1.242)
Weapon	36.151 (16.127)*	.752 (.449)	.208 (.171)	1.044 (.532)	.831 (.977)
Constant	.107 (.108)*	.158 (.123)*	1.611 (.162)*	3.851 (.887)*	4.076 (.895)*
Log pseudolikelihood	−228.607	−264.594	−249.506	−248.378	−116.862
Pseudo R^2	.201	.044	.059	.052	.092
N	444	444	442	444	324

Notes: OR = odds ratio. RSE = robust standard error. Robust standard errors were clustered by precinct pair. 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 due to zero cases.

* $p < .05$.

respectively, $p < .05$). Of the smaller number of stops that involved a search ($N = 149$), the justifications reported for searching citizens in BWC officer reports were also less likely to be regarded as lawful when compared with control officer reports (85.7 percent vs. 94.4 percent, respectively, $p < .05$).

Given the observed differences in the measured characteristics of audited stop reports in the treatment compared to control precincts, Table 6 presents the results of the multivariate logistic regressions of the effects of the BWC treatment on officer stop outcomes controlling for stop characteristics. Similar to the bivariate analyses, subjects stopped by treatment officers outfitted with BWCs were not more or less likely to be frisked, but they were much less likely to be searched and arrested/summonsed relative to subjects stopped by control officers without cameras. Controlling for stop characteristics, treatment stop reports were associated with a statistically significant 51.0 percent decrease ($p < .05$) in the odds that a search was conducted relative to the odds that it was not conducted when compared with control stop reports. Holding the other covariates constant,

the predicted marginal effects of the placement of BWCs on officers suggests that 25.4 percent of treatment stops involved a search, whereas 39.7 percent of the control stops involved a search. Furthermore, relative to control stop reports, BWC stop reports were associated with a statistically significant 51.2 percent decrease ($p < .05$) in the odds that a subject was arrested/summonsed relative to the odds that a subject was arrested/summonsed holding the other covariates constant. The predicted marginal effects of the placement of BWCs on officers suggests that 22.1 percent of treatment stops resulted in an arrest/summons issued whereas 37.2 percent of control stops resulted in an arrest/summons issued controlling for the other variables.

Table 6 also presents the results of the multivariate logistic regressions of the effects of the BWC treatment on monitor team assessments of the lawfulness of stops and frisks/searches in the stops controlling for stop characteristics. Controlling for stop characteristics, treatment stop reports were associated with a statistically significant 48.1 percent decrease ($p < .05$) in the odds that the stop was assessed as lawful relative to the odds that it was not conducted in a lawful manner when compared to control stop reports. Holding the other covariates constant, the predicted marginal effects of the placement of BWC on officers suggests that 75.7 percent of treatment stops met the appropriate lawfulness standard whereas 85.6 percent of the control stops met the appropriate lawfulness standard. Relative to control stop reports, BWC stop reports were associated with a statistically significant 78.9 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 frisk/search conducted during a stop was assessed as not constitutional holding the other covariates constant. The predicted marginal effects of the placement of BWCs on officers suggests that 68.2 percent of treatment stops met the appropriate lawfulness standard, whereas 81.3 percent of the control stops met the appropriate lawfulness standard controlling for the other covariates.

5 | DISCUSSION

This study was not explicitly designed to test whether deterrence, self-awareness, or some other theory was responsible for generating the 21 percent reduction in the number of CCRB complaints filed against BWC treatment officers relative to control officers. Our findings are consistent with the logic models underpinning both theories. Our inability to identify the theoretical mechanisms responsible for the observed reductions, however, is a limitation of this study. Future evaluations of BWCs should be explicitly designed to conduct these much-needed tests of theoretical mechanisms. This study is also limited by diminished statistical power to conduct important subgroup analyses, such as whether BWCs reduce the number of complaints per stop or whether certain kinds of complaints were more or less affected by BWCs. 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 officer-involved shootings. In addition, a recent CCRB (2020) report suggested that deployment of BWCs supports civilian oversight by reducing the time needed to investigate complaints, helping in the determination of what happened in the police–civilian encounter, and increasing the share of cases being closed with a disposition of substantiated, unfounded, or exonerated rather than being closed because the facts could not be sufficiently determined.

A key finding of this study was that the BWC treatment officers generated nearly 39 percent more stop reports when compared with the non-BWC control officers over the course of the preintervention and intervention observation periods. The existing program evaluation

literature is highly mixed on whether BWCs have any influence on police proactivity (e.g., see Lum et al., 2019). Some studies speculate that increased enforcement activity associated with BWC deployments may be from enhanced evidence collection offered by capturing events and crime scenes on video or, perhaps, from officer concerns that the technology diminishes their discretion to not make arrests and issue citations (Braga et al., 2018; Goodall, 2007; ODS Consulting, 2011; Police Executive Research Forum, 2014).

In the context of this study, however, these explanations do not seem adequate to explain the observed increase in the number of stop reports filed by BWC officers. As part of the agreed upon reforms in the *Floyd* (2013) settlement, the NYPD implemented a series of changes to move away from the excessive use of stop and frisk activities to control crime. Given that the BWCs were deployed as part of the *Floyd* reforms, it seems unlikely that NYPD officers would be inspired to conduct more stops of citizens on video as part of crime control strategy or view their discretion to not stop citizens as limited as a result of the technology. On the contrary, the increased number of stops reported in the treatment precincts may be an artifact of the surveillance potential of the BWC technology. Officers, aware that the encounter is recorded, may be more likely to document it.

The analyses of the lawfulness of NYPD stops of citizens support the position that the increase in stop reports made may be influenced by a heightened willingness of NYPD officers to file reports of their stops as a result of the associated video documentation of stops created by the BWCs. The stops made by the treatment officers, as well as the frisks and searches in those stops, were much less likely to be judged as lawful by the monitor team and NYPD QAD auditors alike, relative to the stops made by control officers. It seems highly unlikely that increased unlawfulness would be caused by the presence of BWCs that are capable of producing evidence that could be used to punish officers who willingly violate citizen rights. The stops made by BWC treatment officers were also less likely to produce reports that involved full searches, the issuance of summons, or the arrest of suspects when compared with non-BWC control officer stops. The decreased share of stop reports with additional enforcement actions identified implies that BWC officers have increased their documentation of less serious encounters that would not have resulted in official reports in the absence of the technology. Therefore, the presence of the BWCs may be enhancing officer compliance with NYPD policy directives requiring the documentation of citizen stops.

The increased documentation of stops involving less serious encounters with citizens suggests that BWCs do deter officers from committing policy violations. NYPD policy requires that BWCs are activated during all pedestrian stops and that officers document these encounters by filing stop reports. NYPD QAD analyzes calls for service, incident, and arrest data to determine whether it seems likely an officer encounter with a citizen should have generated a stop report. When QAD finds that a stop report may be missing, they contact the precinct command to investigate whether a stop occurred and to ensure that the encounter was properly reported. The availability of BWC video for specific encounters greatly increases the risk that precinct commanders will detect unreported stops and the officer will be disciplined for not submitting the required paperwork. The presence of telltale video increases officer perceptions of the certainty of policy violation detection and the swiftness of punishment given that the video decreases the need for supervisors to locate and interview people involved in the encounter.

This study further suggests that the NYPD still has problems with unconstitutional stops of citizens made by its officers. Indeed, only 68.2 percent of stops made by BWC treatment officers and 81.3 percent of stops made by control officers met Fourth Amendment lawfulness

standards. Moreover, the results of this experiment provide strong evidence that many NYPD officers do not submit formal reports documenting all of their stops of citizens. These undocumented stops undermine the reliability and validity of statistical analyses to identify racially disparate stop report patterns and practices in NYC. Without complete data on stops, the Federal Monitor will not be able to conclude that the NYPD is in substantial compliance with Fourteenth Amendment requirements of the remedial order.

Recent studies suggest that police officers are responsive to managerial directives. For instance, a study of the impact of internal police union memos to Chicago Police Department officers recommending self-imposed monitoring of how they treated suspects resulted in a large decrease in constitutional violation complaints (Rivera & Ba, 2019). Similarly, an interrupted time series analysis of millions of NYPD stop encounters coupled with officer interviews found that 2013 policy changes mandating thorough narrative descriptions of the reasons justifying stops of suspected criminals generated an immediate increase in the rate of stops producing evidence of the suspected crime (Mummolo, 2018). Our study suggests that the efficacy of managerial directives and union memos in modifying officer behaviors can be augmented by BWC deployments and, for the NYPD, produce more complete data on stops.

Supervisory review of BWC footage may also be useful in reducing the percentage of unlawful stops by increasing officer accountability. As suggested by White and Fradella (2016), BWCs provide a unique opportunity for police leaders to routinely monitor officer behavior during stop and frisk activities. Since most stops do not rise to the level requiring mandatory supervisory review (e.g., use of force incident), routine reviews could be conducted at random or targeted at specific officers, shifts, or precincts. In 2020, the NYPD QAD started reviewing half of the BWC videos of stops in all of its commands and specialized units. When unlawful stops are detected, supervisors are required to meet with the involved officers, provide training, and if necessary, recommend discipline to correct recurring problems.

It is obviously concerning that NYPD officers continue to make some unlawful stops of citizens. Our finding that BWC officers document larger numbers of unlawful stops relative to non-BWC officers, however, is fundamentally good news. Put simply, if police departments are not aware that a problem exists, they are not able to take the required steps to remedy the underlying conditions that causes the problem to persist. The deployment of BWCs on police officers not only increases their compliance with police directives to document all stops, but also it provides police departments with an important opportunity to intervene and monitor their progress toward more lawful policing.

5.1 | Conclusion

The National Academies' Committee on Proactive Policing (2018) recently noted that there was insufficient empirical evidence to support the conclusion that proactive policing strategies, such as aggressive stop-and-frisk programs, cause increased Fourth and Fourteenth Amendment violations. They suggested that studies need to determine whether unlawful police behaviors observed under such strategies exist in jurisdictions when proactive policing programs are not in place. At the same time, the Committee noted that ample qualitative and quantitative evidence raise serious concerns over the relationship between aggressive enforcement strategies and privacy, equality, accountability, and transparency (National Academies, 2018). In New York City and elsewhere, BWCs have been nominated as a potential technological solution to the problem

of unlawful policing. In the context of a "new visibility" of policing (Brown, 2016; Goldsmith, 2010), this study finds that the placement of BWCs on officers can increase their compliance with department directives to document stops of citizens. These data can then be used to determine whether officers are adhering to the rule of law in their enforcement efforts. In sum, BWCs could be useful in reducing persistent problems with unlawful citizen stops.

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How to cite this article: Braga AA, Macdonald JM, McCabe J. Body-worn cameras, lawful police stops, and NYPD officer compliance: A cluster randomized controlled trial. *Criminology*. 2021;1–35. https://doi.org/10.1111/1745-9125.12293

Appendix A

TABLE A1 Effects of body-worn cameras on selected policing activity and encounter civility outcomes: Robust standard errors clustered by matched pairs

Variable	Stop Reports IRR (RSE)	Arrests IRR (RSE)	Arrests with Force IRR (RSE)	Summons IRR (RSE)	Domestic Incidents IRR (RSE)	Crime Complaints IRR (RSE)	CCRB Complaints IRR (RSE)
Impact (Group × Intervention)	1.388 (.134)*	1.052 (.071)	1.019 (.163)	.931 (.156)	.631 (.256)	.997 (.045)	.789 (.078)*
Group (1 = Treatment)	.987 (.149)	.974 (.081)	.867 (.103)	1.082 (.157)	1.356 (.600)	1.031 (.045)	.949 (.111)
Period (1 = Intervention)	.951 (.075)	1.053 (.082)*	1.149 (.134)	.661 (.067)*	2.382 (.588)*	1.146 (.038)*	1.349 (.111)*
Constant	.726 (.089)*	10.665 (.787)*	.151 (.016)*	8.176 (.955)*	.233 (.068)*	33.357 (1.763)*	.268 (.028)*
N	7,778	7,778	7,778	7,778	7,778	7,778	7,778
Log pseudolikelihood	−11,789.542	−55,903.757	−3,647.082	−63,264.262	−10,906.364	−12,6345.410	−5,472.451
Wald X^2 ($df = 3$)	22.390*	7.751	3.400	56.710*	50.130*	20.705*	14.880*

Notes: Robust standard errors clustered by matched pairs. IRR = incidence rate ratio. RSE = robust standard error.
* $p < .05$.

Appendix B

TABLE B1 Effects of body-worn cameras on selected policing activity and encounter civility outcomes: summary of results from selected count-based regressions models to test robustness of main effects

Variable	Stop Reports	Arrests	Arrests with Force	Summons	Domestic Incidents	Crime Complaints	CCRB Complaints
Model 1: Negative Binomial with Clustered Robust SEs							
	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)
DID	1.388 (.134)*	1.052 (.071)	1.019 (.163)	.931 (.156)	.630 (.256)	.997 (.054)	.789 (.078)*
Ln (alpha)	1.170 (.033)*	.245 (.018)*	1.206 (.088)*	.990 (.018)*	2.995 (.046)*	.334 (.016)*	.236 (.083)*
Model 2: Hierarchical Poisson Regression with Level 2 Precinct Matching Covariates Included and Clustered Robust SEs							
	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)	IRR (RSE)
DID	1.412 (.172)*	1.061 (.097)	.977 (.161)	1.018 (.172)	.772 (.285)	1.036 (.047)	.737 (.082)*
Model 3: Impact of Treatment on the Treated (TOT) Estimated by Instrumental Variables (2SLS) Regression with Bootstrapped SEs							
	Coef. (BSE)	Coef. (BSE)	Coef. (BSE)	Coef. (BSE)	Coef. (BSE)	Coef. (BSE)	Coef. (BSE)
TOT	.267 (.054)*	.291 (.384)	−.021 (.017)	.045 (.351)	−.084 (.127)	1.122 (1.025)	−.094 (.021)*

Notes: $N = 7{,}778$. DID = differences-in-differences estimator (Group × Intervention). Group (0 = Control, 1 = Treatment) and Period (0 = Preintervention, 1 = Intervention) covariates included but not shown. IRR = incidence rate ratio. RSE = robust standard error. Robust standard errors clustered by matched pairs. TOT = treatment on treated. Coef. = coefficient. BSE = bootstrapped standard error (1,000 repetitions). Ln(alpha) = natural logarithm of the overdispersion parameter (likelihood ratio test used to reject null hypothesis alpha = 0).

* $p < .05$.

Appendix C

TABLE C1 Comparison of intervention outcome measures for NYPD patrol officers working the third Platoon and anti-crime units in treatment and control precincts

Variable	Treatment	Control	Standardized Mean Difference	*t*	$p > \|t\|$
Stops					
Mean	.946	.691	.077	2.55	.015
Standard deviation	1.849	1.417			
Arrests					
Mean	11.506	11.228	.012	.33	.744
Standard deviation	11.736	10.961			
Arrests with force					
Mean	.153	.173	-.020	–.74	.466
Standard deviation	.475	.527			
Summons					
Mean	5.445	5.402	.002	.04	.971
Standard deviation	11.801	9.122			
Domestic incident reports					
Mean	.476	.557	-.011	–.48	.637
Standard deviation	2.961	4.420			
Crime complaint reports					
Mean	39.330	38.257	.017	.51	.610
Standard deviation	30.506	30.326			
CCRB complaints					
Mean	.271	.362	-.071	–2.34	.025
Standard deviation	.602	.675			

Notes: The intervention outcome measures are based on 12 month counts. The standardized mean differences are Beta coefficients generated by ordinary least-squares regressions of each precinct characteristic on group assignment. The *t* tests and *p* values were estimated using robust standard errors clustered by precinct. $N = 3{,}889$ (1,991 treatment officers, 1,898 control officers).

Appendix D

TABLE D1 Effects of body-worn cameras on selected policing activity and encounter civility outcomes: Permutation test by matched pairs

Variables	Stop Reports IRR (SE(*p*))	Arrests IRR (SE(*p*))	Arrests with Force IRR (SE(*p*))	Summons IRR (SE(*p*))	Domestic Incidents IRR (SE(*p*))	Crime Complaints IRR (SE(*p*))	CCRB Complaints IRR (SE(*p*))
Impact (Group × Intervention)	1.388 (.002)*	1.052 (.015)	1.020 (.009)	.931 (.015)	.630 (.012)	.997 (.008)	.789 (.003)*
Permuted *p* value	.004	.345	.892	.402	.191	.924	.014
N	7,778	7,778	7,778	7,778	7,778	7,778	7,778

Notes: IRR = incidence rate ratio. SE(*p*) = standard errors for permutation test *p* values. Permutation calculated by randomly assigning (shuffling) the body-worn camera treatment by officer within precinct pairs 1,000 times and recalculating the estimate for Impact (Group × Intervention) each time. The *p* value is where the actual estimate is located within the reference distribution of 1,000 Impact estimates. Fixed effects for matched pairs are included by not shown.

Appendix E

TABLE E1 Multivariate logistic regression of BWC treatment on stop characteristics

Covariate	Model 1 OR (RSE)	Model 2 OR (RSE)
Black non-Hispanic	3.098 (2.114)†	3.419 (2.320)†
White Hispanic	2.374 (1.301)	2.936 (1.679)†
Black Hispanic	1.744 (1.026)	1.864 (1.111)
Asian/other	4.352 (3.524)	4.930 (4.279)
Male	.589 (.280)	.626 (.292)
Age	.991 (.006)	.992 (.007)
Radio run	1.060 (.439)	1.006 (.394)
Self-initiated	1.126 (.532)	1.188 (.592)
Violent	1.394 (1.071)	1.146 (.746)
Property	1.215 (1.118)	1.074 (.913)
Drug	3.031 (3.99)	2.172 (2.626)
Weapon	1.431 (1.148)	1.037 (.715)
Frisked	—	1.245 (.389)
Searched	—	.674 (.109)*
Arrested/Summonsed	—	.574 (.108)*
Constant	.907 (1.149)	1.102 (1.301)
Log pseudolikelihood	−286.428	−276.886
Pseudo R^2	.032	.058
N	444	442

Notes: OR = odds ratio. RSE = robust standard error. Robust standard errors were clustered by precinct pair. 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.

†$p < .10$, *$p < .05$.

From: Gregory Gelembiuk <gwgelemb@wisc.edu>
Sent: Friday, January 7, 2022 11:42 AM
To: All Alders <allalders@cityofmadison.com>; Rhodes-Conway, Satya V. <SRhodes-Conway@cityofmadison.com>; PD

PSRC <PDPSRC@cityofmadison.com>
Subject: Regarding the proposed body-worn camera resolution

Dear Alders, Mayor, and PSRC Members,

Please see the attached letter, concerning Legislative Item #68625, the bodycam resolution. In the letter, I briefly note the recommendations of the Body-Worn Camera Feasibility Review Committee and the Equal Opportunities Commission, and I provide an update on multiple new research findings concerning police bodycams, predominantly from studies published this past year.

Best regards,

Dr. Gregory Gelembiuk

Dear Alders and PSRC members,

I am writing regarding Legislative Item #68625, the proposed resolution to initiate a North District body-worn camera (BWC) pilot program.

I will start by again noting that the course of action proposed in this resolution completely ignores the recommendations of the Body-Worn Camera Feasibility Review Committee report (while disingenuously citing the report as its justification). The report specifies 10 "strict preconditions" for implementing BWCs (e.g., that any BWC pilot must be a "rigorous, randomized controlled trial", that the District Attorney must first make certain firm commitments, that MPD must first adopt the report's model policy "with, at most, minor modifications that do not alter the essential substance and principles", etc.). Note that the report even italicizes the word "only", for emphasis, in stating "BWCs should *only* be implemented if done so" in the context of the specified reforms. See page 8 of the report. The report's "strict preconditions", which the report emphasizes were passed unanimously by the Committee, have been not fulfilled and are utterly disregarded by the current resolution, which merely uses the report as a fig leaf to proceed with a pre-existing BWC plan. The report also states: "If the City, MPD, and the DA's Office fail to fulfill these preconditions, then the Committee unanimously agrees that BWCs should not be implemented in Madison."

In addition, the current resolution completely disregards the recommendations of the Equal Opportunities Commission. On March 11, 2021, the EOC recommended against proceeding with a Body-Worn Camera pilot program, by a vote of 8 to 2. In addition, the EOC passed the following recommendation unanimously by a roll call vote: "Given that the Equal Opportunities Commission has expressed their opposition to the Body-Worn Camera Pilot Program, it is recommended in the event a Body-Worn Camera Pilot Program is adopted the City utilize the recommendations within the BWCFRC Report." Here are minutes of the meeting.

In the remainder or this letter, I will provide some interesting, updated information on BWCs from studies published this past year (after completion of the Body-Worn Camera Feasibility Review Committee report). I will start by summarizing the remainder in a brief Abstract (with each sentence in the Abstract hyperlinked to the corresponding section of this letter).

Abstract

Research does not support the claim, used to justify the proposed resolution, that BWCs improve community trust. Studies do appear to consistently show that BWCs increase charges filed against civilians, particularly for lower level offenses, which can contribute to over-incarceration. BWCs produce perceptual biases that cause people to perceive police officers as less culpable when they use excessive force, and civilians as more culpable, and a new study found that BWC perspective appears to exacerbate racial bias in people viewing footage of police use-of-force cases. BWC implementation appears to increase police officer burnout and turnover. Finally, a recent econometric study that has been shared with alders, appearing to show benefits in adjudication of complaints due to BWCs, draws conclusions of questionable validity due to methodological flaws. BWCs serve as another illustration of a frequent pattern: technological solutions to human problems often have alarming side effects that aren't fully understood until the technology is in wide use.

False Premise – the Myth that BWCs Improve Community Trust

As justification for the BWC pilot, the resolution text asserts “body-worn cameras can play a role in improving… community trust in the actions of law enforcement and the criminal justice system”. But this is a fiction – inconsistent with overall evidence from existing studies, which consistently appear to show no improvement in community trust with BWC implementation.

I will note here that the largest study to date examining this question was published on November 22 of 2021 – a carefully designed study involving 3,889 officers (both uniformed and plainclothes) in 20 pairs of matched treatment and control precincts (serving from 47,418 to 188,666 residents apiece) spread across all five boroughs of New York City.

Paper: “Do body-worn cameras improve community perceptions of the police? Results from a controlled experimental evaluation”

Excerpt:

> Objectives: Outfitting police officers with body-worn cameras (BWCs) has been suggested to improve police-community relations. This study evaluates whether the deployment of BWCs on NYPD officers impacted resident perceptions of the police.
>
> Methods: A cluster randomized controlled trial design was used to test the influence of BWCs on resident perceptions of the NYPD in treatment precincts relative to control precincts. Dual-frame randomly selected telephone surveys were used to collect pre-intervention and post-intervention resident perception data.
>
> Results: We find no statistically significant differences between BWC treatment and control precincts in general perceptions of the NYPD or the average assessment of police officer behavior among those who have had recent encounters with the NYPD.

Moreover, not only did the study find no significant improvement in perception of NYPD officers, due to BWCs, among residents overall, but also BWCs produced no significant improvements in perception of officers within any of the racial/ethnic subgroups examined (i.e., the paper examined white, Black, and Hispanic subgroups).

I’ll add that the lead author, Anthony Braga, is a BWC proponent who spent a large part of his career embedded with NYPD. He was highly motivated to obtain an affirmative answer to the question. But that’s not what the data showed.

BWCs Create Perceptual Biases Favoring Officers at the Expense of Civilians

Many people erroneously believe that BWC video provides unbiased "objective truth". But in reality, officer mounted bodycams generate strong perceptual distortions/biases. BWCs point outward and aren't filming the officer, but instead function as an extension of the officer; the fisheye lens makes people appear closer than they are; the jiggling of the camera makes events seem more jumbled/threatening; etc. The consequences for interpretation of events become evident as people view and construct meaning out of the visual stimuli in the videos. The research here finds that, when viewing an officer's BWC video (as opposed to onlooker video), people tend to identify with the officer

and their racial biases are exacerbated (so that viewers tend to see excessive use of force by officers against BIPOC individuals as justified).

Paper: "Camera Point-of-View Exacerbates Racial Bias in Viewers of Police Use of Force Videos" (Bailey et al (2021)).

Journal of Communication ISSN 0021-9916

ORIGINAL ARTICLE

Camera Point-of-View Exacerbates Racial Bias in Viewers of Police Use of Force Videos

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The implementation of body-worn cameras (BWC) by policing agencies has received widespread support from many individuals, including citizens and police officers. Despite their increasing prevalence, little is known about how the point-of-view (POV) of these cameras affects perceptions of viewers. In this research, we investigate how POV interacts with skin color of citizens in police use of force videos to affect perceptions of procedural justice. In an experimental study, participants watched eight police use of force videos—half recorded from BWC and half from an onlooker's perspective—in which skin tone of the citizen varied. Results indicate that POV interacts with citizen skin tone such that, compared to the onlooker perspective, the BWC exacerbated viewer racial bias against dark skin tone citizens. Furthermore, identification with the police officer fully mediated this relationship. Results are discussed in relation to media theory and practical implications.

Keywords: Point-of-View, Racial Bias, Police, Use of Force, Procedural Justice, Identification

Excerpt from this paper:

> To put a fine point on the findings presented here: the camera perspective of the BWCs heralded as a panacea for racial inequities in policing exacerbates racial bias such that officers are found to be more justified in their use of force in videos that present very brutal and violent interactions. BWC videos have been presented as a leveling factor that will present the 'facts' of an interaction between citizens and officers. While they may record events and provide important information to be considered, they record events from a particular perspective to which we, as human beings, are naturally bound to respond.... BWC perspective exacerbates racial bias on perceptions of procedural justice. Further, identification with the police officer fully, and identification with the citizen partially, mediate these relationships.

The perceptual biases that occur with video evidence, driven by pre-existing beliefs and camera perspective, are well documented. Here is a particularly clear explication from the book "Unfair: The New Science of Criminal Injustice" by Adam Benforado (2016):

> We operate under the illusion that reality enters our brain through our senses unfiltered. But at any given moment, our race, gender, age, profession, politics, religion, and countless other identity-defining characteristics and affiliations are coloring what we see.
>
> In a powerful demonstration of this phenomenon, a group of law professors decided to test the Supreme Court's conclusion that "no reasonable juror" could watch the footage of the chase that left Victor Harris paralyzed and see Victor's evasion of the police as anything but extremely dangerous and the cause of the eventual crash. The researchers asked a diverse group of 1,350 Americans to watch the video and then offer their impressions.
>
> What they found were clear rifts in perception along ideological, cultural, and other lines concerning the key issues in the case. A less affluent, liberal, highly educated black woman with egalitarian and communitarian views was far more likely than a wealthy, conservative white man supportive of existing social hierarchies and individualism to see Officer Scott and the police as the primary culprits.
>
> If different people with different backgrounds and identities can look at the same events and see very different facts, is it also possible that the same person can look at the same events and see very different facts depending on how information is presented? Over the last few decades, researchers have conducted a number of experiments showing that when we view events as if standing in the shoes of the person experiencing them, we are much more likely to attribute the actor's behavior to forces and constraints in the surrounding environment than when we adopt the perspective of an outside observer, in which case we tend to make attributions that focus on the individual's disposition and character...
>
> Imagine that you are impaneled on a jury and have to decide whether the defendant's confession was voluntary or coerced by the police. As luck would have it, the entire interrogation was recorded, and you are provided with a videotape from one of three cameras in the room: a camera directed at the interrogator, a camera directed at the defendant, or a camera positioned to the side, showing both parties. It would seem reasonable to assume that regardless of the footage you were shown, you would come to the same conclusion, since all three cameras captured the exact same scene. When scientists conducted a number of studies using such a setup, however, they found that perspective made a big difference. By simply

> shifting the point of view from the person being questioned to the interrogator, researchers were able to significantly reduce the number of people who thought the resulting confession was coerced. Watching the interrogator through the eyes of the suspect, it was a lot easier to see—and feel—the menace and pressure. Those who watched the videotape that showed both sides made assessments that fell in between the two conditions.
>
> This bias seems to occur both for minor offenses like shoplifting and for more serious crimes like burglary, rape, and manslaughter. And it's surprisingly sticky: Greater expertise (being a law enforcement officer or a judge), increased accountability, and judicial instructions aimed at encouraging people to be more mindful of perspective bias all appear to be largely ineffective.

Multiple studies have provided clear and consistent evidence of strong perceptual biases arising from BWC video, driven by an intersection of situational biases (biases resulting from the interaction between contextual factors and subconscious cognitive processes) and dispositional biases (biases motivated by culture, beliefs, values, and group commitments of the viewer).

For example, Sommers (2016) found that:

> Participants who saw the encounter with their own eyes [via BWC video] were not significantly less likely to draw on their prior identification with police when making decisions - but they were more certain of their opinions if they had a pre-existing tendency to identify with the police. When we compare the responses of participants given video and nonvideo testimony, we find that those who saw the videos and already identified with the police were more likely to express certitude in their judgment that the officer had acted reasonably or unreasonably. This finding should give pause to advocates who hope that body cameras will make it easier to indict and convict police officers for excessive force. These results suggest that video evidence fails to reduce polarization [i.e., polarized interpretations of what happened in an incident] significantly while simultaneously prompting fact finders who most strongly identify with police to become more unshakable in their judgments.

Turner et al (2019) found that, for police use-of-force incidents, including police shootings, people viewing BWC video, as opposed to dashcam video or written police reports about the incident, gave lower judgements of intentionality (i.e., the extent to which an individual acted with the goal to produce a specific outcome), lower ratings of blame, and lower recommended punishment for officers using high levels of force.

Likewise, Jones et al (2019) concluded that "participants who watched body-camera footage, compared with people who watched surveillance footage of the same encounter, perceived the officer's behavior as being more justified and made more lenient punishment decisions." They further note:

> Our results demonstrate that some body-camera footage—specifically videos that capture an officer using his or her body to apprehend a civilian—can lead to biased perceptions of police encounters that benefit the officer. Our findings suggest that this occurs because: (i) in body-camera footage, the civilian is the more easily visible figure, thus making less salient the officer's role in the encounter; and (ii) the body camera—attached to an officer's uniform—is unable to adequately capture certain use of force movements that are important in determining an officer's intent.

If you want to witness the jumbled, confusing nature of BWC video for yourself, and the biases it can elicit in interpretation, here are some examples. 1. Video created by Seth Stoughton, a BWC expert who is a former police officer himself. BWC video versus stationary camera video of the same interaction. 2. Video (used in the Turner et al (2019) study) of an officer using excessive force, breaking a car window – BWC video versus dashcam video. 3. Video (used in the Turner et al (2019) study) of an officer-involved shooting – BWC video versus dashcam video. For the latter two examples (2 and 3), the BWC video produced lower judgements of blame and punishment than the dashcam video. 4. Police arrest of an individual – BWC video versus surveillance camera video. In BWC video, it appears the individual is clearly resisting arrest – until you watch the surveillance camera video. The officers built their account of the arrest around the BWC video, and the officers' account was taken as valid until the surveillance footage emerged.

The perceptual biases inherent in BWC video underlie a dynamic noted by Henne et al (2021), in their systematic review of BWC research:

> Others have documented how BWC footage, especially when combined with officer accounts of events, often undermines the credibility of citizen narratives about police violence (Brucato, 2015; Russell-Brown, 2016). By buttressing police explanations, BWCs can operate as repressive tools against citizens seeking to make claims against law enforcement officers (Brucato, 2015).

Moreover, as Howard Wasserman (a civil rights and constitutional litigation scholar) noted in 2015:

> [T]he Supreme Court shows no sign of moving from its view that video can be (and often is) so conclusive and unambiguous that the court can determine its meaning and jury consideration is not required. Paradoxically, body cameras may prove worse for civil rights plaintiffs — more constitutional cases will feature video, offering courts more opportunities to misuse video evidence and more opportunities to keep cases away from civil juries.

And empirically, Zamoff (2020) found:

> Law enforcement defendants prevail on summary judgment nearly four out of every five times when they have the benefit of a complete bodycam record of the encounter that gave rise to the lawsuit. This is consistent with the predictions of the commentators who hypothesized that factfinders would often side with the police when confronted with a real-time video taken from the officer's perspective.

BWC Surveillance and Mass Incarceration

Police body-worn cameras have been marketed to the public as a tool for police reform and accountability. But as Michelle Alexander notes, BWCs are outward pointing devices that don't film an officer, but, rather, everyone the officer interacts with. In reality, BWCs predominantly serve as a tool of mass surveillance – to gather evidence for use against residents. One manifestation of this is an increase in the rate at which residents are prosecuted, especially for minor offenses, after police departments acquire BWCs. This appears to be a consistent pattern, seen across essentially all BWC studies that gather data on charging rates. This, of course, hits the most overpoliced communities – poor and BIPOC communities – hardest, since that's where police officers and police interaction tend to be concentrated. Madison already has one of the highest rates of racial disparities in arrests and charges among U.S. cities. A Black resident is ~11 times more likely to be arrested here than a white resident. Despite a lot of empty talk about addressing that disparity, it's actually grown worse over time, and implementation of BWCs would tend to exacerbate this.

Here is a letter I composed a year ago, surveying all BWC studies that had collected data showing their effect on charging rates. It covers seven studies (of variable quality) and provides further context.

In 2021, two additional BWC studies were published that included data on charging rates. Both further corroborate this pattern.

One (Clare et al (2021)) is a study in Perth and Bunbury Australia, with BWCs randomly allocated across shifts. There was a highly significant increase in charges (i.e., rate of charge per computer aided dispatch job) when officers wore BWCs.

The second (Petersen et al (2021)) was a cluster randomized controlled trial in Miami Beach. Again, there was a significant increase in charges (i.e., rate of charge per call for service) when officers wore BWCs, and the ratio of misdemeanor to felony charges was marginally increased.

Essentially all prosecutors use BWC footage to charge residents. Very few use BWC footage to charge officers. Society does not benefit from implementing a technology that will further increase mass incarceration, especially of BIPOC residents.

The results of a recent study suggest that, in the long run, incarceration creates more crime than it prevents (since entry into the criminal justice system, and being burdened with a criminal record, makes people more likely to subsequently commit further crime). Likewise, a review of 29 studies showed arrest and formal processing through the juvenile justice system resulted in greater delinquency, and more crime overall, compared to diversion or doing nothing at all. Similarly, fourteen months after Baltimore State Attorney Marilyn Mosby implemented a policy not to prosecute low level criminal offenses, a report by researchers at John Hopkins Bloomberg School of Public Health, examining outcomes, concluded that declining to prosecute such offenses may avert arrests among individuals with intersecting vulnerabilities without posing a threat to public safety or resulting in increased public complaints.

Someone might argue that in some particular cases, BWC footage might help show a defendant's innocence. But I would urge people to think about the overall societal systemic effects (of increased charging due to BWCs, disproportionately for minor offenses), rather than in terms of an individual anecdote or hypothetical case. In the debate on firearms laws, people too often think in terms of the

hypothetical cases where a good guy with a gun could save people; but at the overall systemic level, increasing availability of guns results in more deaths, leaving society worse off. The same is true with BWCs – with both firearms and surveillance, it's easy to succumb to the erroneous notion that asserting yet more control will allow us to dominate and solve a problem, when it actually leaves everyone worse off.

We don't need to subject yet more people in the criminal justice system and further exacerbate mass incarceration.

BWCs and Officer Turnover

As the BWC Committee report correctly notes, there is evidence that BWC implementation increases police officer burnout:

> A study of 271 officers across five police departments found that officers wearing BWCs reported higher levels of burnout compared to those who did not, and this difference was highly statistically significant. Body-worn cameras can be seen as a form of electronic performance monitoring of officers, and studies on other forms of electronic performance monitoring have consistently shown increased burnout and stress…

See Adams & Mastracci (2018) for more details.

I will add here that I recently came across a complementary study (Schuck & Rabe-Hemp (2018)) that appears to further corroborate and extend this finding. Specifically, Schuck & Rabe-Hemp show that police "agencies that adopted body-worn cameras had higher rates of voluntary turnover than those that did not." Their analysis uses data from 2,239 law enforcement agencies, employing Poisson regression to test hypotheses about factors that increase or decrease officer turnover rates. Schuck & Rabe-Hemp conclude:

> salary is not the only important predictor of voluntary turnover. The number of additional economic incentives and participation in a defined benefits retirement plan were independently related to lower levels of voluntary turnover…. using body-worn cameras on patrol officers was associated with higher rates of voluntary turnover but not dismissals, and collective bargaining was associated with lower rates of voluntary turnover.

The magnitude of the effect on turnover of implementing BWCs appears roughly similar (but opposite in sign) to that of adding an additional economic incentive, having a defined benefits plan, or having collective bargaining. The apparent BWC-related increase in turnover is consistent with greater burnout (due to the stress of what may perceived as constant electronic performance monitoring and/or, potentially, the workload increase that BWCs generate).

In recent years, there has been considerable discussion in Madison government about the problem of relatively high officer turnover in MPD, and the need to implement measures to decrease this. It would be wise to consider that, in deciding whether to proceed with a BWC program.

BWCs and Adjudication of Complaints

A recent paper by econometricians (Çubukçu et al (2021)), claiming to find that BWC implementation increased the rate of sustained police complaints, was shared with the Madison Common Council several months ago. However, there is good reason to question the validity and generalizability of this study's conclusions.

The paper was based on observational data rather than experimental data. In experiments (such as randomized controlled trials), a researcher intervenes to alter specific variables while holding all others constant. This allows clear conclusions to be drawn. Making inferences from observational data is more difficult. To apply statistical and mathematical methods to observational data concerning real-world sociological questions, an econometrician has to make some quite strong, limiting, and unreal assumptions (completeness, homogeneity, stability, measurability, independence, linearity, additivity, strict exogeneity, etc.).

Specifically in this case, Çubukçu et al used a method called differences-in-difference analysis, which is generally recognized as producing results that are less trustworthy than those from randomized controlled trials or from certain other econometric methods (such as regression discontinuity analysis). One reason for the lower perceived credibility of difference-in-differences analysis stems from the

particular strong assumptions it relies on (that often fail to hold in dynamic social situations), as well as the degree to which it is subject to researcher discretion/manipulation.

In the specific case of Çubukçu et al (2021), it appears that the authors made methodological errors and that their analysis relied on fundamental assumptions that were violated. In addition, it appears that the data they used for BWC implementation dates contains errors (which would invalidate the analysis). Please see **Appendix** for a full explication of such problems in this paper.

I'll also note that two of the authors of this study recently published a paper (Sahin & Çubukçu (2021)) analyzing the effect of police dashcam implementation on complaints. This research examined the impact of police dashcams on the total, dismissed, and sustained excessive use-of-force complaints for data from 891 police departments in the USA with more than 100 officers. The primary independent variable was the rate of dashcams per 100 police department employees, and various other factors were controlled for (e.g., whether the department has a specialized Internal Affairs unit, whether it has collective bargaining, etc.). Sahin & Çubukçu (2021) concluded:

> We found a statistically significant relationship between the in-car camera rates and the number of dismissed complaints ($p < 0.05$). As the in-car camera rate increases, so does the number of dismissed complaints. If a department were to increase its in-car camera rate per 100 officers by one point, the number of dismissed complaints would increase 1%.

The conclusion, from extensive national data, that police in-car cameras significantly reduce the rate at which (use-of-force) complaints are sustained seems at odds with the conclusion, from Chicago data, that body cameras significantly increase the rate of complaints being sustained. This potentially raises further questions about the validity, or generalizability to other cities, of the Çubukçu et al (2021) conclusions for Chicago.

Hype and New "Technological Solutions"

Albert Fox Cahn, Esq. (founder and executive director of the Surveillance Technology Oversight Project) notes:

> Bodycam footage...gives only the point of view of the police officers. As a matter of fundamental human psychology, we're primed to align ourselves with the actions of the subject.....Simply put: Showing the officer's perspective makes viewers defer to their narrative.
>
> Cameras that were sold to the public with the promise of increased accountability also end up reinforcing the police narrative. This dynamic is yet another example of a disturbing trend: Technological solutions to human problems often have alarming side effects that aren't fully understood until the technology is in wide use.

Ethan Zuckerman, director of the Center for Civic Media at the Massachusetts Institute of Technology writes:

> The hope that pervasive cameras by themselves would counterbalance the systemic racism that leads to the over-policing of communities of color and the disproportionate use of force against black men was simply a techno-utopian fantasy,

Henne et al (2021) performed a systematic review of the research literature on BWCs and noted how widespread favorable assumptions about BWCs frequently distorted published research, resulting in papers that deviated from accepted statistical standards and that sought to rationalize away unfavorable findings:

> [M]any results interpreted as supporting BWCs as a police reform fail to meet accepted standards of evaluation; that is, they do not adhere to their own guiding methodological principles…. Many studies inconsistently adhered to accepted statistical logics. At times, authors recognised this problem, citing issues with statistical power due to low incident rates (e.g., White et al., 2018). Others make no note of the statistical issues afflicting their experiments...
>
> For instance, a US-based study found no significant difference in the number of complaints received by officers wearing BWCs compared to officers not wearing BWCs during their one-year experiment (Ariel et al., 2015). The authors nonetheless claim that BWCs reduce officer complaints because of a significant drop across all complaints (that is, for both officers wearing and not wearing BWCs) during the experimental period compared to the overall number of complaints received the year prior to BWCs being implemented. Their explanation: BWCs improve law enforcement behaviour even for officers who observe others wearing the cameras. They assert a civilising effect despite not conforming to expectations of RCTs….
>
> White and colleagues (2018) [performed research] on the impact of BWCs on violence during police-citizen encounters. While they acknowledge 'a persistent undercurrent of racial tension' in contemporary policing, their methodology does not account for it (White et al., 2018). Further, they conclude that BWCs have a positive effect on police-citizen encounters since use-of-force incidents and citizen complaints seem to drop after the implementation of BWC technology, even though this finding is not statistically significant.

A systematic review of the BWC research literature by Backman & Hansen Löfstrand (2021) had similar observations:

> In several studies, mixed or negative research findings on the effects of BWCs tend to be explained away by reference to 'implementation failures', and scholars call for further research on police activation of BWCs or even recommend policymakers not be 'disheartened' by the mixed research evidence and police agencies to 'consider a BWC program' (Malm, 2019, p. 121f.; see also Drover & Ariel, 2015; Ariel et al., 2016a; Hedberg et al., 2017; Sousa et al., 2018; Lawrence et al., 2019). Such arguments are examples of a kind of 'modifying work' (Asdal, 2015) where research articles take part in creating a reality in which the assumption that BWC technology will solve policing problems is fortified….
>
> In studies of BWC effects, researchers generally started out by recognizing positive expectations about the effectiveness of BWCs regarding the racialized police crisis in the U.S. In some research publications, positive expectations are generally held to be true and valid….
>
> We have found that the existing research largely investigates the effectiveness of BWCs worn by police officers in the U.S., and build upon a set of dominant policing problem representations drawn on to warrant both BWC research and implementation: the police crisis in the U.S. and the police use of force, lack of oversight and control of police officers, citizen dissatisfaction and

lack of police legitimacy, and police officer resistance towards BWC use. Assumptions underlying all four problem representations is that BWC technology will amend these problems and is legitimate and useful if the public supports it. Taken together, this enhances the representation of BWC technology as a self-evident means of improving community relations and police legitimacy.

It is not surprising that research has been unable to find support for the expected effects of BWCs (with the exception of a decrease in the number of citizen complaints). This has, however, not curbed some researchers' enthusiasm for BWC use, who refer to lack of effects as 'implementation failures' and recommend police agencies to enforce implementation and activation of BWCs (Ariel et al., 2016a; Drover & Ariel, 2015; Hedberg et al., 2017; Lawrence et al., 2019; Malm, 2019, p. 121f.; Sousa et al., 2018).

New "technological solutions" to human problems often come with a great deal of hype, distorting popular perceptions, news reporting, and research agendas. In an infamous example, in 1949, the Portuguese neurologist António Egas Moniz received the Nobel Prize in Medicine for his invention of the prefrontal lobotomy. Now people think "how could that have happened"? But at the time, it made sense – lobotomies appeared to solve the problem of unmanageable patients with psychiatric conditions, initially received only glowing news reporting, and many of the initial medical papers published about lobotomies appeared to demonstrate their benefits. It took considerable time before we began to recognize and adequately acknowledge the downside.

1 is good *for lle-e-e!/?*

GOOD FOR STEERS—Beef grows meatier nowadays . . . for it's a scientific fact that—compared to untreated cattle—beef-steers gain up to 50 pounds extra when protected from horn flies and many other pests with DDT insecticides.

Knox Out FOR THE HOME—helps to make healthier, more comfortable homes . . . protects your family from

GOOD FOR FRUITS—Bigger apples, juicier fruits that are free from unsightly worms . . . all benefits resulting from

Knox Out FOR DAIRIES—Up to 20% more milk . . . more butter . . . more cheese . . . tests prove greater milk pro- . . . en dairy cows are protected . . . m the annoyance of many . . . sects with DDT insecti-

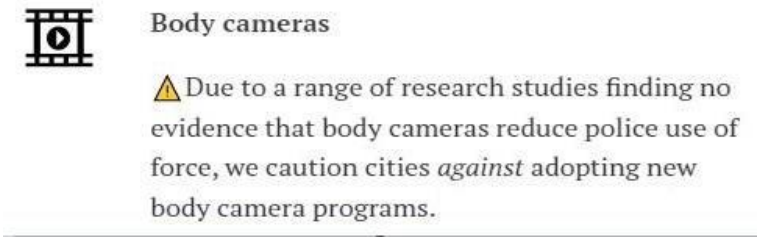
Out Stock

The Stop LAPD Spying Coalition notes:

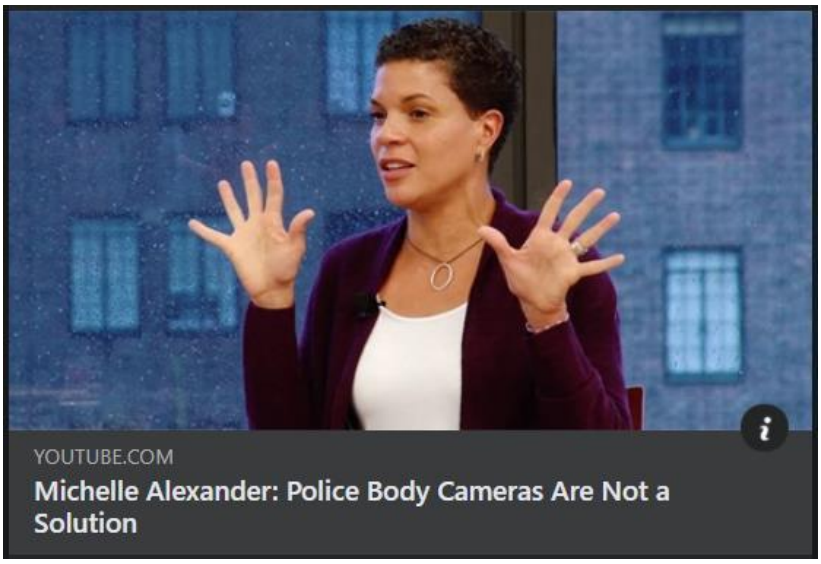
The Movement for Black Lives calls for:

An End to the Mass Surveillance of Black Communities, and the End to the Use of Technologies that Criminalize and Target Our Communities (Including IMSI Catchers, Drones, Body Cameras, and Predictive Policing Software).

Campaign Zero, the BLM organization advocating for research-based policy solutions to end police brutality in America, used to endorse BWCs. It now says:



I'll close with a link to a video of a talk by Michelle Alexander, author of "The New Jim Crow: Mass Incarceration in the Age of Colorblindness".



Sincerely,

Dr. Gregory Gelembiuk

Appendix

This Appendix delineates flaws/issues in Çubukçu et al (2021).

1. The Chicago police district BWC implementation dates used by Çubukçu et al (2021) (see Appendix Table 1 of the paper) contain errors. The implementation date that Çubukçu et al (2021) specify for the 4th District (South Chicago) is a year later than that given in a report by the Chicago Office of the Inspector General (OIG). Moreover, the date given in the OIG report is corroborated by numerous news reports (e.g., see here, here, and here) and a press release by the City of Chicago. The date used by the paper's authors for the 11th District (Harrison) is a month early, when compared to the OIG report, putting it into a different quarter. These errors would undermine the analysis, which requires valid dates for the staggered implementation. In addition, there was partial implementation of BWCs in the 14th District (Shakespeare) starting in early 2015, which is not accounted for at all in the analysis.

2. The analysis in Çubukçu et al (2021) uses a standard difference-in-differences model, though the outcome variable is a binary dummy variable (e.g., sustained versus other outcomes), rather than a continuous variable. This creates major problems for difference-in-differences modeling.

Though the paper fails to provide critical details of the modeling approach used, it appears that they used a Linear Probability Model (LPM) – in essence, acting as though the outcome variable were

continuous. And this would be consistent with the authors' decision to use robust standard errors, which would be necessary given heteroscedasticity due to use of an LPM. However, an LPM would yield biased and inconsistent estimates, and this carries over to estimates of marginal effects. E.g. see Dave Giles (2012): "in almost all circumstances, the LPM yields biased and inconsistent estimates. You didn't know that? Then take a look at the paper by Horrace and Oaxaca (2006), and some previous results given by Amemiya (1977)!". Moreover, that would matter most when the probabilities are close to zero or one, as is true for much of the outcome data in this case. Basically, a decision to use a LPM model is improper here.

Alternatively, it's possible that they used a nonlinear model (probit or logit). But you can't just use a nonlinear model plug-and-play in a difference-in-differences context, given functional form requirements. As Lechner (2010) explains:

> We start with a "natural" nonlinear model with a linear index structure which is transformed by a link function, G(·), to yield the conditional expectation of the potential outcome. ... The common trend assumption relies on differencing out specific terms of the unobservable potential outcome, which does not happen in this nonlinear specification ... Whereas the linear specification requires the group specific differences to be time constant, the nonlinear specification requires them to be absent. Of course, this property of this nonlinear specification removes the attractive feature that DiD allows for some selection on unobservable group and individual specific differences. Thus, we conclude that estimating a DiD model with the standard specification of a nonlinear model would usually lead to an inconsistent estimator if the standard common trend assumption is upheld. In other words, if the standard DiD assumptions hold, this nonlinear model does not exploit them (it will usually violate them). Therefore, estimation based on this model does not identify the causal effect.

So their entire analysis uses a fairly indefensible approach (probably a LPM), that can't be relied on to give valid estimates of causal effects.

3. Çubukçu et al (2021) interpret their results as being due to an improvement in adjudication given BWC evidence (leading to more sustained complaints). However, in drawing this conclusion, they appear to not consider the effects of BWCs on the number of complaints filed. A recent meta-analysis (using data from all BWC trials to date with complaint data) shows a 16.6% decline in the number of formal complaints (this was the only statistically significant BWC-produced change in citizen/police behavior found in the meta-analysis).

The cause of a reduction in complaints when officers wear BWCs has not been fully ascertained, but it is thought to be driven in substantial part by a reduction in frivolous complaints and by police departments dissuading potential complainants from filing formal complaints (after informal review of the BWC video).

Meanwhile, a separate study (Ferrazares (2021)) using a very similar difference-in-differences analysis estimated a 33% drop in the number of use-of-force complaints in Chicago following BWC deployment, and a substantial increase in complaints of illegal search. There thus appeared to have been large shifts in the composition of the pool of complaints. I will also note that among categories of complaints, the rate at which use-of-force complaints are sustained is especially low (the national average is 8%).

The Çubukçu et al (2021) paper estimates that an additional 9.9% of overall complaints were sustained following BWC deployment. But if the number of complaints filed is reduced due in large part to civilians filing fewer frivolous or weak complaints, one would expect a corresponding increase in the rate at which the remaining complaints are sustained (and a drop in the not-sustained rate), even without any change in adjudication. A shift in the types of complaints filed, as described by Ferrazares (2021), could also have this effect. In other words, even if the Çubukçu et al (2021) estimate (of a 9.9% increase in complaints allocated to the sustain category) were correct, an increase of this magnitude could readily be explained by a change in number or composition of complaint filings.

4. Difference-in-differences analysis with staggered treatment timing (as in the Çubukçu et al (2021) study) produces biased estimates of the average treatment effect when there is treatment effect heterogeneity across units (in this case, districts) or across time (e.g., see Goodman-Bacon (2020), Baker et al (2021), Sun & Abraham (2020), de Chaisemartin & D'Haultfœuille (2020)). This leads to misleading inferences (e.g., such estimates can even be of the wrong sign, etc.) and is recognized as a major problem in econometrics literature.

Çubukçu et al (2021) allow dynamic treatment effects (that change over time) in their second model, in part addressing this issue. But their specification requires that districts share the same path of treatment effects. The model allows for differences across districts, via the variable φ_d,, but it's time invariant (the same pre and post treatment). Their approach does not eliminate bias in estimates of average treatment effect because of potential heterogeneity of treatment effects across districts. Moreover, such bias would be particularly large when treatment effect heterogeneity is correlated with treatment timing.

That's very possibly the case here. According to news reports, districts with the highest crime levels were prioritized for BWC implementation in the rollout (e.g., a news report describing districts chosen for the initial wave of BWC implementation notes "The cameras will be used in seven of the most violent districts across Chicago, mostly concentrated on the South and West Sides"; additional similar news reports here and here). So the composition of the pool of complaints, demographics, poverty levels, etc. likely differed across districts in a way that correlated with timing of treatment implementation. Moreover, a separate analysis of this same Chicago dataset (Ferrazares (2021)) noted changes in the demographics of complainants following BWC implementation. It seems likely that heterogeneity in treatment effects across districts would have biased the Çubukçu et al (2021) estimates of average treatment effect.

5. Çubukçu et al (2021) use an invalid approach to test for pretrends, in order to justify their parallel trends assumption. They state:

> We formally test the validity of the "parallel trends" assumption by performing an event study analysis that allows the BWCs to have an impact on the outcomes in the periods prior to deployment. This analysis involves estimating an augmented version of equation (2), in which both the lead and lagged values of the BWC indicator are included in the model. If the estimates on the lead (placebo) indicators are meaningful in the statistical sense, then we would worry that this critical assumption fails and that any effect identified in equations (1) and (2) are spuriously driven by existing differentials in trends.

They are testing for nonzero coefficients in the pretreatment leads. But as Sun & Abraham (2020) show:

> Invalidity of pretrend tests based on pre-period coefficients.
>
> Contamination [from treatment effects in other periods] undermines the practice of testing for pretrends using pre-period coefficients. Proposition 3 implies that when effects are not homogenous across cohorts, it is problematic to interpret non-zero estimates for μ_g as evidence for pretrends, where the set g contains some leads $l < 0$. Proposition 4 implies that even with homogeneous treatment effect, if the effects associated with the excluded periods are not zero, then contamination may still occur. Therefore without strong assumptions, pre-period coefficients should not be used to test for pretrends because contamination can lead to estimates that are non-zero in the absence of pretrends or zero in the presence of pre-trends.

See Sun & Abraham (2020) for more details. Also, as they note, "Callaway and Sant'Anna (2020a) provides alternative tests for pretrends that do not suffer from this drawback."

6. Çubukçu et al (2021) correctly acknowledge that "if the racial or ethnic composition of the complainants change over time in a way that is correlated with the deployment of BWCs", it would render their identification strategy invalid. They state that they had performed a regression to check for this possibility (i.e., a change in racial/ethnic composition), and it didn't return a statistically significant estimate. However, such a change is exactly what Ferrazares (2021) found, using a very similar difference-in-differences analysis run on the same Chicago data. Ferrazares notes a large and highly significant reduction in white officer-black civilian use-of-force complaints. If racial/ethnic composition of complainants shifted in this manner in response to treatment, it would violate the assumption of strict exogeneity that the two-way fixed effect difference-in-differences analysis performed by Çubukçu et al (2021) relied on. Note that the race of each complainant and officer were included in their model as covariates. Difference-in-differences analysis does not allow a time-varying covariate that is affected by treatment and that affects the outcome.

7. In their difference-in-differences model, Çubukçu et al (2021) include covariates specifying "characteristics of the incident such as whether a police shooting is involved". Inclusion of such covariates is questionable, as it requires an assumption that BWC implementation doesn't affect these characteristics (otherwise, the strict exogeneity assumption that the analysis relies on would be violated). With the inclusion of the covariate specifying whether the complaint related to a police shooting, one must assume that BWC implementation doesn't influence the number of police shooting-related complaints relative to other types of complaints. But there's no justification given for this assumption, and it may very well be false. I'll note that Ferrazares (2021) concluded that BWC implementation caused shifts in the composition of the complaint pool in Chicago.

8. Recent studies using simulations found an extremely high false positive rate for difference-in-differences analyses of the type used by Çubukçu et al (2021). One can simulate thousands of datasets from a known model (such that one knows "truth"), then perform difference-in-differences analysis and see how often it arrives at the (known) correct conclusion. For context, I'll note that when the null hypothesis is true, a statistical test (e.g., using a standard difference-in-differences two-way fixed effects model) should reject the null only 5 percent of the time, and when the null hypothesis is false, the test should have high power to reject it. Here is an excerpt from a paper (Griffin et al (2021)) examining simulations of differences-in-differences modeling of the effects of state-level policies on opioid mortality:

> In the context of frequentist hypothesis testing, many models yielded high Type I error rates and very low rates of correctly rejecting the null hypothesis (< 10%), raising concerns of spurious conclusions about policy effectiveness….
>
> Type I error rates [rates of incorrectly rejecting a true null hypothesis] were very high for the classic DID two-way fixed effects model (Figure 5a), ranging up to 67%. Cluster SE [standard error] adjustment greatly reduced the Type I error rates for this model when 5 or more states implemented a policy, but they were still 2 to 3 times larger than the traditional target of 5%, ranging from 9% to 17%.... For the two-way fixed effects model (Figure 7a), correct rejection rates [i.e., power to correctly reject a false null hypothesis] were low across all effect sizes, with a maximum value of 27%.

Here is an excerpt from a similar paper (Schell et al (2018)) examining the ability of difference-in-differences analysis to draw correct conclusion regarding the effects of state-level firearms law on firearms mortality:

> Almost all of the models that are commonly used in this field demonstrate poor type 1 error rates when fit to these data. For example, the classic two-way linear fixed-effects model (i.e., standard difference-in-differences model), using population weights and without any adjustment to the SE, have an average type 1 error rate of 0.62 across the six types of simulated laws we considered (three different numbers of states by two different phase-in periods). This is 12 times the rate of false positives that are expected when using an a = 0.05 level of significance. Even using a cluster adjustment, the best adjustment to SEs for this model, the average type 1 error rate is 0.20, still four times higher than the claimed false positive rate. Somewhat surprisingly, the SE adjustments often made the type 1 error worse, although, in some cases, clustering adjustments did reduce these errors.

Such simulations of the effects of staggered implementation of policies/laws across the 50 U.S. states should fairly closely reflect the situation of staggered BWC implementation across 22 Chicago police districts (e.g. the overall number of units is relatively similar, etc.).

9. Empirical claims based on difference-in-differences analyses are generally recognized as less trustworthy than those from randomized controlled trials or regression discontinuity designs. This is in part because difference-in-differences analyses are prone to p-hacking and specification mining, and require making many assumptions that may be of questionable validity in dynamic social situations being analyzed. In general, it is much harder to draw valid conclusions from observational data than from actual experiments (such as randomized controlled trials) in which chosen factor are manipulated while others are held constant. Imbens (2010) notes that "(R)andomized experiments occupy a special place in the hierarchy of evidence, namely at the very top."

"P-hacking," occurs when researchers collect or select data or statistical analyses until nonsignificant results become significant. A recent study (Brodeur et al (2020). "Methods Matter: P-Hacking and Publication Bias in Causal Analysis in Economics") examined p-values in published papers making empirical causal claims, to see if the degree of p-hacking varies by analytical method. They conclude:

> Our paper contributes to a discussion of the trustworthiness of empirical claims made by economics researchers… The primary aim of this study is to investigate the extent of the p-

> hacking and publication bias problems both in aggregate and by method. Our analysis points to significant between-method differences, with papers using IV [instrumental variables] and DID [difference-in-differences] identified as particularly problematic.

They also note:

> First, looking at the whole of the distributions we can see that many (around half) of RCT [randomized controlled trials] and RDD [regression discontinuity design] studies report null results with large p-values as their main estimates, whereas IV and DID studies typically reject the null. Second, DID and IV are more likely to report marginally significant estimates than RCT and RDD.

Basically, it appears that difference-in-differences analyses rarely report null (unexciting) results – whatever treatment is being tested is usually declared to work (in contrast to the reality that treatments often don't give hoped for results). Brodeur et al note that

> [A] potential explanation is that some methods offer researchers different degrees of freedom than others…. For non-experimental methods (like IV [and DID]) there are many stages in the research process when researchers exercise discretion. This is in contrast to RCTs where there are fewer researcher degrees of freedom (and where pre-registration is more likely to be expected).

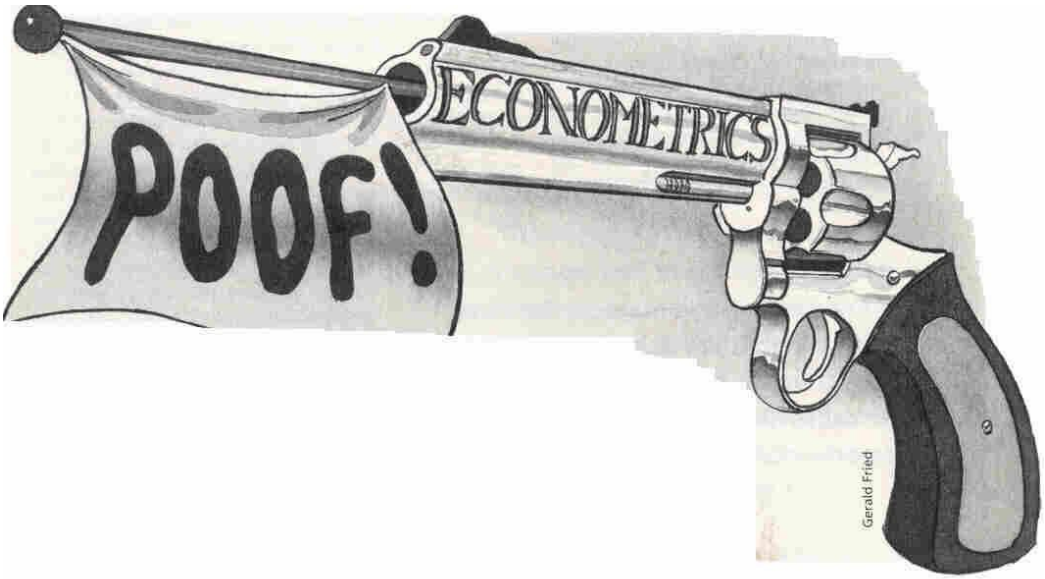
Similarly, Economist Mark Thoma writes about the problem of "specification mining", in which a model can be tinkered with, adding or removing terms, until the analyst obtains the result they want:

> There's a version of this in econometrics, i.e. you know the model is correct, you are just having trouble finding evidence for it. It goes as follows. You are testing a theory you came up with, but the data are uncooperative and say you are wrong. But instead of accepting that, you tell yourself "My theory is right, I just haven't found the right econometric specification yet. I need to add variables, remove variables, take a log, add an interaction, square a term, do a different correction for misspecification, try a different sample period, etc., etc., etc." Then, after finally digging out that one specification of the econometric model that confirms your hypothesis, you declare victory, write it up, and send it off (somehow never mentioning the intense specification mining that produced the result).
>
> Too much econometric work proceeds along these lines. Not quite this blatantly, but that is, in effect, what happens in too many cases. I think it is often best to think of econometric results as the best case the researcher could make for a particular theory rather than a true test of the model.

For example, Çubukçu et al (2021) include in their difference-in-differences model a covariate specifying whether a complaint was related to a police shooting or not. That appears an odd choice. They're not separating out categories of complaints in any principled, systematic way (e.g., use-of-force complaints, improper stop and search complaints, etc.) – just adding one completely arbitrary term for whether it's police shooting-related or not. Including such a term as a covariate seems even odder when you consider that BWC implementation might influence police shootings or filings of complaints, such that addition of the term could violate fundamental assumptions of the difference-in-differences modeling, rendering the entire analysis invalid.

In his paper "Myths of Murder and Multiple Regression", Sociologist Ted Goertzel notes a generalproblem with many such econometric studies examining sociological questions:

> Do you believe that every time a prisoner is executed in the United States, eight future murdersare deterred? Do you believe that a 1% increase in the number of citizens licensed to carry concealed weapons causes a 3.3% decrease in the state's murder rate? Do you believe that 10 to 20% of the decline in crime in the 1990s was caused by an increase in abortions in the 1970s?Or that the murder rate would have increased by 250% since 1974 if the United States had not built so many new prisons?
>
> If you were misled by any of these studies, you may have fallen for a pernicious form of junk science: the use of mathematical models with no demonstrated predictive capability to draw policy conclusions. These studies are superficially impressive. Written by reputable social scientists from prestigious institutions, they often appear in peer reviewed scientific journals. Filled with complex statistical calculations, they give precise numerical "facts" that can be usedas debaters' points in policy arguments. But these "facts" are will o' the wisps. Before the ink isdry on one study, another appears with completely different "facts." Despite their scientific appearance, these models do not meet the fundamental criterion for a useful mathematical model: the ability to make predictions that are better than random chance.



From: Krystle Shore <krystle.shore@gmail.com>
Sent: Friday, January 7, 2022 12:46 PM
To: PD PSRC <PDPSRC@cityofmadison.com>; All Alders <allalders@cityofmadison.com>
Cc: Rhodes-Conway, Satya V. <SRhodes-Conway@cityofmadison.com>
Subject: Resolution to Initiate BWC Pilot Program

Dear Members of the Madison Public Safety Review Committee & Madison Common Council,

Please see my attached letter regarding your upcoming resolution to initiate a BWC pilot program within the Madison Police Department.

Kind regards,
Krystle Shore, Ph.D. Candidate
Department of Sociology and Legal Studies, University of Waterloo
Office: PAS 2078
Alternate email: kshore@uwaterloo.ca
she/her/hers

Krystle Shore, PhD Candidate
University of Waterloo
Waterloo, ON, Canada
kshore@uwaterloo.ca

January 7, 2022

Madison Public Safety Review Committee & Madison Common Council
Madison, WI, United States
PDPSRC@cityofmadison.com | allalders@cityofmadison.com

RE: Resolution to Initiate BWC Pilot Program

Dear Members of the Madison Public Safety Review Committee and Madison Common Council,

My name is Krystle Shore and I am currently a PhD Candidate in the Department of Sociology and Legal Studies at the University of Waterloo in Ontario, Canada. My research focus relates to the use of police surveillance technology; I have considerable experience examining the impact of police body-worn cameras (BWCs) as well as myriad other digital surveillance technologies often used to address social problems. My work is published in academic books and journals, and has been featured in several local and international news media outlets. Recently, I was contacted by a local Madison community organizer and asked to share my thoughts and research with you in hopes that it will inform your upcoming decision regarding the Madison Police Department's implementation of a BWC pilot program. To be clear, there is no empirically sound evidence that demonstrates BWCs are an effective means of reducing police misconduct; further, BWCs represent a harmful and expensive police surveillance tool. I will now elaborate on these points.

In 2017 and again in 2020 I, along with my colleagues Dr. Kathryn Henne and PhD Candidate Jenna Harb, systematically reviewed all academic studies assessing the effect of BWCs on police misconduct. We have reviewed over 14,000 studies to date, and have taken a fine-tooth comb to the claims made in the 21 studies that explicitly use experimental methodologies (e.g., randomized controlled trials—highly regarded as the "gold standard" in evidence production) to evaluate whether BWC's reduce police misconduct. Of note, the majority of these studies focus on U.S.-based BWC pilot programs. Our findings show that there is no clear evidence that BWCs indeed reduce police misconduct, and that the few studies that do make such claims divorce police misconduct from race and racism, and suffer from serious methodological challenges that undermine their results. Here are some excerpts from our published work:

"While evaluative studies of BWCs may appear race-neutral because they seem objective, they are far from it (see also Shore, 2020). They reflect colour-blind practices that either minimise or erase racial inequality from police-citizen interactions."

(Henne, Shore, & Harb, 2021, p. 13)

"Many claims that BWCs reduce police misconduct are, for the most part, methodologically unsubstantiated if following accepted scholarly standards of assessment. They tend to suffer from statistical and logical challenges that undermine their results; that is, they lack statistical significance, fail to maintain strict experimental conditions during large-scale police studies, and use inconsistent definitions of police misconduct."

(Henne, Shore, & Harb, 2020, p.8)

"[BWCs] do not address organizational dynamics and structural inequalities that contribute to police violence, making them largely ineffective."

(Shore & Henne, 2020, p.3)

Not only do BWCs fail to induce a clear reduction in police misconduct, they also carry a capacity to exacerbate existing harms for racialized communities. Research shows that Black and other racialized groups are disproportionately targeted by police surveillance technologies when compared to their white counterparts (see, for example, the 2020 report by the Ontario Human Rights Commission). In addition, the technology is expensive. The millions of dollars required to implement and maintain BWC programs would be better spent investing in community programs that reduce racialized harm:

"Anti-poverty and decarceration programs — including housing, health and education services — are the kinds of reforms experts argue are necessary for meaningful change. Funding community services and resources are especially critical in Black and Indigenous communities, where inequality has continued to widen during the coronavirus pandemic."

(Shore & Henne, 2020, p.5)

BWCs may sound like an attractive solution to police misconduct, but like other narrow behavioural reforms (e.g., "bias training") the implementation of BWCs will not induce meaningful and lasting positive change. The expensive technology did not stop Derek Chauvin from killing George Floyd, and it will not guarantee a reduction in harm for the citizens of Madison, WI. As community leaders, I implore you to vote against empowering your local police service with augmented surveillance capabilities and, instead, to direct your attention and funding toward initiatives that empower your community.

Sincerely,

Krystle Shore, PhD Candidate
Department of Sociology & Legal Studies
University of Waterloo

References

Henne, K., Shore, K., & Harb, J. I. (2021). Body-worn cameras, police violence, and the politics of evidence: A case of ontological gerrymandering. *Critical Social Policy* https://doi.org/10.1177/02610183211033923.

Henne, K., Shore, K., & Harb, J. I. (2020). Police-issued surveillance technologies: A systematic review and analysis of the research. Report prepared for the Justice and Technoscience Lab, Australian National University, Canberra, AUS.

Shore, K. (2020). Questioning assumptions of de-policing and erasures of race: A rejoinder to Ariel and colleagues' study of camera-induced passivity among traffic police in Uruguay. In B. C. Newell (Ed.), *Policing on Camera* (pp. 156-164). London: Routledge.

Shore, K., & Henne, K. (2020). Rather than defunding police, politicians are increasing funding for body- worn cameras. *The Conversation Canada,* August 20.

Ontario Human Rights Commission (2020). A disparate impact: Second interim report on the inquiry into racial profiling and racial discrimination of Black persons by the Toronto Police Service. Retrieved from www.ohrc.on.ca

From: Daniel Levitin <dnlevitin@gmail.com>
Sent: Tuesday, January 11, 2022 10:42 AM
To: PD PSRC <PDPSRC@cityofmadison.com>; All Alders <allalders@cityofmadison.com>
Subject: PSRC Item 13

Good morning.

My name is Daniel Levitin, and I am emailing to express my opposition to the body camera pilot program currently under consideration. In my view, this is quite an expensive program with minimal upside.

Generally speaking, the claimed benefits of body cameras on police have failed to materialize. In particular, the widely-claimed advantage of cutting down on police misconduct cannot be statistically substantiated, even after much effort. Without evidence of efficacy in reducing police misconduct, it would be better to devote the funding for the program to social services, since these are well-documented to reduce crime.

I wish to point out as well, and as many others have, that the strict preconditions set forth by the Feasibility Review Committee. To me, the most crucial of these is point 9, that there be plans in place for rigorous study to determine whether there are negative side effects of the program such as rates of charging and plea bargaining. Also important is points 2 and 3 on perceptual bias and the information that body-worn cameras miss. The resolution creating the program contains no mention of any of these preconditions by name nor any implementation of the preconditions into its text. It is therefore my opinion that, even if body-worn cameras were to be introduced in Madison, which I already oppose, that this resolution introduces them in a particularly negligent fashion.

I hope you will all vote in opposition to the resolution.

Daniel Levitin
225 E. Lakelawn Pl.
Madison, WI, 53703
(District 2)

From: Bonnie Roe <bonnie.roe@gmail.com>
Sent: Wednesday, January 12, 2022 8:38 AM
To: Mayor <Mayor@cityofmadison.com>; Bottari, Mary <MBottari@cityofmadison.com>; All Alders <allalders@cityofmadison.com>; Figueroa Cole, Yannette <district10@cityofmadison.com>; PD PSRC <PDPSRC@cityofmadison.com>
Cc: Barnes, Shon F <SBarnes@cityofmadison.com>
Subject: Wake up call on BWCs

Dear Mayor, Alders, Chair and Members of the PSRC,

Yesterday an officer-involved critical incident took place, where a suspect in an armed robbery investigation jumped off a balcony and allegedly began firing at officers. Multiple officers returned fire on the suspect, who was taken to the hospital for his injuries.

The investigation being led by DCI would be greatly aided had the officers been wearing body-worn cameras. The investigation could be finished faster, therefore shortening the time that five officers are on investigative leave and arriving at a conclusion about what happened.

Please vote to support the resolution coming before you to implement the body-worn camera pilot in the north district. We need to join the 21st century and the cities all around us (including UWPD) and equip our officers with body cameras for the good of the whole community.

Thank you,
Bonnie Roe
District 10

From: Greg Jones <gcjones15@att.net>
Sent: Wednesday, January 12, 2022 9:58 AM
To: All Alders <allalders@cityofmadison.com>
Subject: Statement in support of the Body Worn Camera Pilot

Good morning,

The attached statement is submitted for the upcoming Council meeting on January 18, 2022.

Greg Jones
608-274-3997

Law Enforcement and Leaders of Color Collaboration
Statement in support of Madison Police Body Worn Camera Pilot

The Law Enforcement and Leaders of Color Collaboration (LELCC) was established following an officer-involved shooting in Ferguson, Mo. in 2014, but was crystalized following an officer-involved shooting in Madison WI in 2015. The Collaboration is facilitated by the Dane County NAACP and United Way of Dane County. Since 2014, community leaders and representatives from local law enforcement agencies across Dane County have come together to address issues relating to building trust between law enforcement and communities of color; equity and inclusion in the workplace; and use of force. The Dane County Chiefs of Police Association has been a consistent partner in the Collaboration.

In February 2016, the LELCC created a Special Community/Police Task Force which produced recommendations to change key policing practices relating to the reduction of police use of force. Recommendations put forward included: 1. Incorporating available technology as a tool in documenting police and citizen interactions. 2. Dane County law enforcement agencies should equip patrol cars with dashboard/squad car cameras. 3. Dane County law enforcement agencies should explore outfitting patrol officers with body worn cameras in communities where they are desired, with allowing community-supported policies to govern use. 4. Law Enforcement agencies should conduct random reviews of footage to evaluate officer performance. 5. The development of clear community-supported policies governing the use, activation and de-activation of dashboard/squad & body worn cameras and/or audio devices.

At the December 15, 2021, meeting, the LELCC expressed desire to reaffirm the use of body worn cameras and state why police should wear body worn cameras. Members made the following statements in support of the Madison Police Body Worn Camera Pilot. Body Worn Cameras:

> Provide greater accountability, enhance professional development and training, build trust, and produce justice.
>
> Provide greater benefit to the investigation process, increased outcomes to legal challenges, and clearer examination of matters relating to use of force.
>
> Adopting body worn cameras following the pilot project is an acceptable pathway to full implementation.
>
> There is a need for policies to address privacy concerns when recordings expose private medical conditions.
>
> Body Cameras are less polarizing and potentially more evaluative, and the city should move forward with the pilot. The Common Council should consider previous recommendations and reports during deliberation on body worn cameras.

The LELCC encourages the Madison Common Council to approve the pilot project requested by the Madison Police Department. Furthermore, we strongly encourage the Council to adopt Body Worn Cameras for the entire Department as soon as possible.

The adoption of Body Worn Cameras is major reform addressing the entrenched biases that influence police/community interactions in communities of color. The Council can begin the remediation process with its approval.

Submitted by the Law Enforcement Leaders of Color Collaboration

From: Gisela Wilson <giselawilson@gmail.com>
Sent: Wednesday, January 12, 2022 3:21 PM
To: PD PSRC <PDPSRC@cityofmadison.com>; All Alders <allalders@cityofmadison.com>
Subject: Body Worn Camera Pilot Program

January 12, 2022

RE: Body Worn Camera Pilot Program
Item 13 (PSRC January 12 meeting agenda)

Dear Members of the Public Safety Review Committee and Alders,

I'm writing today to urge all Alders and members of the Public Safety Review Committee and Alders to vote NO and reject the Body Worn Camera Pilot Program. **The primary reasons to vote No on the Body Worn Camera Pilot Program are:**

1. Implementation of body worn cameras (BWCs) has been tried in numerous cities. Those programs have failed to improve officer accountability. The predominant reasons being:
(a) BWC footage is from the wrong perspective — it doesn't show what officers are doing, which undermines the logic of officer accountability. BWC footage produces strong perceptual biases that favor police. For example, due to officer movement BWC footage is very wobbly creating an impression of resistance even when the person or suspect is stationary and accommodating;
(b) BWCs can be turned off or, worse, incidents staged;
(c) It can take years for departments to release BWC footage and often they don't release all of it;
(d) BWCs generate a preponderance of evidence from officer's perspective and, for accountability, that's not the perspective we need. The primary use of BCW footage has been to defend cops against charges and complaints and counter third party video and/or testimony. Body cameras further stack the evidence and tools available in favor of police.
In short, **the benefits of police body cams are a myth.**

2. Members of Body Worn Camera Feasibility Committee suggested several policy correctives if the Body Worn Camera Pilot Program were to be implemented. Even though I have strong doubts that corrective policies would be implemented, **the policy correctives have NOT even been included** in the Pilot Program guidelines.

3. Implementation of BWC programs is terribly expensive. The estimates included in the Feasibility Report come from vendors hoping to sell their products and reel police departments and cities in. As a result, the cost of implementation is a gross underestimate. Cities implementing BWCs have documented the cost to be 8-10% of the annual police budget. Given the high expense, several cities are shutting their BWC programs down. Rather than wanting to be "in style" by implementing BWCs, Madison should be heeding these newer trends if it wants to be at the forefront.

4. There is ample evidence that primary effect of BWCs is to increase the number of arrests for petty crimes. Increasing focus on petty crimes are not an optimal use of officers' time or taxpayer dollars.

5. Recent studies demonstrate have shown that Body Cameras work to increase, rather than prevent, racial bias in policing.

6. Review of footage from BWCs would further eat into officer's time.

7. The benefits of policing, itself, are a myth. Policing is an institution that has been under reform almost since this country's inception. Policing is an excuse not to listen to and heed the needs of those that aren't part of the white upper classes, which violence of the policing serves to protect.

In summary, throwing good money after bad by investing in the pipe dream that BWCs will improve officer accountability is irresponsible, especially at a time when so many communities are in crisis.

Sincerely,

Gisela Wilson, PhD
1244 Morrison
Madison, WI 53703
District 6

From: Lisa Hansen <laax86@gmail.com>
Sent: Wednesday, January 12, 2022 3:22 PM
To: All Alders <allalders@cityofmadison.com>; PD PSRC <PDPSRC@cityofmadison.com>
Subject: Bodycam Resolution Agenda Item #13

Dear Alders and PSRC Members,

I'm writing to you to urge you to oppose the proposed bodycam resolution.

There is research showing several concerns with bodycam use by police.
*There is no statistically significant change in officer use of force vs no bodycam use.
*There is no statistically significant change in misconduct complaints against the police that a city receives.
*The officers and police departments own and control how the camera and footage are used.
*The footage is from one perspective, making it an incomplete picture of the event, which can be used to skew perceptions and create false narratives that help only the police. Therefore not helping the community to hold police accountable. There is a good likelihood that the footage will be more likely against citizens for prosecution than to hold police accountable.
*They can exacerbate racial bias.
*There are concerns of threats to civil rights with use of artificial intelligence, such as facial recognition, with bodycam footage.

Bodycam programs are very expensive and all of this punitive response does not get at the root cause of problems that drive a large part of crime in our community. This money would be better used to invest in our community (education, housing, healthcare, food access, job seeking / training assistance, public transportation, etc.). These are the things that will truly help, support, and uplift our community members. Which helps reduce crime.

Finally, this resolution goes against the recommendations from the Bodycam Committee report and Equal Opportunities Commission. The Bodycam Committee specified 10 strict preconditions that have to be fulfilled for bodycames to be implemented, and if these are not fulfilled then the Committee unanimously agrees that bodycams should not be implemented here in Madison.

https://www.aclu-wa.org/story/%C2%A0will-body-cameras-help-end-police-violence%C2%A0
"A comprehensive review of 70 empirical studies of body-worn cameras found that body cameras have not had statistically significant or consistent effects in decreasing police use of force."

Thank you.
Lisa Hansen
1302 Dewberry Dr
Madison

From: Greg Jones <gcjones15@att.net>
Sent: Friday, January 14, 2022 9:37 AM
To: All Alders <allalders@cityofmadison.com>
Cc: Bottari, Mary <MBottari@cityofmadison.com>; Carter, Sheri <district14@cityofmadison.com>; Barnes, Shon F <SBarnes@cityofmadison.com>
Subject: MPD Body Worn Camera Pilot

Good morning,

The Dane County NAACP supports the proposed pilot relating to Body Worn Cameras. Attached is our statement of support. Please enter this statement into the record and include on the upcoming January 18th council meeting. I will register to speak.

Feel free to contact me with questions.

Greg Jones, President
608-274-3997

WWW.NAACP.ORG

NAACP

Dane County Branch #36AB

January 3, 2022

To: Madison Common Council
Subject: Dane County Statement on Madison Police Body Worn Cameras Pilot

Dear Council Members,

The Dane County NAACP supports the body worn camera pilot outlined by the Madison Police Department and recommends immediate approval and implementation of the pilot. Moreover, the Dane County NAACP requests the Madison Common Council to quickly approve body worn cameras for the entire department.

Body Worn Cameras:

- Can increase transparency and accountability and thus may improve law enforcement legitimacy in communities of color. There is a lack of trust and confidence in law enforcement.

- Can increase civility, i.e., higher rates of citizen compliance to officer commands during encounters and fewer complaints lodged against law enforcement.

- Can lead to a faster resolution of citizen complaints and lawsuits that allege excessive use of force and other forms of officer misconduct.

Whether they're worn by an officer or mounted on police equipment, cameras can provide first-hand evidence of public interactions.

To help ensure that police-operated cameras are used to enhance civil rights, departments must follow the following guiding principles:

A. Develop camera policies in public with the input of civil rights advocates and the local community.

B. Commit to a set of narrow and well-defined purposes for which cameras and their footage may be used, particularly in facial recognition applications which could exacerbate existing disparities in law enforcement practices across communities.

C. Specify clear operational policies for recording, retention, and access. Enforce strict disciplinary protocols for policy violations.

D. Make footage available to promote accountability with appropriate privacy safeguards in place.

Decades of distrust between communities of color and law enforcement have been magnified by the recent rash of police violence perpetrated against unarmed African Americans. We encourage the Madison Common Council to approve the pilot project and seek to implement body worn cameras departmentwide.

Submitted by

Greg Jones, President

From: Ryan Hartkopf <ryanhartkopf@gmail.com>
Sent: Friday, January 14, 2022 5:03 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Opposition to body-worn camera pilot #68625

Esteemed alders,

I am struck by the lack of detail in the resolution #68625 that seeks to implement a body-worn camera pilot in Madison. It effectively gives MPD full control over how a body-worn camera pilot would be executed, and does not address the requirements put forth by the Police Body-Worn Camera Feasibility Review Committee in January 2021.

If a body-worn camera pilot is to be launched in Madison, I would like to see as much care put into it by the alders as the Feasibility Committee put into their report.

Thank you,

Ryan Hartkopf
6633 Raymond Rd
Madison, WI 53711

From: Dawn Hinebaugh <hinebd@yahoo.com>
Sent: Saturday, January 15, 2022 5:35 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Opposition to body cam resolution

Madison Alders,

I am **NOT** in favor of the current pilot police body cam resolution and request that you vote **NO.** Unfortunately, the resolution does not include ANY of the suggestions from two city committees that worked long and hard on this issue. That is a big misstep.

In addition, there is no current scientific data that suggests that *"body-worn cameras can play a role in improving both police performance and community trust in the actions of law enforcement and the criminal justice system".* taken from the resolution

Current science does not support police body cams as a way to help stem situations between police and citizens. In fact, body cams have led to alternative, negative outcomes such as leading to ICE learning about undocumented folks and using the data for their benefit. That's just not right.

Please reject the current resolution. Thank you.

Best,
Dawn Hinebaugh
4701 Barby Lane

From: katherineastyer@gmail.com <katherineastyer@gmail.com>
Sent: Monday, January 17, 2022 12:02 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Opposing item 44, January 18th meeting

Dear Alders,

I am writing you today to ask that you consider opposing item 44 on the January 18th meeting agenda. Others will say it more eloquently than I, but this is a big expense that could be better spent on other things (I.e. housing, mental health care, wraparound services). Bodycam footage can be manipulated and narrated a certain way to bring viewers around to the wearer's point of view, leading to more justice system involvement in people's lives. Please consider alternatives to this proposition.

Thank you and have a good day!
Katie Styer
421 Berwyn drive
Madison

From: BP Dane County <bpsa20pac@gmail.com>
Sent: Tuesday, January 18, 2022 12:43 PM
To: All Alders <allalders@cityofmadison.com>; Harrington-McKinney, Barbara <district1@cityofmadison.com>; Martin, Arvina <district11@cityofmadison.com>; Evers, Tag <district13@cityofmadison.com>; Currie, Jael <district16@cityofmadison.com>; Halverson, Gary <district17@cityofmadison.com>; Myadze, Charles <district18@cityofmadison.com>; Furman, Keith <district19@cityofmadison.com>; Heck, Patrick <district2@cityofmadison.com>; Albouras, Christian <district20@cityofmadison.com>; Lemmer, Lindsay <district3@cityofmadison.com>; Verveer, Michael <district4@cityofmadison.com>; Bennett, Juliana <district8@cityofmadison.com>; Vidaver, Regina <district5@cityofmadison.com>
Cc: Hart, David <dahiii@hotmail.com>; Kirbie Mack <kirbiemack@gmail.com>; Ruben Anthony <ranthony@ulgm.org>; Greg Jones <gcjones15@att.net>; Floyd Rose <president@100blackmenmadison.com>
Subject: Body Worn Cameras Pilot pogram

Good Afternoon Madison Common Council Members,

Regarding, Common Council Agenda #68625 "Implementing Body-Worn Camera Pilot Program".
Attached please find BPSADC's position on the BWC's issue that will be before the Council at tonight's January18, 2022 meeting.

Sincerely,
Theresa Sanders, Secretary
BPSADC, Inc.

Blacks for Political and
Social Action of Dane County, Inc

January 18, 2022

BPSADC, INC

Dear Members of the Common Council,

On behalf of BPSADC, the undersigned write to request that the Madison Common Council move forward and support the decision to conduct a study that would determine if the City of Madison is conducive to our Police Officers wearing body-cameras (PWCs).

We are somewhat concerned that this matter was sent to the Public Safety and Review Committee for additional review and consideration as a ploy to circumvent carrying out the budget as proposed. The body worn camera discussion was previously before the Public Safety & Review Committee for review. There has been no new information presented, no new registrants and basically is seen simply as a disingenuous move on someone's part to stall this pilot. It is our understanding that the PSRC has not been meeting regularly, committee meetings have been cancelled for lack of quorum or its chair not in attendance.

The goal of the pilot is to document and determine if PWCs is appropriate for Madison. For many Black people, BWCs will provide an authentic record of what happened during an incident involving police. We do not argue that BWCs will eradicate police aggression against Black people and other people of color - what it does is to provide a documented record of the event.

A decision was codified and monies allocated for this purpose yet some are using this process as a vehicle to implement their own agendas. We wholeheartedly request that you act in good faith and in the best interest of the City and its citizens and support the BWCS Pilot.

Sincerely,

Kirbie Mack
Theresa Sanders
Ray Allen
Tracey Caradine
Rev. Dr. Alex Gee
Rev. Dr. Marcus Allen
Rev. Joseph Baring
Rev. William Badger
Atty. Angela Arrington
Atty. Joshua Hargrove
Teresita Torrence
Dr. Corinda Rainey Moore
Dr. Cheryl Gittens
Carola Peterson Gaines
Deidre Morgan
Constance Miles
Kaleem Caire
Constance Miles
Pam Soward
Jacqueline Jolly
Annette Miller

BPSADC, INC
P.O. Box 8571, Madison, WI 53708
PHONE 608 244 8282 | EMAIL bpsa20PAC@gmail.com

From: Gisela Wilson <giselawilson@gmail.com>
Sent: Tuesday, January 18, 2022 2:01 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Tonight's Council Meeting - Item 44 - Implementing Body-Worn Camera Pilot Program

January 18, 2022

RE: Body Worn Camera Pilot Program
Item 44 (Common Council January 18 Meeting Agenda)

Dear Alders,

I'm writing today to urge all Alders vote NO and reject the Body Worn Camera Pilot Program. **The primary reasons to vote No on the Body Worn Camera Pilot Program are:**

1. Implementation of body worn cameras (BWCs) has been tried in numerous cities. Those programs have failed to improve officer accountability. The predominant reasons being:
(a) BWC footage is from the wrong perspective — it doesn't show what officers are doing, which undermines the logic of officer accountability. BWC footage produces strong perceptual biases that favor police. For example, due to officer movement BWC footage is very wobbly creating an impression of resistance even when the person or suspect is stationary and accommodating;
(b) BWCs can be turned off or, worse, incidents staged;
(c) It can take years for departments to release BWC footage and often they don't release all of it;
(d) BWCs generate a preponderance of evidence from officer's perspective and, for accountability, that's not the perspective we need. The primary use of BCW footage has been to defend cops against charges and complaints and counter third party video and/or testimony. Body cameras further stack the evidence and tools available in favor of police.
In short, **the benefits of police body cams are a myth.**

2. Members of Body Worn Camera Feasibility Committee suggested several policy correctives if the Body Worn Camera Pilot Program were to be adopted. Even though I have strong doubts that corrective policies would be actually be implemented in an effective manner, **the policy correctives have NOT even been included** in the Pilot Program guidelines.

3. Implementation of BWC programs is terribly expensive. The estimates included in the Feasibility Report come from vendors hoping to sell their products and reel police departments and cities in. As a result, the cost of implementation is a gross underestimate. Cities implementing BWCs have documented the cost to be 8-10% of the annual police budget. Given the high expense, several cities are shutting their BWC programs down. Rather than wanting to be "in style" by implementing BWCs, Madison should be heeding these newer trends if it wants to be at the forefront.

4. There is ample evidence that primary effect of BWCs is to increase the number of arrests for petty crimes. Increasing focus on petty crimes is not an optimal use of officers' time or taxpayer dollars.

5. Recent studies demonstrate have shown that Body Cameras work to increase, rather than prevent, racial bias in policing.

6. Review of footage from BWCs would further eat into officer's time.

7. The benefits of policing, itself, are a myth. Policing is an institution that has been under reform almost since this country's inception. Policing is an excuse not to listen to and heed the needs of those that aren't part of the white upper classes, which violence of the policing serves to protect.

In summary, throwing good money after bad by investing in the pipe dream that BWCs will improve officer accountability is irresponsible, especially at a time when so many communities are in crisis.

Sincerely,

Gisela Wilson, PhD
1244 Morrison
Madison, WI 53703
District 6

From: Nicholas Davies <nbdavies@gmail.com>
Sent: Tuesday, January 18, 2022 4:12 PM
To: All Alders <allalders@cityofmadison.com>
Subject: No on body-camera pilot program (68625)

Hello alders,

I ask you to follow the PSRC recommendation and place this item on file. I think Alder Heck made a compelling argument for doing so.

The ad-hoc committee recommended a body-camera program only if a set of preconditions could be met, and I don't see those being met so far or as part of item 68625.

In general, when I consider a body-camera program, what I want to evaluate is whether it will do more to hold police accountable, or whether it will be used more as a tool of civilian surveillance. And even if it does confer a benefit overall, whether it's worth the budget and administrative burden.

A body-camera program is not the same as sending an objective documentary crew out with every patrol officer. Officers will have control over where the camera is, what direction it's pointing, and to what extent its view is obstructed. So when capturing the footage, they effectively have the control of an editor/director.

In addition, it's my understanding that it would be police reviewing the footage, casting further doubt that, even if it records a tree falling in the forest, the public may never hear a sound.

Our technological options are evolving fast, and I would like to see Madison avoid sinking substantial cost into this one, when the evidence of its effectiveness is so mixed.

Thank you,

Nick Davies
3717 Richard St

From: Harry Richardson <richardsonharry348@gmail.com>
Sent: Tuesday, January 18, 2022 5:36 PM
To: All Alders <allalders@cityofmadison.com>
Subject: no body cameras on police

Dear Alderpersons,
I hope that you will reject this costly experiment of police body cameras. Studies have shown that the cameras create perception distortion favoring the police. The footage is in the control of the police and can be edited by them. Future body camera footage could be subpoenaed by ICE and other federal agencies for facial recognition usage which is subject to unreliability and abuse. Finally reported costs of a fully implemented program of $23 million dollars over five years is just too costly. This money would be better spent on social service programs to aid homeless people, cut down on recidivism in jails and so forth rather than feeding an already bloated police budget. Madison must get at the root of the problems of why people go to jail and how to prevent incarceration. This proposal will not help resolve this basic problem. Thank you for your consideration of this important issue.
Sincerely Yours,
-Harry Richardson
456 N Few St

From: Norm Littlejohn <norm.littlejohn@gmail.com>
Sent: Tuesday, January 18, 2022 6:30 PM
To: All Alders <allalders@cityofmadison.com>; Mayor <Mayor@cityofmadison.com>
Subject: NO BODY CAMS FOR POLICE!

Hello, Madison Alders and Mayor Rhodes-Conway -- I'm writing to oppose the pilot program for body cams for Madison Police Department officers. These expensive cameras have a history of almost entirely supporting prosecutions, and seldom if ever helping to make police accountable to the communities they police. They literally show only the police officers' point of view, and ***do not capture everything that happens in a police encounter with citizens***.

We should not spend our money on further empowering police; they have plenty of tools at their disposal already, and the police department has had the largest budget of any city department for years.

Please turn down this pilot program. Thank you.

Norm Littlejohn
2209 Cypress Way #16, Madison

From: Steve Verburg <stverburg@gmail.com>
Sent: Tuesday, January 18, 2022 6:58 PM
To: All Alders <allalders@cityofmadison.com>
Subject: please vote no on bodycam pilot

Members of the Madison Common Council:

I'm writing to urge you to vote against agenda item 44, the bodyworn camera pilot program.

The committee that studied this issued a report calling for a number of conditions to be met before the city embarked on any bodyworn camera program. These conditions have not been met. The conditions were intended to ensure that bodyworn cameras don't harm the city or its residents. This by itself is enough to require that the current proposal be rejected.

Even if all of the conditions were met, bodyworn cameras would be harmful to the community.

I was listening to a "Coffee With A Cop" meeting a few days ago. Attendees were urged to contact city officials to demand approval of the bodyworn camera pilot. Madison Police Department Neighborhood Resource Officer Howard Payne told attendees at the Pinney Library and on a Zoom connection that he personally was an advocate for bodyworn cameras and he hoped that anyone who agreed with him would advocate for the equipment. Officer Payne specifically suggested that citizens contact the mayor and alders.

It was telling that Officer Payne compared bodyworn cameras to audio recorders that Madison police sometimes activate while interacting with the public. Officer Payne said that if the audio recorder is activated, it is a way to exonerate police officers accused of misconduct.

Tellingly, he made no mention of any possibility that a recording could be used to sustain a citizen complaint.

There are reasons police organizations favor bodyworn cameras. Police and prosecutors will always have control over whether recordings are made, preserved, and made available.

Thanks for considering my request that you vote against the proposed bodyworn camera proposal.

Sincerely,

Steve Verburg
Madison Wi
53716

From: Alexandra Wilburn <wilburnalexandra@gmail.com>
Sent: Tuesday, January 18, 2022 7:19 PM
To: All Alders <allalders@cityofmadison.com>
Subject: On body cam opposition

Unlike some of my fellow abolitionists, I'm not completely against body cameras. I believe we need an independent monitor and a fully funded and supported PCOB before we implement any new technological tools for police.

Through observation of incidents; some MPD members do not have the citizens best interest at heart and neither does district attorney ismael ozanne does not nor the police and fire commission.

The district attorney in this town has a history of covering up for the police when there is misconduct (Tony Robinson Jr.) , and the police and fire commission aided in the cover up as well, So I think we need proper civilian oversight before implementation of more physical tools for police investigation

Those systems of accountability are full of people who hold protecting the "blue wall" over true accountability and justice.

I think it is incorrect to implement new technology without an independent-of-police agency to assure transparent oversight of the roll out of this technology.

Please help get the administrative processes of accountability measures in place before we implement the physical tools.

Please also ban tear gas as it is banned in war and found in the Quantrone study/report to only incite not quell the violence.That'd really progress us towards a humane, up to the moral standards of the times, system of law enforcement (in my opinion)

On Body Cams - There's a man named John Roy King who was assaulted on camera and would have gotten a longer sentence if there were no cameras. I helped build an awareness campaign to get people to contact the law enforcement agencies to release the footage because they didn't release it to the public defender, and took weeks to give the footage to the public. Despite being on camera the district attorneys office in columbia county decided that John incited the violence upon himself because he was scared and tried to jump in front of the car, to be in view of the dash camera. I believe is incredibly ignorant of them to view the footage and still see John as the problem in the situation instead of the excessive force and violence of the law enforcement agents.

That's one example of life experience I have that is leading me to my conclusions of waiting for body cameras.

I want a proper independent monitor and PCOB in place before the body cam pilot program, because in the hands of a law enforcement department I've received incredible difficulty getting footage and then the district attornies still blamed John for getting assaulted by law enforcement.

It may not have been MPD in this life experience I am citing but it revealed in my opinion a glaring issue with cameras -> assurance to access to the footage in a timely manner AND accountability for people who try to stall public from attaining the footage particularly when it is involved in someones defense.

Also - Body cameras are cameras and because of lighting and positioning body camera footage is not an infallible way to see an occurrence.

The Black community (and every community) is not a monolith. I'm reading a book on the history of abolition and back in the 1800's some Black folk wanted to be removed from this country to go colonize African, some wanted to keep the union together and end slavery in the south, some wanted to abandon the south, and have the north start their own country.

Let's not settle for Quasi-liberal, let's be truly progressive. We need systems of accountability before new technology.

Be Well!
Alexandra Wilburn

P.s.

John King Documentation/footage

John's side of things https://www.youtube.com/watch?v=uvskwNmF_iA&t=115s

Dash and body cam of incident
https://www.youtube.com/watch?v=udeLj6JQbag&t=1s

From: Veronica Figueroa <artmvfdesign@gmail.com>
Sent: Tuesday, January 18, 2022 8:18 PM
To: All Alders <allalders@cityofmadison.com>
Subject: Opposing Body Worn Cameras

Dear all council members,

Since 2015 the council and other committees have invested endless hours gathering data and input. The community has also provided input about this issue. Police Body-Worn Camera has failed to be adapted over and over again. Meanwhile, the relationship between police and the community has not improved.

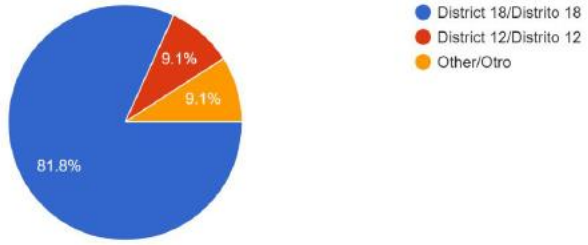
For the sake of more data, I took it upon myself to request input from my community (See document attached). Attached you find a small chart indicating what our community wants. At this time, the most attainable solutions worth pursuing are police-community relations and police reform. Please refer to the MADISON POLICE DEPARTMENT POLICY & PROCEDURE REVIEW AD HOC COMMITTEE to review the recommendation and implement them. I opposed this old/new request. Let us refocus the conversation here and invest in our communities and in the relationship building that will create a safer community.

Respectfully,

Veronica Figueroa
Artist & Graphic Designer
MVF Art & Design
https://www.artmvfdesign.org
608-977-4071
3709 School Rd
Madison, WI 53704

Are you a district 18 or district 12 resident?/¿Es usted resident del distrito 18 o del distrito 12?

11 responses



If you have the choice to invest $445,000 in your community, what would you prioritize? Si tiene la opción de invertir $ 445,000 en su comunidad, cual seria su prioridad?

11 responses