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Police reform in public housing contexts: Body-worn cameras, surveillance, and harm reduction in New York City Housing Authority Developments

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Abstract

Research summary: The concern of crime in New York City public housing complexes motivated heightened police patrol of buildings and the enforcement of trespass laws. The 2013 federal court settlement of *Davis et al. v. City of New York et al.* mandated that the New York City Police Department (NYPD) implement a series of reforms, including the deployment of body-worn cameras (BWCs) on officers, to address unconstitutional use of criminal trespass enforcement and stop and frisk practices in public housing developments. This study employed a stepped wedge quasi-experimental design that involved the sequential crossover of public housing service area clusters from control to BWC implementation until all NYPD housing bureau officers were equipped with BWCs. Panel regression models at the individual officer and service area levels were used to estimate BWC program impacts on outcomes between 2015 and 2019. Logistic regression models were used to estimate the impact of the BWCs on the lawfulness of officer stop reports that were randomly selected for audit between 2017 and 2019. Results show that BWC deploy-

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ment in public housing reduced excessive enforcement, citizen complaints, and use of force by NYPD housing officers. Findings further suggest that BWCs can help reduce constitutionally problematic stops and frisks of citizens.

Policy implications: Problematic police activities in public housing contexts can be reformed using BWCs. When coupled with routine supervisory review of video footage, the deployment of BWCs on public housing officers can improve compliance with department directives to reduce enforcement actions and increase documentation of citizen stops.

Public housing provides an important source of affordable homes for more than 900,000 low-income households in the United States, the majority of which are minority residents with incomes below the poverty line (Fischer et al., 2021). Unfortunately, many public housing developments have unsafe and unhealthy environments driven by persistent problems related to a legacy of segregation, poor funding for maintenance and repair, and lack of oversight from agencies administering public housing programs (Finkel et al., 2010; Popkin et al., 2020). In 2017, the New York City Housing Authority (NYCHA), which administers 17% of public housing in the United States, estimated that their developments needed $32 billion in capital improvements over 5 years to improve living conditions for their residents (STV Incorporated & AECOM USA, 2018). Many public housing developments experience higher levels of crime and violence relative to contiguous communities due to the concentration of vulnerable populations in sometimes substandard living conditions (Fagan et al., 2006; Holzman, 1996).

The connection between crime and public housing has long been recognized as one of the motivations behind increased police surveillance and enforcement in public housing complexes (Austen, 2012; Schill, 1993). The New York City Police Department (NYPD) is well known for their use of proactive policing strategies that encouraged its officers to make very high numbers of citizen stops and misdemeanor arrests to control crime during the 2000s and early 2010s. Guided by mapping and spatial analyses of crime incidents (Eterno & Silverman, 2012), these aggressive enforcement actions were concentrated in public places that experienced high levels of violent crime such as NYCHA public housing developments. Although there is evidence that these NYPD strategies were associated with modest crime control gains (MacDonald et al., 2016; Weisburd et al., 2016), numerous studies suggested that these policing efforts generated racially disparate impacts on police stops and arrests of Black and Hispanic residents (e.g., Fagan & Davies, 2000; Gelman et al., 2007; Jones-Brown et al., 2010). Fagan et al. (2012) estimated that NYPD stops, frisks, and arrests of Black and Hispanic NYCHA residents and their visitors were much higher in public housing settings relative to enforcement patterns in adjacent neighborhoods.

In 2010, a group of NYCHA residents and their visitors filed a federal class action lawsuit against the City of New York, *Davis et al. v. City of New York et al.* (10 Civ. 0699), which accused the NYPD of unconstitutional use of stops and arrests for criminal trespass in NYCHA housing complexes. Ultimately in 2015, the City of New York settled with the *Davis* plaintiffs and agreed to undergo a series of reforms to NYPD criminal trespass enforcement practices specifically. The City of New York

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also agreed to ongoing federal court monitoring with other plaintiffs in cases involving unconstitutional use of stop, question, and frisk (*Floyd et al. v. City of New York et al.*—08 Civ. 1034) and trespass enforcement in privately owned buildings (*Ligon et al. v. City of New York et al.*—12 Civ. 2274). The combined settlement agreement included a broad set of remedies to reduce racial disparities and improve the lawfulness of NYPD stops of citizens (see 959 F. Supp. 2d 668, 685; S.D.N.Y. 2013, Remedial Order). The deployment of body-worn cameras (hereafter, BWCs) on NYPD officers was mandated to create objective records of stop-and-frisk encounters, encourage lawful and respectful police-citizen interactions, and offer a way to substantiate whether NYPD officers engage in alleged misconduct. These same issues were specifically mandated to apply to the NYPD's trespass enforcement activities in NYCHA housing as part of the *Davis et al.* case.

The existing program evaluation evidence on the impact of BWCs on police officer work behaviors is largely mixed with some indication that the presence of the technology may improve the civility of police-citizen encounters (Lum et al., 2020; Williams et al., 2021). Scant research assesses the impact of BWC technology on the lawfulness of police actions during encounters with citizens, and, to date, no studies evaluate the use of BWCs in public housing contexts. In this study, the impact of BWC deployment on officers working in NYCHA public housing developments is measured. The *Davis et al.* settlement specifically identified problematic trespass enforcement practices in NYCHA developments, making it important to examine the impact of the 2018 BWC deployment on Housing Bureau officers and their interactions with civilians. This evaluation assesses whether the deployment of BWCs on Housing Bureau officers affected the civility of police-citizen encounters, the level of police enforcement activity, and the lawfulness of police encounters, comparing those outcome measures both before officers were equipped with BWCs and for a year after BWCs were deployed. These are particularly important issues to examine in the context of NYCHA housing, which are areas with the highest rates of violent crime in NYC, have undergone extensive surveillance of residents and their guests, and have experienced decades of intensive law enforcement activities.

1 | LITERATURE REVIEW

1.1 | Public housing, crime, and aggressive law enforcement

In the United States, public housing started during the 1930s and was intended to reduce disadvantaged neighborhood conditions for the benefit of mostly White working-class populations (Cavanaugh, 2005). After World War II, public housing construction expanded with the development of clusters of high-rise towers nested in neighborhoods that were experiencing noteworthy social and structural changes (Schill, 1993). The development of high-rise public housing was advocated to reduce crime, efficiently clear out "slums," and improve the public health of communities (Corburn, 2013). During the 1950s, White working-class families, often supported by the GI Bill, increasingly moved out of public housing to the suburbs and were replaced by poor non-White residents (Marcuse, 1995). Conflicts in varied social policy areas, such as welfare policy, education, and housing, accompanied these dramatic changes in the racial composition of city neighborhoods (Massey & Denton, 1993; Perlstein, 2008). Increasing shares of minority residents were accompanied by decreasing support for the maintenance of aging buildings, leading to the deterioration of the physical environment in public housing (Finkel et al., 2010; Popkin et al., 2020). White flight from public housing and the surrounding neighborhoods was influenced by the racial threat posed by concentrated minority populations and the declining physical conditions of public housing developments (Blalock, 1967; Sko-

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gan, 1990). Public housing complexes in most U.S. cities became increasingly poor over time. Like high-crime neighborhoods (e.g., Sampson et al., 1997), the clustering of crime in public housing stems from concentrated disadvantage, diminished collective efficacy of residents, and social and physical conditions of public housing spaces that facilitate crime opportunities (Venkatesh, 2000).

Concerns about crime in public housing have shaped public policy agendas for many decades and have generated numerous crime control and prevention programs. Violent crime rates are generally higher in public housing sites relative to other locations in urban neighborhoods (Dunworth & Saiger, 1994; Griffiths & Tita, 2009; Haberman et al., 2013). For instance, only 7% of New York City residents resided in NYCHA developments, but 27% of fatal and nonfatal shootings in the city occurred in these developments between 2010 and 2019 (Hermann, 2020). NYCHA developments have also been identified as central locations for illicit drug distribution networks supplying surrounding neighborhoods (Curtis, 2003). The mid-to-late 1980s crack epidemic further fortified a racialized perception that high-rise housing projects, mostly populated by Black residents, were epicenters of illegal drugs and violence in cities (Austen, 2012). This perception remains today, with recent media accounts suggesting that the underlying drug and crime dynamics in NYCHA housing drove the COVID-19 pandemic rise in shootings in New York City (e.g., Smith, 2021). Such narratives have supported law enforcement, over improved building maintenance and rehabilitation, as a primary response to curbing crime in public housing projects (Lazarus, 2004).

The strategies used by public housing authorities and police departments to control crime have varied over the last 50 years. During the 1970s, crime prevention through environmental design strategies (CPTED) were implemented to manipulate aspects of the built environment observed to give rise to crime opportunities in public housing; CPTED interventions included improved lighting, decentralized lawns and play areas, the installation of fences and gates, and well-placed windows to enhance informal surveillance (Jeffrey, 1977; Newman, 1973). Heightened gun violence associated with the mid-1980s to early-1990s crack epidemic spurred public housing authorities and police departments to take a more aggressive stance toward crime in public housing developments. U.S. Congress passed the Anti-Drug Abuse Act of 1988 (P.L. 100-690) that supported the U.S. Department of Housing and Urban Development (HUD) in distributing hundreds of millions of dollars in grants to implement drug control efforts as part of their Public Housing Drug Elimination Program. Although some of this money was spent on antidrug abuse programs and physical improvements to housing sites, many jurisdictions used these funds to support increased numbers of police officers in public housing developments, trespass arrests, warrant sweeps, buy-busts, surveillance operations, and other traditional drug law enforcement tactics (Annan & Skogan, 1993; Uchida et al., 1992).

Public housing authorities also implemented policies such as limiting access to public housing buildings through electronic identification systems, improving tenant screening procedures by including criminal background checks in the application process, and increasing evictions of tenants who were involved in illicit drug sales and other criminal activities (Collins et al., 1998; Weisel, 1990). In 1996, a New York district court ruled that the crack epidemic constituted a quantum leap in the seriousness of violent crime and drug problems in NYCHA public housing and modified an existing consent decree to facilitate the eviction of tenants involved in drug sales (*Escalera v. NYCHA*, 1996; White, 1996). What is more, under the guidance of a federal "one strike" policy and in accord with the Housing Opportunity Program Extension Act of 1996, public housing managers used administrative hearings to evict suspected offenders without waiting for a criminal conviction; a "preponderance of the evidence" was sufficient to establish a tenant agreement violation occurred that supported tenant eviction (*HUD v. Rucker*, 2002; Hannaford, 2003).

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The 1994 Violent Crime Control and Law Enforcement Act and establishment of the Office of Community Oriented Policing Services supported the adoption of community and problem-oriented policing programs in public housing sites. These programs supported partnerships between police, housing managers, tenants, service providers, and others to address the underlying conditions that gave rise to persistent crime and disorder problems (Dunworth & Saiger, 1994; Mazerolle et al., 2000; Weisel, 1990). In the late 1990s and early 2000s, some police departments drifted away from community and problem-oriented policing approaches and toward a "new policing" model that applied complex metrics, strong management, and aggressive enforcement and surveillance to focus policing on high crime-risk persons and places (Heymann, 2000). The NYPD promulgated this approach that emphasized crime mapping and analysis to identify high-crime areas; used trend analysis to measure performance for internal management; applied public shaming, reassignment, and potential demotion of high-level managers who failed to reduce crime in their precincts; and implemented proactive tactics to disrupt potential criminal activities (Eterno & Silverman, 2012). These tactics included aggressive street stops and frisks to disrupt crimes and seize weapons and order maintenance strategies that used arrests for misdemeanor offenses to suppress more serious crimes.

Although these NYPD strategies were applied citywide, many NYCHA developments experienced more intensive enforcement attention given their identification as violent crime hot spots (Fagan et al., 2012). Beyond increased stops and misdemeanor arrests, the NYPD used other enforcement tactics to control violent crime and disrupt drug trafficking. Trespass arrests intended to eliminate loitering in hallways, stairwells, and other locations were used to disrupt indoor drug trade and reduce social disorder (Boland, 1998; Carlis, 2009). Interior patrols (sometimes called "vertical patrols") used to systematically check lobbies, stairwells, and roof landings for criminal and disorderly activity represent a central policing activity in NYCHA housing (Carlis, 2009). NYPD also notifies NYCHA of any arrests of residents, whether these events take place in public housing or not, and these reports can lead to evictions (Obanor, 2016). Security cameras are omnipresent in NYCHA properties with direct feeds to the NYPD so they can monitor residents and their visitors for possible criminal behavior and identify persons wanted for prior crimes (Diaz, 2019).

The Fourth Amendment provides the legal basis for civilian stops, arrests, frisks, searches, and use of force by police officers. 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, require 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 very low burden of proof to meet the reasonable suspicion requirement to conduct a stop and frisk (White & Fradella, 2016). Some legal scholars and criminologists argue that the reasonable suspicion standard invites racial discrimination as explicit and implicit biases can cause minority group members to be stopped and frisked by simply living or working in a perceived high-crime area (Capers, 2011; 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 (*Draper v. United States*, 258 U.S. 307, 1959). As these actions are more intrusive than pat-down frisks of individuals, searches also require probable cause that an officer believes that a crime suspect or evidence of a crime will be found in the location searched. Searches sometimes require the police to acquire a warrant from

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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, meet reasonableness standards and balance the rights of the individual against government interests (e.g., *Mapp v. Ohio*, 367 US 643, 1961; *Graham v. Connor*, 490 U.S. 386, 1989).

An analysis of 2004–2011 stops, frisks, and arrests found that trespass stops and arrests in NYCHA public housing were two times greater than in the immediate surrounding area, and this difference was driven by increased percentages of Black and Hispanic populations in public housing (Fagan et al., 2012). A more recent analysis suggests that these enforcement and surveillance patterns were associated with a "public housing to prison pipeline" that disproportionately incarcerated residents of Black and Hispanic neighborhoods where NYCHA housing is present (Holder et al., 2022). In addition to allegations of unlawful and racially disparate stops and frisks, the 2010 *Davis et al.* lawsuit contended that NYPD officers illegally detained and arrested NYCHA residents and their visitors on trespassing charges when they were not engaged in illegal activities nor on NYCHA property without proper authorization. In the 2015 *Davis et al.* settlement, the federal court judge hearing the lawsuit ruled that the NYPD was engaged in a pattern and practice of unconstitutional trespass stops and arrests of citizens, including violations of 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 based on race, religion, country of origin, and other characteristics.

The *Davis* case became part of the court monitoring process that was ordered in the *Floyd et al.* lawsuit that successfully challenged the NYPD's stop-and-frisk policies. To ensure that officer stops of citizens conformed to 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 BWC program. An independent monitor was appointed by the federal court to oversee the execution of the settlement reforms. All officers received training in the lawful conduct of stops, frisks, and searches. However, NYPD Housing Bureau officers received additional training on the interior patrol of NYCHA buildings, how to engage civilians in a respectful and courteous manner, NYCHA rules and regulations, and criminal trespass elements.[2] Since the *Davis* settlement, citywide NYPD criminal trespass arrests dropped by almost 74%, trespass summonses have declined by 80%, and trespass stops have decreased by 96% from 2013 to 2019.[3] These substantial declines in NYPD trespass enforcement activities were observed in NYCHA buildings, in areas surrounding NYCHA buildings, and throughout New York City's five boroughs.

1.2 | BWCs: Theoretical perspectives and evaluation evidence

Deterrence and public awareness theories are often posited as theoretical justifications for deploying BWCs on police officers to ensure civil and lawful police-citizen encounters (Ariel et al., 2015; Braga et al., 2018). Deterrence theory suggests prospective offenders decide against committing criminal acts when the perceived costs exceed the perceived benefits (Zimring & Hawkins, 1973). Studies find that deterrence is generated when specific interventions enhance offender perceptions of their perceived risk of apprehension and certainty of punishment (Nagin, 2013). BWCs may deter inappropriate and illegal behaviors by enhancing police officer and citizen perceptions of sanction risks through the capture of their actions on video. Public awareness theory suggests

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that people are more likely to compare their current behavior to established social norms, conform to set rules, and behave in socially desirable ways when they know that are being watched (Duval & Wicklund, 1972). Studies have demonstrated that the presence of mirrors, audiences, video and audio recordings, and other environmental cues inspire self-awareness (Silvia & Duval, 2001). The presence of BWCs may stimulate police officers and citizens to consider societal norms during their encounters and modify their immediate behaviors to be more socially desirable.

The saturation of closed-circuit television cameras, drones, smartphones, and other video recording devices has made video surveillance ubiquitous in modern society (Evans, 2015; Haggerty et al., 2011). These general societal changes, coupled with the rapid adoption of BWCs by police departments, have generated a "new visibility" of policing as high-quality videos of police-citizen encounters can be instantly and easily shared with mass audiences (Brown, 2016; Goldsmith, 2010). The prominence of video cameras in public places and the routine recording of police officer activities have developed into a key accountability system. In essence, the new visibility of policing has created a "synopticon" or reciprocal support system where the few watch the many (Doyle, 2011; Mathiesen, 1997). In addition to addressing community demands to deploy BWCs on officers, police departments have a practical reason to adopt this technology. BWC recordings capture the officers' gaze and these videos can be helpful in limiting organizational liability by countering narratives developed by citizens who use smartphones to record officer behaviors during interactions with citizens.

The use of BWCs as a prophylactic technology to counter the gazes of citizen smartphone videos is consistent with Foucault's (1977) competing gaze paradigm. Police departments and community supporters alike recognize the importance of recording encounters that capture events from the officers' perspectives that can be used to protect them from unjustified allegations of officer wrongdoing. Some 78% of police departments that deployed BWCs on their officers reported that reduced liability was a key reason that the technology was adopted (Hyland, 2018). In two Florida counties, a survey of residents revealed that community members who were favorable toward law enforcement were more likely to support BWCs, not as an accountability mechanism but as a tool to combat negative views of the police that result from "rare bad acts" captured on smartphone video (Crow et al., 2017, p. 605). 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 offered by BWC videos can hold both officers and citizens accountable for their behaviors during encounters.

The available program evaluation evidence on the effects of BWCs on the civility of police-citizen encounters as measured by citizen complaints and officer use of force incidents, police work activities, and police lawfulness is briefly reviewed here. The divergent program evaluation findings presented here may be due to variations in local implementation contexts such as 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).

Many randomized controlled trials (RCTs) and quasi-experimental evaluations report large reductions in citizen complaints of inappropriate police behavior and misconduct when BWCs are deployed on officers relative to non-BWC-wearing counterparts (e.g., Ariel et al., 2015; Braga et al., 2018; Jennings et al., 2015), whereas a smaller number does not find any noteworthy impacts (e.g., White et al., 2018; Yokum et al., 2017). A systematic review and meta-analysis of 30 independent studies suggest that BWCs reduce citizen complaints against officers, but it is unclear whether these reductions represent bona fide improvements in the civility of police-citizen encounters or changes in citizen reporting behavior (Lum et al., 2020).

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Several controlled evaluations have found no differences in various police uses of force outcomes when BWC officers are compared to control non-BWC officers (e.g., Headley et al., 2017; Peterson et al., 2018; White et al., 2018). However, other controlled evaluations do report reductions in police use of force outcomes for BWC officers relative to control non-BWC officers (e.g., Ariel et al., 2015; Braga et al., 2018; Hedberg et al., 2017; Jennings et al., 2015). Lum et al.'s (2020) systematic review did not find BWCs were associated with significant reductions in police use of force. A moderator analysis, however, showed that BWCs officer use of force was lower during citizen encounters when police departments have policies that limit officer discretion in deciding when to activate a camera. A more recent meta-analysis of BWC evaluations found that the technology was associated with significant reductions in police use of force (Williams et al., 2021). Additionally, a recent quasi-experimental evaluation of the implementation of BWCs across U.S. police departments found a reduction in police-involved citizen fatalities between 2005 and 2019 (Miller & Chillar, 2022).

The positive impacts of BWCs on citizen complaints and officer use of force suggest that the technology could enhance police legitimacy. Although other factors are also influential (such as crime control effectiveness and lawfulness; see Tankebe, 2009), community members' perceptions of police legitimacy are powerfully shaped by the processes through which the police enforce the law (Tyler, 2006). Civilians view police encounters as procedurally just when police decision-making is considered fair, and police treat them with respect and dignity (Tyler, 2006; Weisburd et al., 2022). Several studies suggest BWCs stimulate procedurally just behaviors when officers wear the equipment, and as a result, these improved police-citizen encounters enhance citizen perceptions of police legitimacy and facilitate cooperation and compliance with police directives during these interactions (Demir, Apel, et al., 2020; Demir, Braga, et al., 2020; McCluskey et al., 2019).

Some studies suggest that BWCs may make officers more likely to enforce laws if the resulting videos are reviewed by supervisors who may unfairly discipline them for not issuing summons or making arrests of citizens when such actions could be legally justified (Police Executive Research Forum, 2014; Ready & Young, 2015). Survey research further suggests that police officers view BWC videos as potentially important tools that create video evidence of events during police-citizen encounters that support arrests and subsequent prosecutions of criminal offenders (Goodall, 2007). Several RCTs reported no changes in various work behaviors for officers outfitted with BWCs relative to non-BWC control officers (Braga et al., 2020; Grossmith et al., 2015; Peterson et al., 2018). Other RCTs and quasi-experimental evaluations have noted significant differences in the work behaviors of BWC officers relative to non-BWC comparison officers such as increased self-initiated encounters, citations, and arrests (Braga et al., 2018; Hedberg et al., 2017; Ready & Young, 2015; Wallace et al., 2018).

A key factor motivating the adoption of BWCs in many jurisdictions has been concerns over accountability for unlawful police actions during interactions with citizens (Stanley, 2015; Todak et al., 2018). Despite almost 74% of large police departments reporting increased officer accountability as a justification for BWC adoption (Hyland, 2018), tests of the impacts of BWCs on police lawfulness have been surprisingly absent from BWC program evaluation research (Lum et al., 2020). To date, only one study has examined whether BWC deployments impacted the lawfulness of police actions during encounters with citizens. An RCT found that BWCs may increase officer compliance with NYPD directives to document all citizen stops and detect unlawful policing during police-citizen encounters (Braga et al., 2022). That study did not evaluate BWC impacts on lawful policing by NYPD officers assigned to work in NYCHA housing developments.

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1.3 | Study context of New York City Housing Authority Developments and BWC implementation

The NYPD Housing Bureau provides policing services to the vast majority of residents in NYCHA housing through nine Police Service Areas (PSAs) that effectively are managed as their own set of precincts. Specifically the NYPD Housing Bureau services residents in 258 NYCHA housing developments—75.2% of the 343 total NYCHA housing developments—through nine NYPD PSAs in four boroughs: Brooklyn, the Bronx, Manhattan, and Queens.[4] In total, NYPD officers working in the Housing Bureau provided policing services to some 258 housing developments occupying three square miles and consisting of 467 buildings, 149,684 apartments, and an estimated 333,977 residents. In 2017, the year prior to BWC deployment, the overall NYCHA resident population was 54.9% Hispanic, 36.6% Black, 3.1% Asian, 3.1% White, and 2.3% other races.[5]

Table 1 presents the total number of developments and residents living in NYCHA housing served by each of the nine PSAs in 2017. Summary data are presented for the average number of NYPD officers assigned, 911 emergency calls for service dispatched, major crime incidents (murder, rape, robbery, felony assault, burglary, grand larceny, and grand larceny auto), arrests, arrests where force was used by the officer(s) making the arrest(s), and number of stop reports between 2015 and 2017 in each PSA. There were variations in resident populations, reported crimes, officer work activities, and other indicators across the nine PSAs with some commands serving larger populations and handling more crime incidents and calls for service. NYPD Housing Bureau officers in the nine PSAs were equipped with BWCs on a rolling basis (by PSA) over the course of a nearly 11-month period (Table 1). The PSA BWC implementation began the week of February 12, 2018 (PSA 8) and was completed the week of December 16, 2018 (PSA 9). When BWC deployment occurred, all officers working in a specific PSA received training on the particulars of the BWC policy and the proper equipment operation. The NYPD policy on BWC requires the video recording of all citizen stop encounters and interior patrols of NYCHA buildings.[6]

Table 2 presents descriptive characteristics and Citizen Complaint Review Board (CCRB) complaint histories for the Housing Bureau officers assigned to patrol in the PSA commands as of December 31, 2017 ($N = 1916$). The vast majority of Housing Bureau officers assigned to patrol held the rank of police officer (88.7%), whereas a much smaller share held the rank of sergeant (11.3%). Housing Bureau patrol officers in the PSA commands were primarily male (81.9%), had an average age of 33.6 years, and averaged 6.8 years on the job. Housing Bureau patrol officers were racially diverse with almost two thirds (65.7%) identifying as non-White. PSA officers generated, on average, 0.18 CCRB complaints per year on the job (or less than one complaint every 5 years).

2 | RESEARCH DESIGN

Given the small number of PSAs and the complexities associated with the citywide implementation of BWCs, it was not practical to design an RCT of the placement of BWCs on NYPD Housing Bureau officers. Instead, a quasi-experimental design or controlled trial was used. Although quasi-experiments do not have the same degree of internal validity as an RCT, a well-designed controlled trial can produce results that are of similar quality (Berk et al., 2010; Lipsey & Wilson, 1993).

The fact that BWCs were provided to Housing Bureau officers on a rolling basis, one PSA at a time, allowed the research team to use a cluster stepped-wedge quasi-experimental (controlled trial) evaluation design (also known as a case-crossover design; see Hemming et al., 2015; Hu & Hoover, 2018). In this study, the pre-intervention period in which police officers clustered within

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TABLE 1 Police Service Area (PSA) developments, residents, body-worn camera (BWC) deployment dates, and New York City Police Department (NYPD) officer staffing and work activities, 2015–2017.

	Borough	Total developments	Total residents	Mean officers	Mean 911 calls	Mean major crimes	Mean arrests	Mean arrests w/force	Mean stop reports	BWC deployment date
PSA 1	Brooklyn	20	36,764	246.3	29,280.3	512.0	1790.7	38.3	241.7	Week of 6/18/2018
PSA 2	Brooklyn	43	43,906	328.0	17,979.3	696.0	2350.7	65.0	121.7	Week of 10/21/2018
PSA 3	Brooklyn	28	45,186	262.0	22,117.0	638.7	2089.3	53.7	124.3	Week of 3/5/2018
PSA 4	Manhattan	32	35,243	166.3	12,284.0	388.0	1759.7	22.0	204.7	Week of 4/9/2018
PSA 5	Manhattan	29	37,834	245.7	21,489.3	567.7	3125.0	51.3	204.3	Week of 12/2/2018
PSA 6	Manhattan	26	29,185	158.3	14,743.0	432.3	1497.0	15.3	153.0	Week of 8/19/2018
PSA 7	Bronx	49	49,248	241.3	22,109.7	677.3	3651.7	43.7	313.7	Week of 7/1/2018
PSA 8	Bronx	19	31,278	193.0	16,803.7	605.0	2046.0	27.3	178.7	Week of 2/12/2018
PSA 9	Queens	12	25,333	217.3	21,037.7	284.3	1264.0	16.3	65.7	Week of 12/16/2018
PSA total		258	333,977	2058.3	177,844.0	4,801.3	19,574.0	333.0	1607.7	

Note: PSA development and residents counts tabulated as of December 31, 2017.

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TABLE 2 Descriptive characteristics and Citizen Complaint Review Board (CCRB) complaint history of Housing Bureau officers assigned to patrol in Police Service Area (PSA) commands.

Officers assigned		
	Number	**Percent**
PSA 1	236	12.3
PSA 2	311	16.2
PSA 3	243	12.7
PSA 4	150	7.8
PSA 5	215	11.2
PSA 6	147	7.7
PSA 7	218	11.4
PSA 8	178	9.3
PSA 9	218	11.4
Total	1,916	100
Rank		
Police officer	1,700	88.7
Sergeant	216	11.3
Gender		
Male	1,569	81.9
Female	347	18.1
Race		
Hispanic	693	36.2
White	658	34.3
Black	367	19.2
Asian/Pacific Islander	198	10.3
Age		
Mean	33.6	
Standard deviation	6.8	
Range	21–59	
Years on the Job		
Mean	6.8	
Standard deviation	5.5	
Range	<1–31	
CCRB complaint rate per year on the job		
Mean	0.18	
Standard deviation	0.27	
Range	0–3.2	
CCRB complaints during career		
No complaints	978	51
One or more complaints	938	49

Note: Based on December 31, 2017 snapshot.

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PSA commands yet to receive BWCs served as a controlled comparison group. At subsequent intervals (the "steps"), one PSA command was equipped with BWCs and thus crossed from the no-treatment controlled comparison group to the BWC treatment group. This process continued until all Housing Bureau officers in the nine PSAs were equipped with BWCs and thus crossed over to be the treatment group. At the end of the study, all PSA commands were equipped with BWCs. Data were collected throughout the period of BWC deployments, so that each PSA has data under both control (no use of BWCs) and treatment (use of BWCs) periods.

The cluster stepped-wedge quasi-experimental design helped to guard against treatment "contamination" across individual officers with PSAs. Officers with BWCs could 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). The diffusion of treatment into control conditions threatens the internal validity of the experiment by violating the stable unit treatment value assumption that the effect of a treatment on a given observational unit is independent of the intervention assignment to other observational units (Rubin, 1980). When PSAs crossed over from control to treatment conditions, BWCs were deployed on *all officers* in that command. Treating groups of officers, rather than individual officers, who work in distinct PSAs to have BWCs or not limits the contamination problem. NYPD PSAs largely function as smaller independent police departments that ensure precinct border integrity and limit Housing Bureau officers from crossing PSAs except in emergency circumstances.

2.1 | Outcomes

The Housing Bureau BWC evaluation measured the impact of BWC deployment using three sets of outcome measures: (1) civility of police-citizen interactions; (2) policing activity; and (3) police lawfulness. All outcome data were provided by the NYPD and CCRB as part of the *Davis et al.* settlement; human subject protections were monitored and ensured by the Southern District of New York, United States District Court, and the Institutional Review Board at the University of Pennsylvania. The evaluation of the deployment of BWCs on NYPD Housing Bureau officers was completely unobtrusive. No human subjects were recruited for or directly participated in the study. Human subjects that appeared in the acquired administrative data were not interviewed, observed, or asked to perform any direct research activities. Privacy was ensured by removing all personal identifiers shortly after receiving the official data from the NYPD and CCRB.

The evaluation examined the change in these outcome measures before and after the deployment of BWCs in each PSA. Two civility outcomes were included in the evaluation: Housing Bureau officer arrest reports listing use of force[7] and CCRB complaints against Housing Bureau officers between January 1, 2015 and December 31, 2019. Policing activity outcomes included counts of arrests (total arrests, trespass arrests, and disorder/obstruction arrests), summonses issued (total and disorder summonses), interior patrols,[8] officer-initiated calls, and stop reports made by Housing Bureau officers between January 1, 2015, and December 31, 2019.

To evaluate police lawfulness, the study compared stop reports from Housing Bureau Officers in PSAs before they were equipped with BWCs, with stop reports from Housing Bureau officers in PSAs after they were equipped with BWCs. The stop reports used for this evaluation were taken from the stop reports reviewed by the NYPD Monitor Team as part of its general monitorship compliance reviews.[9] The Monitor Team developed a two-prong sampling methodology that involved cluster random sampling and simple random sampling to select representative samples of NYPD stop reports beginning January 1, 2017, and continuing through December 31, 2019.

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The first sampling methodology involved stop reports audited by the NYPD's Quality Assurance Division (QAD), which is the NYPD unit that conducts audits of the Department's activities. Based on a statistical power analysis, the Monitor Team randomly selected commands from a list of 131 NYPD commands (clusters of stop reports that were inclusive of the 9 PSAs) each quarter until at least 300 stop reports were selected. This process yielded an annual sample of at least 1200 stop reports that were representative of citywide NYPD stops that was used to provide QAD auditors feedback on the accuracy of their internal lawfulness assessments. The second sampling methodology involved the selection of an annual simple random sample of stop reports from all stop reports recorded in NYCHA housing developments. The combined sampling methodologies generated 684 unique stop reports made by Housing Bureau officers between January 1, 2017, and December 31, 2019, including 298 stops made by officers without BWCs and 350 stop reports made by Housing Bureau officers equipped with BWCs. It is worth noting that the sampling procedure yielded 17.4% more stop reports (+52) for review when officers wore BWCs relative to when officers did not have BWCs. Given that the sampling procedure allowed all stop reports within each command to have an equal probability of selection over time, this suggests that Housing Bureau officers outfitted with BWCs during the study period filled out more stop reports than Housing Bureau officers without cameras.

The steps below describe the multistage evaluation process used by the Monitor Team to determine the lawfulness of stop reports in NYCHA Housing:

1. Two Monitor Team reviewers (Reviewers 1 and 2) independently assessed whether officer(s) articulated reasonable suspicion of a crime to make a lawful stop, provided reasonable suspicion that the subject was armed and dangerous to justify a frisk if made during the stop, and justified probable cause for a search if conducted during the stop.[10]
2. Reviewer 1 and 2 shared their independent assessments with each other and discussed their findings. When there were disagreements between raters, a third reviewer (Reviewer 3) independently assessed the lawfulness of the stop, frisk, and search described in the stop reports and then shared those results with Reviewers 1 and 2.
3. The Monitor and Deputy Monitor reviewed the stop reports with disagreements and then all five raters (Monitor, Deputy Monitor, and Reviewers 1, 2, and 3) discussed the stop reports and reached a conclusion on the lawfulness of the encounter described in each stop report.
4. The Monitor Team's list of legally insufficient stop reports was subsequently sent to the NYPD for review. The Monitor Team and representatives from the NYPD Risk Management Bureau (RMB) and QAD then met to discuss the identified unlawful stop reports.
5. After the Monitor Team meeting with RMB and QAD, the Monitor Team made a final assessment of the lawfulness of audited stops for that quarter.

The final lawfulness results for the sample of 684 stop reports were recorded in a computerized database and provided to the research team for subsequent analysis.

2.2 | Statistical models

Two complementary analytical approaches were used to estimate the impacts of BWCs on police activity among Housing Bureau officers. The first analysis used aggregate data collected at the PSA level to estimate the effects associated with the implementation of BWCs in the PSAs. The second analysis used data collected at the individual officer level to estimate the impact of BWC deployment at the officer level (within PSAs).

The units of analysis in the PSA command-level evaluation were "PSA-months" over a 5-year period. Monthly counts of outcome measures in each PSA were collected between January 1, 2015 and December 31, 2019 ($N = 9$ PSAs $\times$ 12 months $\times$ 5 years $=$ 540 PSA-months). Count data represent the number of times that an event occurs within a specific month in a given PSA.

Poisson regression models were used to estimate the effect of BWCs on the monthly counts of an outcome for each PSA command. A simplified version of the panel regression model (1) was estimated accodring to the following form:

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

where $i = 1\ldots 9$ PSA commands, with PSA i consisting of $t = 1, \ldots, n_i$ monthly observations, and Y_{it} is the outcome variable indicating the monthly count of an outcome in a specific PSA i during month-year t. The regressor *BWC* is a dummy variable identifying whether a PSA command adopted BWCs (1) or not (0) during the study period. The coefficient β_1 is the estimate of the direct effect of BWC treatment on the selected outcome measures. *Trend* represents a monthly (linear) covariate used to control for trends in specific outcomes, such as declines in arrests, stops, and summonses in the PSAs during this time period. The model also controlled for factors that change each month within PSAs that may be correlated with the outcome measures, such as the number of officers assigned to the PSAs monthly, citizen calls for service received in the PSAs, and major crime incidents reported in the PSAs, and β represents the vector of estimates of these attributes (X_{it}).

Fixed effects for PSA (α) and month (δ) were included to control for common trends to all PSAs in a given month and unmeasured differences between PSAs that were stable over the 5-year time period. The parameter estimates were expressed as incidence rate ratios (IRR) (i.e., exponentiated coefficients), or the ratio of change in the count. Standard errors are clustered at the PSA level to account for unmeasured dependence within PSAs over time and overdispersion (Berk & MacDonald, 2008). As a robustness check, all models were also estimated with negative binomial regressions. The results presented do not change. In other alternative specifications of model (1), we include the term $\gamma(BWC_i \times Month_t)$, which represents PSA-specific monthly time trends. The inclusion of PSA-specific monthly time trends enables an assessment of whether the findings hold after taking into account that each PSA is implementing cameras at different time points and may have diverging trends (Angrist & Pischke, 2009).

To assess how outcomes evolved in the months leading up to and after PSAs adopted BWCs, an event study regression model (2)was estimated as an expansion of model (1) according to the following form:

$$Y_{it} = \sum_{\substack{j=-12 \\ j \neq -1}}^{11} \theta_j 1(t = j) + \beta_2 Trend_t + \beta X_{it} + \alpha_i + \varepsilon_{it}. \quad (2)$$

The first set of parameters θ denotes indicators for each month relative to the implementation of BWCs in each PSA. We exclude the month before the adoption of cameras to serve as the reference period. The event study model controls for PSA fixed effects and other covariates reflected in the vector of X attributes (Steinberg et al., 2019; Stevenson & Wolfers, 2006). Robust standard errors were clustered by PSA to adjust for unmeasured dependence at the PSA level over time.

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For the officer-level analysis, the units of analysis in this evaluation were "officer-years" over a 4-year period. The NYPD provided the Monitor Team with data on Housing Bureau officers who worked in the PSAs at the time BWCs were deployed in each of the nine PSAs ($N = 1903$). Given temporal variations in BWC adoption dates across the nine PSAs, four 1-year outcome observation periods were created for each officer, normalized by the deployment date in each PSA (i.e., intervention year, −1 pre-intervention year, −2 pre-intervention year, and −3 pre-intervention year). However, given varying lengths of service, not all officers were observed for the entire 4-year study period. For instance, 62 officers (3.3%) were rookies with less than 1 year of service before BWC deployment; 190 officers (10.0%) had 1 prior service year; 190 officers (14.7%) had 2 prior service years; and the remainder, 1371 officers (72.0%) had 3 or more prior service years. The effective number of observations included in our evaluation was 6766 "officer-years."

Panel Poisson regression models were then used to analyze the annual change in CCRB complaint counts and other outcomes for each Housing Bureau officer before and after the BWCs were deployed in their respective PSA commands, controlling for other covariates. A simplified version of a panel regression model (3) was estimated according to the following form:

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

where $i = 1 \ldots n_i$ officers in $j = 1 \ldots 9$ PSA commands, $t = 1, \ldots, n_t$ yearly observations, and Y_{ijt} is the outcome variable indicating the count of CCRB complaints for an individual officer in a specific PSA i during year t. The regressor *BWC* is a dummy variable identifying whether a PSA command adopted BWCs (1) or not (0) in given a year ($t = 1$), whereas *Year* regressors are fixed-effects dummy variables (=1) identifying whether the officer-year CCRB complaint outcome was in the PSA in the three pre-intervention years t. The coefficient β_1 is the officer-specific effect of BWC during the treatment year, whereas θ are the officer-specific estimates from the 3 pre-intervention years. In this model, β represents the vector of estimates for covariates for each officer (X), such as age, sex, race, rank, and years on the job. Fixed effects for each PSA (α) are included to control for common trends to officers clustered in a given PSA. Robust standard errors were clustered at the individual-officer level.

The Housing Bureau BWC evaluation also had to account for the fact that circumstances of PSAs (contextual effects) on Housing Bureau officers could have varied over the course of the 4-year study period. For instance, Patrol Services Bureau officers in precincts surrounding and intersecting with the PSAs were outfitted with BWCs at varying times during the study period. As such, NYPD Housing Bureau officers could have received an indirect exposure to BWCs via their presence within the surrounding precinct prior to the deployment of BWCs in the Housing Bureau officers' PSAs. The presence of precinct officers with BWCs responding to calls for service in NYCHA housing developments, such as for providing backup for Housing Bureau officers, could also influence Housing Bureau officer and citizen behavior during these encounters. Thus, to account for potential PSA trajectory changes associated with indirect exposure to BWC treatment, we ran the same panel regression models with PSAs represented as random effects rather than fixed effects over the 4-year study period (Raudenbush & Bryk, 2002). In these random-effects models, standard errors were clustered by PSA.

For the analyses of the lawfulness outcomes, logistic regression models were used to estimate the impact of the BWCs on officer actions, controlling for characteristics of the stops. Robust standard errors clustered by PSA were used to control for unmeasured dependence within PSAs over time. Parameter estimates were expressed as odds ratios.

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3 | RESULTS

3.1 | PSA command level

Table 3 shows the results of the PSA command-level analysis. The findings suggest that the BWC intervention enhanced the civility of encounters between NYPD Housing Bureau officers and civilians. Controlling for the other variables, the BWC intervention was associated with a statistically significant 42.9% decrease ($p < 0.05$) in the monthly counts of CCRB complaints against officers. The BWC intervention was also associated with a statistically significant 15.6% decrease ($p < 0.05$) in the monthly number of arrests in which officers reported using force during the intervention period relative to the pre-intervention period. The results further suggest that the Housing Bureau officers increased the number of stop reports they filed and generally decreased their enforcement activities after being equipped with BWCs. The BWC intervention was associated with a statistically significant 47.5% increase ($p < 0.01$) in the monthly numbers of stop reports submitted by Housing Bureau officers during the intervention period relative to the pre-intervention period, holding other variables constant.

Controlling for preexisting decreases in enforcement actions in NYCHA developments and other covariates, the analyses found that the BWC intervention was associated with statistically significant decreases in monthly counts of total arrests (−34.3%, $p < 0.01$), trespass arrests (−30.9%, $p < 0.05$), interior patrols of NYCHA buildings (−30.4%, $p < 0.01$), and issuance of summonses for disorderly behavior (−60.7%, $p < 0.01$) during the intervention period relative to the pre-intervention period. However, the BWC intervention did not yield statistically significant changes in the monthly numbers of total summonses issued, officer-initiated calls for service, and disorder/obstruction arrests over the course of the study time-period.

Table 4 shows results from the alternative specifications of model (1) that include linear time trends (e.g., the term $\gamma PSA_i \times Month_t$). The inclusion of PSA-specific linear time trends enables one to assess if the main effects of BWCs hold after controlling for the potential for divergent trends across each PSA. The results generally confirmed the findings of the main effects models presented in Table 3. After controlling for PSA-specific time trends, the deployments of BWCs are associated with significant declines in monthly numbers of CCRB complaints, arrests, arrests with force, interior patrols, trespass arrests, and disorder summonses, as well as a statistically significant increase in monthly number of stop reports (all $p < 0.05$).

Figure 1 presents graphs visualizing the results for arrests. The figure displays the month-specific coefficients and 95% confidence intervals for the 12 months before and after BWC implementation. The figure shows that in the 6 months prior to BWC implementation, the arrest trends are parallel in 4 out of 6 months. The decrease in arrests during the pre-intervention period was part of a longer-term secular decline in NYPD Housing Bureau enforcement actions rather than a short-run anticipatory behavioral change by officers expecting to be outfitted with BWCs. NYPD Housing Bureau arrests declined by 73.4% from 37,039 in 2013 to 9854 in 2017 (the year before the 2018 BWC implementation). Table 5 presents the event study model month-specific coefficients for arrest outcomes during the 12 months before and after BWC deployment on Housing Bureau officers. The coefficients generally show that the monthly counts of arrests before BWC deployment were associated with increased numbers of arrests (IRR > 1), whereas the monthly counts of arrests after BWC implementation were associated with decreased numbers of arrests (IRR < 1).

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TABLE 3 Poisson regressions estimating body-worn camera (BWC) impact on monthly counts of selected outcomes in New York City Police Department (NYPD) Police Service Areas (PSAs).

	CCRB	Stops	Arrests	Arrests w/Force	Summonses	Interior patrols	Officer-initiated calls
BWC impact	0.574 (0.152)*	1.475 (0.186)**	0.657 (0.047)**	0.844 (0.067)*	1.001 (0.162)	0.696 (0.015)**	0.943 (0.052)
PSA 2	0.677 (00.209)	0.881 (0.288)	0.949 (0.150)	1.018 (0.263)	0.363 (0.153)*	0.691 (0.108)*	0.858 (0.102)
PSA 3	0.462 (0.049)**	0.963 (0.092)	1.049 (0.045)	1.133 (0.093)	1.390 (0.152)**	0.960 (0.065)	0.951 (0.065)
PSA 4	1.845 (0.804)	0.949 (0.342)	1.943 (0.313)**	1.019 (0.289)	1.125 (0.481)	1.140 (0.157)	0.746 (0.127)
PSA 5	0.871 (0.074)	1.322 (0.119)**	1.898 (0.048)**	1.267 (0.055)**	1.377 (0.053)**	0.970 (0.058)	0.924 (0.095)
PSA 6	1.240 (0.567)	0.820 (0.312)	1.407 (0.247)	0.821 (0.253)	1.134 (0.522)	1.211 (0.181)	0.771 (0.132)
PSA 7	1.681 (0.095)*	2.039 (0.161)**	2.273 (0.052)**	1.141 (0.055)**	2.567 (0.086)**	1.005 (0.053)	0.884 (0.076)
PSA 8	1.175 (0.352)	1.454 (0.371)	1.841 (0.201)**	1.087 (0.206)	2.280 (0.656)**	1.129 (0.112)	0.802 (0.113)
PSA 9	0.750 (0.243)	0.5669 (0.101)**	0.918 (0.049)	0.599 (0.079)**	1.895 (0.328)**	0.975 (0.067)	1.124 (0.147)
Trend	1.007 (0.006)	0.969 (0.006)**	0.989 (0.002)**	0.984 (0.004)**	0.956 (0.006)**	1.018 (0.001)**	0.997 (0.002)
Officers	1.009 (0.004)*	0.997 (0.004)	1.003 (0.002)	1.003 (0.003)	1.005 (0.005)	1.004 (0.002)*	0.999 (0.001)
Calls for service	0.999 (0.001)	1.001 (0.001)*	1.001 (0.001)**	1.001 (0.001)**	1.001 (0.001)	1.001 (0.001)**	1.001 (0.001)**
Major crime incidents	1.003 (0.009)	0.994 (0.002)*	1.006 (0.001)**	1.007 (0.003)	1.012 (0.002)	0.999 (0.001)	1.001 (0.001)
Constant	1.004 (1.143)	42.594 (42.573)**	40.374 (18.297)**	0.937 (0.686)	44.557 (55.287)**	436.522 (172.046)**	830.64 (265.128)**
Log pseudolikelihood	−2231.331	−2498.676	−3587.439	−991.353	−9099.457	−62,584.118	−10,807.417
Pseudo R^2	0.106	0.365	0.668	0.118	0.689	0.756	0.910
N	540	540	540	540	540	540	540

(Continues)

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TABLE 3 (Continued)

	Trespass arrests	Disorder/Obstruct arrests	Disorder summonses
BWC Impact	0.691 (0.127)*	0.977 (0.279)	0.393 (0.118)**
PSA 2	0.492 (0.135)*	0.803 (0.474)	0.439 (0.192)
PSA 3	0.846 (0.070)*	0.991 (0.185)	1.076 (0.129)
PSA 4	1.774 (0.334)**	0.989 (0.510)	0.332 (0.132)**
PSA 5	1.312 (0.101)**	1.117 (0.096)	0.416 (0.022)**
PSA 6	0.717 (0.143)	0.507 (0.291)	0.216 (0.093)**
PSA 7	1.497 (0.098)**	0.771 (0.076)**	2.091 (0.129)**
PSA 8	0.791 (0.105)	0.662 (0.225)	1.049 (0.275)
PSA 9	0.269 (0.023)**	0.509 (0.087)**	0.837 (0.163)
Trend	0.979 (0.003)**	0.987 (0.006)	0.968 (0.007)**
Officers	0.999 (0.003)	0.998 (0.007)	995 (0.005)
Calls for service	1.001 (0.001)	1.001 (0.001)**	0.999 (0.001)
Major crime incidents	0.997 (0.002)	1.005 (0.005)	1.014 (0.006)*
Constant	40.438 (20.441)**	1.365 (2.102)	44.658 (54.734)**
Log pseudolikelihood	−2640.481	−823.983	−2179.812
Pseudo R^2	0.489	0.061	0.592
N	540	540	540

Notes: PSA 1 is the reference category for the PSA fixed-effects dummy variables. Estimates reported as incidence rate ratios. Robust standard errors clustered by PSA reported in parentheses. Month fixed-effects variables included but not shown.
*$p < 0.05$. **$p < 0.01$.

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TABLE 4 Body-worn camera (BWC) effects on selected outcomes controlling for Police Service Area (PSA)-specific linear time trends.

Outcome variables	IRR
CCRB	0.554 (0.134)*
Stops	1.358 (0.142)**
Arrests	0.631 (0.041)**
Arrests w/Force	0.804 (0.060)**
Summons	0.848 (0.106)
Interior patrols	0.693 (0.016)**
Officer-initiated calls	0.933 (0.054)
Trespass arrests	0.648 (0.124)
Disorder arrests	0.624 (0.327)
Obstruction arrests	1.087(0.276)
Disorder summonses	0.364 (0.106)**

Notes: $N = 540$ for each model. Robust standard errors clustered by PSA reported in parentheses. PSA fixed effects, month fixed effects, monthly linear trends, officers, calls for service, and major crime incidents included but not shown.
Abbreviations: CCRB, Citizen Complaint Review Board; IRR, incidence rate ratio.
*$p < 0.05$. **$p < 0.01$.

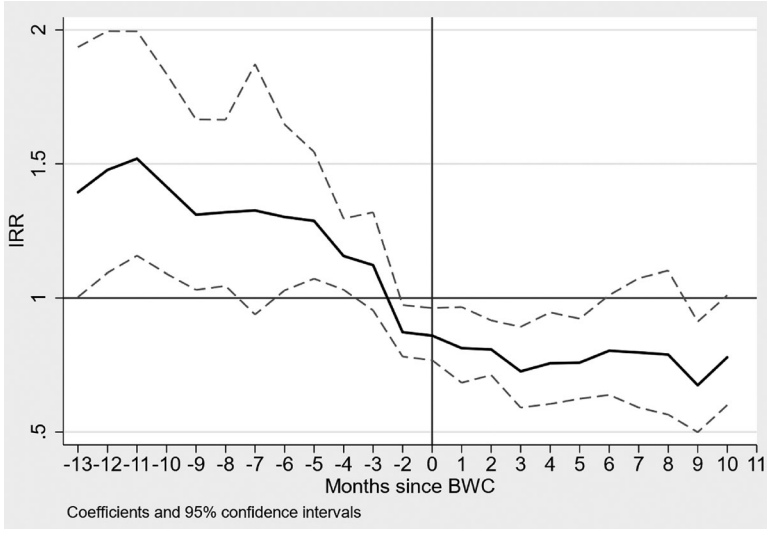

FIGURE 1 Event study model of arrests before and after body-worn camera (BWC) deployment.

3.2 | PSA officer level

Table 6 presents the results of the Housing Bureau officer-level panel regression of outcomes, controlling for individual officer characteristics. The results suggest that outfitting Housing Bureau officers with BWCs improved the civility of police-civilian encounters and changed the officers' policing activities. Controlling for study year, PSA assigned, and officer characteristics, Housing Bureau officers generated 42.3% fewer CCRB complaints ($p < 0.01$) and reported using force in 20.3% fewer arrests ($p < 0.01$) during the BWC year relative to pre-intervention years. Housing

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TABLE 5 Event study model estimating body-worn camera (BWC) effects on month arrest counts, 12 months before and after BWC deployment.

	IRR
Month −12	1.418 (0.237)
Month −11	1.514** (0.233)
Month −10	1.513** (0.215)
Month −9	1.452** (0.189)
Month −8	1.342 (0.163)
Month −7	1.331 (0.157)
Month −6	1.331 (0.236)
Month −5	1.310 (0.159)
Month −4	1.308** (0.126)
Month −3	1.183** (0.070)
Month −2	1.134 (0.096)
Month 0	0.914 (0.056)
Month +1	0.875 (0.053)
Month +2	0.811 (0.072)
Month +3	0.828** (0.056)
Month +4	0.723** (0.073)
Month +5	0.771 (0.088)
Month +6	0.788 (0.084)
Month +7	0.804 (0.094)
Month +8	0.807 (0.125)
Month +9	0.820 (0.139)
Month +10	0.698 (0.108)
Month +11	0.781 (0.100)

Notes: Robust standard errors clustered by PSA reported in parentheses. Each row reflects the monthly estimate prior to and after implementation of BWC (−1 month is reference). PSA fixed effects, month fixed effects, officers, calls for service, and major crime incidents included but not shown.

Abbreviation: IRR, incidence rate ratio.

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

Bureau officers made 23.3% fewer total arrests ($p < 0.01$) and 23.2% fewer trespassing arrests ($p < 0.01$) during the year they were equipped with BWCs relative to prior years. Summonses issued by Housing Bureau officers increased by 8% ($p < 0.01$) during the BWC year when compared to pre-intervention years, controlling for other covariates.[11] However, disorder summonses issued by Housing Bureau officers dropped by 13.1% ($p < 0.01$) during the year they wore BWCs relative to prior years. Finally, consistent with the result of the PSA level analysis, the number of stop reports submitted by Housing Bureau officers increased sharply by 68.1% ($p < 0.01$) during the BWC year relative to pre-intervention years.

Table 7 presents the results of the Housing Bureau officer-level panel regression of outcomes, with PSA covariates specified as random effects as opposed to fixed effects, controlling for individual officer characteristics. The PSA random-effects specification did not change the findings that equipping Housing Bureau officers with BWCs improved the civility of police-citizen outcomes and decreased their policing enforcement activities. Indeed, the direction, size, and statistical

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TABLE 6 Body-worn camera (BWC) effects on study outcomes, by year, Police Service Area (PSA), and officer characteristics.

Variables	(1) CCRB	(2) Stops	(3) Arrests	(4) Force arrests	(5) Summonses	(6) Trespass arrests	(7) Disorder sum-monses
BWC intervention year +1	0.557**	1.681**	0.767**	0.797**	1.080**	0.768**	0.869**
	(0.024)	(0.048)	(0.005)	(0.061)	(0.010)	(0.018)	(0.021)
Pre-intervention year −3	1.039	1.503**	1.824**	2.779**	4.628**	3.200**	4.244**
	(0.061)	(0.055)	(0.016)	(0.175)	(0.049)	(0.104)	(0.128)
Pre-intervention year −2	1.020	1.254**	1.615**	1.601**	2.359**	1.934**	1.697**
	(0.046)	(0.041)	(0.015)	(0.135)	(0.027)	(0.050)	(0.051)
PSA 2	1.245	0.478**	0.888**	0.658	0.884**	1.015	0.839*
	(0.162)	(0.051)	(0.034)	(0.162)	(0.034)	(0.084)	(0.063)
PSA 3	0.620**	0.833*	1.034	1.227	1.039	1.228*	1.057
	(0.102)	(0.074)	(0.043)	(0.289)	(0.043)	(0.110)	(0.085)
PSA 4	1.831**	1.315**	1.903**	2.952**	1.869**	2.673**	2.596**
	(0.237)	(0.109)	(0.087)	(0.683)	(0.087)	(0.256)	(0.188)
PSA 5	1.110	1.095	1.379**	1.355	1.350**	1.746**	1.658**
	(0.157)	(0.088)	(0.056)	(0.336)	(0.057)	(0.158)	(0.126)
PSA 6	1.113	1.446**	1.869**	3.446**	1.842**	2.478**	2.554**
	(0.184)	(0.116)	(0.095)	(0.771)	(0.095)	(0.259)	(0.206)
PSA 7	1.361*	0.939	1.860**	2.440**	1.822**	2.318**	2.553**
	(0.193)	(0.084)	(0.078)	(0.503)	(0.078)	(0.198)	(0.182)
PSA 8	1.020	1.150	1.392**	1.410	1.392**	1.763**	1.720**
	(0.158)	(0.091)	(0.065)	(0.340)	(0.065)	(0.170)	(0.138)
PSA 9	0.843	0.768**	0.862**	0.884	0.873**	0.951	0.837*
	(0.125)	(0.071)	(0.036)	(0.242)	(0.037)	(0.089)	(0.071)
Age	0.969**	1.001	0.994*	0.965*	0.994*	0.999	0.995
	(0.009)	(0.006)	(0.003)	(0.016)	(0.003)	(0.006)	(0.005)
Years on the job	1.024	0.939**	0.972**	0.915**	0.971**	0.952**	0.954**
	(0.013)	(0.008)	(0.003)	(0.021)	(0.003)	(0.007)	(0.006)
Sergeant	1.108	1.187*	0.922*	0.772	0.919*	1.071	0.889*
	(0.135)	(0.098)	(0.032)	(0.209)	(0.032)	(0.077)	(0.052)
Black	1.043	0.947	1.021	1.324	1.021	0.971	0.980
	(0.106)	(0.066)	(0.032)	(0.234)	(0.032)	(0.061)	(0.052)
Hispanic	1.074	0.970	0.986	0.959	0.987	1.064	0.968
	(0.091)	(0.053)	(0.026)	(0.125)	(0.026)	(0.059)	(0.042)
Asian/other	0.972	0.989	1.029	1.277	1.020	1.125	1.025
	(0.125)	(0.074)	(0.041)	(0.241)	(0.041)	(0.094)	(0.068)
Female	0.763**	0.957	0.959	0.829	0.952	0.928	0.905*
	(0.073)	(0.059)	(0.026)	(0.138)	(0.025)	(0.057)	(0.041)
Observations	6766	6766	6766	6766	6766	6766	6766
Number of officers	1903	1903	1903	1903	1903	1903	1903
Clusters	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Estimates reported as incidence rate ratios. Robust standard errors clustered by PSA reported in parentheses. Pre-intervention year −1 is the reference year. PSA 1 is the reference PSA.

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

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TABLE 7 Body-worn camera (BWC) effects on study outcomes by year and officer characteristics, Police Service Area (PSA); random-effects models.

Outcome	IRR
CCRB	0.552 (0.024)**
Stops	1.677 (0.038)**
Arrests	0.764 (0.015)**
Arrests w/Force	0.778 (0.094)*
Summonses	1.077 (0.027)**
Trespass arrests	0.768 (0.036)**
Disorder summonses	0.865 (0.063)*

Notes: Robust standard errors clustered by PSA reported in parentheses. Pre-intervention year and officer covariates included but not shown.

Abbreviations: CCRB, Citizen Complaint Review Board; IRR, incidence rate ratio.

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

significance of all BWC intervention-year outcomes remained almost the same as the effects estimated in the officer-level models with PSA fixed effects.

3.3 | Lawfulness of officer stops

Table 8 presents a comparison of the gender, race/ethnicity, suspected crime, officer actions, how the encounter was generated (radio run, officer-initiated, or direct contact from victim/witness), and the lawfulness of the officer's actions for stop reports of officers without BWCs relative to the stop reports of officers with BWCs. In both groups, stopped civilians tended to be younger Black and Hispanic males suspected of a range of crimes. Stopped individuals were more likely to be Black non-Hispanic subjects (70.9% vs. 61.7%, respectively, $p < 0.05$) and less likely to be White Hispanic subjects (15.4% vs. 25.8%, respectively, $p < 0.05$) during the BWC intervention period relative to the period before BWC deployment. Housing Bureau officers in the stop reports were more likely to be mobilized through radio runs (50.0% vs. 28.9%, respectively, $p < 0.05$) and less likely to conduct officer-initiated encounters (41.1% vs. 63.4%, respectively, $p < 0.05$) during the BWC intervention period relative to the period before BWC deployment. Moreover, Housing Bureau officers were more likely to stop civilians on suspected violent crimes (16.4% vs. 27.4%, respectively, $p < 0.05$) and other/unknown crimes (2.3% vs. 5.4%, respectively, $p < 0.05$), and less likely to stop civilians based on disorder offenses (41.3% vs. 22.6%, respectively, $p < 0.05$) and drug offenses (8.4% vs. 4.6%, respectively, $p < 0.05$) during the BWC intervention period relative to the period before BWC deployment.

Individuals were frisked and issued summonses in similar percentages of pre-intervention and BWC-intervention stops made by Housing Bureau officers. However, civilians in BWC intervention stops relative to pre-intervention stops were much less likely to be searched (30.6% vs. 47.0%, respectively, $p < 0.05$) and arrested (33.4% vs. 49.3%, respectively, $p < 0.05$). Table 8 also presents the results of the stop report lawfulness audits conducted by the Monitor Team. Overall, the justifications reported for stopping civilians were a little more likely to be regarded as lawful in the BWC intervention stop reports relative to the pre-intervention stop reports (67.1% vs. 58.1%, respectively, $p < 0.05$). Of the 55.8% of stop reports that involved a frisk ($N = 362$ of 648), the justifications reported for frisking civilians in Housing Bureau officer stop reports during the BWC intervention

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TABLE 8 Comparison of stop report characteristics during pre-intervention and body-worn camera (BWC) intervention periods.

	Pre-intervention		Intervention		Std. mean difference
	N	%	*N*	%	
Male	251	84.2%	311	88.9%	0.068
Female	46	15.4%	36	10.3%	
Missing	1	0.3%	3	0.8%	
Black non-Hispanic	184	61.7%	248	70.9%	0.096*
White Hispanic	77	25.8%	54	15.4%	−0.129*
Black Hispanic	21	7.0%	28	8.0%	0.018
White non-Hispanic	10	3.4%	11	3.1%	−0.006
Asian/other/missing	6	2.0%	9	2.6%	0.018
Mean age (SD)	273	26.5 (11.7)	316	28.0 (12.1)	0.065
Mobilization					
Radio run	86	28.9%	175	50.0%	0.215*
Officer-initiated	189	63.4%	144	41.1%	−0.222*
Complainant/witness	23	7.7%	31	8.9%	0.021
Suspected crime					
Violent	49	16.4%	96	27.4%	0.131*
Weapon	78	26.2%	114	32.6%	0.070
Property	16	5.4%	26	7.4%	0.042
Disorder	123	41.3%	79	22.6%	−0.201*
Drug	25	8.4%	16	4.6%	−0.078*
Other/unknown	7	2.3%	19	5.4%	0.078*
Officer action outcomes					
Monitor—Lawful stop	173	58.1%	235	67.1%	0.094*
Frisked suspect	156	52.3%	206	58.9%	0.065
Monitor-Lawful frisk	139	89.1%	179	86.9%	−0.033
Searched suspect	140	47.0%	107	30.6%	−0.168*
Monitor-Lawful search	136	97.1%	97	90.7%	−0.146*
Arrested suspect	147	49.3%	117	33.4%	−0.161*
Issued summons	10	3.4%	13	3.7%	0.010

Note: Total $N = 648$ (pre-intervention $N = 298$, intervention $N = 350$).
*$p < 0.05$.

period were not significantly more or less likely to be regarded as constitutional (satisfying reasonable suspicion requirements) when compared to Housing Bureau officer stop reports during the pre-intervention period. In the 38.1% of stop reports that reported a search ($N = 247$ of 648), the justifications reported for searching civilians in stop reports by Housing Bureau officers equipped with BWCs were less likely to be lawful (meeting probably cause requirements) when compared to stop reports by Housing Bureau officers who were not equipped with BWCs (90.7% vs. 97.1%, respectively, $p < 0.05$).

Table 9 presents the results of the multivariate logistic regressions of Housing Bureau officer action outcomes on whether the stop report was completed during the BWC intervention

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TABLE 9 Multivariate logistic regressions of body-worn camera (BWC) impact on officer action outcomes, controlling for stop characteristics.

	Frisked	Searched	Arrested/ Summonsed	Lawful stop	Lawful frisk/search
BWC treatment	0.890 (0.203)	0.534 (0.124)*	0.602 (0.111)*	1.442 (0.337)	0.581 (0.118)*
Marginal effect:					
BWC intervention	0.570	0.300	0.384	0.683	0.703
Pre-period	0.659	0.503	0.601	0.571	0.781
Male	2.165 (1.287)	0.953 (0.299)	0.989 (0.337)	1.562 (0.483)	1.379 (0.584)
Black non-Hispanic	1.878 (0.853)	0.834 (0.375)	0.478 (0.292)	1.974 (0.899)	1.151 (0.326)
White Hispanic	1.623 (0.822)	0.870 (0.437)	0.561 (0.287)	1.237 (0.563)	0.955 (0.467)
Black Hispanic	1.278 (0.791)	1.345 (0.648)	1.390 (0.767)	1.257 (0.530)	0.567 (0.334)
Asian/other	0.932(0.682)	0.380 (0.208)	0.647 (0.378)	0.552 (0.533)	–
Age	0.984 (0.008)*	1.028 (0.012)*	1.033 (0.015)*	1.026 (0.007)*	1.022 (0.008)*
Radio run	1.245 (0.539)	2.596 (0.723)*	1.610 (0.835)	0.538 (0.305)	1.078 (0.501)
Officer-initiated	0.682 (0.209)	2.536 (1.118)*	1.759 (1.001)	0.425 (0.251)	0.802 (0.388)
Violent	0.855 (0.369)	0.365 (0.277)	0.963 (0.614)	3.071 (1.755)*	1.236 (0.709)
Property	0.702 (0.297)	0.660 (0.449)	1.024 (0.619)	1.971 (0.948)	1.092 (0.814)
Drug	0.997 (0.637)	0.531 (0.435)	1.354 (1.306)	11.472 (9.454)*	1.091 (0.645)
Weapon	12.824 (5.408)*	1.832 (1.337)	7.317 (5.544)*	1.294 (0.769)	9.653 (5.274)*
Disorder	0.417 (0.155)*	0.383 (0.272)	0.571 (0.401)	1.948 (1.157)	2.415 (1.844)
Constant	0.608 (0.618)	0.298 (0.296)	0.595 (0.382)	0.280 (0.204)	0.579 (0.690)
Log pseudolikelihood	−293.885	−338.981	−305.032	−361.239	−317.242
Pseudo R^2	0.2718	0.147	0.251	0.079	0.113
N	589	589	589	589	438

Notes: Odds ratio reported. Robust standard errors clustered by PSA reported in parentheses. Female was reference category for the male covariate. White non-Hispanic suspect was the reference category for the other race covariates. Complainant/witness-initiated stop was the reference category for the mobilization covariates. Other and unknown suspected crime was the reference category for the suspected crime type categories. The Asian dummy variable was omitted from the Lawful Frisk/Search regression because there were zero applicable cases.
*$p < 0.05$.

period relative to the pre-intervention period, controlling for stop characteristics. Due to the small number of stop reports involving the issuance of summonses, the arrest and summons officer action outcomes were collapsed into one binary variable (1 = arrested/summonsed, 0 = not arrested/summonsed) in the multivariate logistic regression assessing the likelihood of an arrest/summons outcome in the stop reports. Further, because of the small number of stop reports involving searches, the frisk and search officer action outcomes were also collapsed into one binary variable (1 = frisked/searched, 0 = not frisked/searched) in the multivariate logistic regression assessing the likelihood that the Monitor Team judged that the frisks/searches conducted during the stops were lawful. The results show that for four out of the five outcomes, there was a significant reduction in the percentage of stops with searches and arrests or summonses during the BWC intervention period relative to the pre-period.

Individuals stopped by Housing Bureau officers outfitted with BWCs were not more or less likely to be frisked but were much less likely to be searched and arrested/summonsed relative to individuals stopped by officers before they were outfitted with cameras. Controlling for stop characteristics, BWC intervention period stop reports were associated with a statistically significant 46.6% decrease ($p < 0.05$) in the odds that a search was conducted compared to pre-intervention

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stop reports. Holding the other covariates constant, the predicted marginal effect of equipping officers with BWCs suggests that 30.0% of BWC intervention stops involved a search, whereas 50.3% of the pre-intervention stops involved a search. Further, relative to pre-intervention stop reports, stop reports prepared after BWC deployment were associated with a statistically significant 39.8% decrease ($p < 0.05$) in the odds that a subject was arrested/summonsed relative to the pre-intervention period, holding the other covariates constant. The predicted marginal effect of equipping officers with BWCs suggests that 38.4% of BWC intervention stops resulted in an arrest/issued summons, whereas 60.1% of pre-intervention stops resulted in an arrest/issued summons, controlling for the other variables.

Table 9 also presents the results of the multivariate logistic regressions of the impact of BWC intervention on Monitor Team assessments of the lawfulness of stops, frisks, and searches in the stops, controlling for stop characteristics. Although the effects of BWCs on whether the stop was judged to be lawful by the Monitor Team were in the same positive direction as the bivariate comparison in Table 9, the results were no longer statistically significant when stop characteristics were included in the multivariate analysis. However, relative to pre-intervention stop reports, stop reports prepared after BWC deployment were associated with a statistically significant 41.9% decrease ($p < 0.05$) in the odds that a frisk/search conducted during a stop was assessed as constitutional relative to the pre-intervention stops involving a frisk/search, holding the other covariates constant. The predicted marginal effect of the placement of BWCs on Housing Bureau officers shows that 70.3% of BWC intervention frisks and searches met a lawfulness standard compared to 78.1% of the pre-intervention frisks and searches, holding other variables constant.

4 | CONCLUSION

This study is the first to measure the effects of placing BWCs on police officers working in public housing environments. The available evaluation literature appraising officers working in more general street assignments suggests that BWCs do seem to reduce civilian complaints against officers (Lum et al., 2020), and a recent review suggests that there are significant reductions in officer force when police are equipped with BWCs (Williams et al., 2021). In both the PSA command- and officer-level analyses, our results suggest significant reductions in CCRB complaints made against NYPD Housing Bureau officers and in the use of force during arrests after the deployment of BWCs to officers working in public housing areas. For officers working in public housing settings, increased civility could generate collateral benefits, such as fewer injuries to civilians and officers and reduced civil litigation. Police legitimacy may also be enhanced as reduced citizen complaints and diminished use of force during arrests suggest that BWCs may inspire procedurally just police behaviors during encounters with citizens (see also Demir, Apel, et al., 2020).

The study also suggests significant reductions in total arrests, trespass arrests, and disorder summonses during the years that BWCs were deployed to officers in public housing areas relative to pre-implementation years in both sets of analyses. In the PSA command-level analysis, the Housing Bureau BWC evaluation also found that officers conducted 30% fewer interior patrols after the deployment of BWCs. Equipping Housing Bureau officers with BWCs did not alter the number of officer-initiated calls for service in the public housing areas, suggesting that there was not an overall decline in police activity. Nevertheless, the overall weight of the evidence on policing activity outcomes suggests that equipping Housing Bureau officers with BWCs further decreased the downward trajectory of enforcement activity in NYCHA housing. These results suggest that the deployment of BWCs on housing officers in the context of court-mandated polic-

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ing reforms can reduce the harms associated with the long history of very high levels of police surveillance and enforcement actions taken against public housing residents, their visitors, and neighbors from surrounding communities in New York City and other urban areas.

The number of stop reports submitted by Housing Bureau officers, however, increased dramatically in the year after BWC deployment relative to pre-deployment years (+48% PSA command-level analysis, +68% officer-level analysis). Further, the analysis of randomly selected stop reports suggests that stops were more likely to involve Black subjects, less likely to originate from officer-initiated activities, and more likely to involve violent crimes during the BWC deployment year. The results suggest a possible shift in stop report activity—increased stop reports were more focused on substantive crime activities and not generated by officer-initiated enforcement on more subjective crime categories such as disorderly behavior and drug sales. Stops made by Housing Bureau officers after BWC deployment were also less likely to involve an arrest or summons, less likely to involve a search, and those stops with searches were less likely to be judged as lawful relative to stops with searches made before BWC implementation. Finally, it is worth noting that the increase in the number of stop reports did not correspond to a decrease in the proportion of those stops found to be legally sufficient.

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

The elevated reporting of citizen stops involving less serious circumstances suggests that officers may be deterred from failing to document stops when wearing BWCs. NYPD policy requires officers to activate BWCs during all pedestrian stops, and officers must document these encounters by filing stop reports. Unfortunately, the NYPD Monitor has consistently observed that NYPD officers were not documenting all their stops and the underreporting of stops limits assessments to determine whether the NYPD is in compliance with the reforms required by the *Davis*, *Ligon*, and *Floyd* settlements.[12] The NYPD analyzes call, incident, and arrest data to determine whether it seems likely that police-citizen encounters should have been documented via stop reports. PSA commanders are notified of potentially missing stop reports for Housing Bureau officers under their supervision. The availability of BWC video for specific encounters increases the likelihood that PSA commanders will detect unreported stops. Although it is not known whether officers were in fact disciplined for failing to submit a stop report, either before or after officers were equipped with BWCs, the failure to submit a stop report can subject an officer to a disciplinary violation. The presence of revealing video therefore likely increases officer perceptions that policy violations would be detected, given that the video decreases the need for supervisors to locate and interview people involved in the encounter. As such, the increase in citizen stops suggests that NYPD Housing Bureau officers changed their reporting behavior due to the presence of BWCs supported by an existing compliance system.

It is obviously concerning that NYPD officers continue to make some stops that involve unlawful searches of citizens (see also Braga et al., 2022). However, the finding that the deployment

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of BWCs on Housing Bureau officers resulted in the documentation of larger numbers of stops relative to pre-deployment is fundamentally good news. Put simply, if the NYPD is not aware that a problem exists, they are not able to remedy the underlying conditions that cause the problem to persist. The deployment of BWCs on Housing Bureau officers not only appears to increase their compliance with NYPD directives to document all stops, but it also provides the Department with an important opportunity to intervene and monitor their progress toward ensuring constitutional policing. This observation is particularly important for policing in public housing contexts as prior analyses documented patterns of racially selective enforcement that suggest systemic violations of the Fourteenth Amendment's prohibition on racial discrimination (Fagan et al., 2012).

The findings of this study are limited by the research design used. Most importantly, the BWC implementation occurred when there were large declines in NYPD enforcement activity in and around NYCHA housing buildings. The controlled trial (quasi-experimental) design of the study measured the impact of BWC deployment on Housing Bureau officers controlling for preexisting existing trends and other factors. The findings from the PSA command-level and officer-level analyses generally complemented each other and suggest that the BWC deployment produced significant changes in the civility of police-civilian encounters, police enforcement activity, and the reporting of civilian stops. However, the results presented here are limited by the completeness and quality of the data provided by the NYPD. The analyses of stop report narratives represent what officers chose to document and may not be reflective of the events as they actually transpired. Further, the NYPD only provided counts of the use of force during arrests rather than incident-level information on arrests that did or did not involve the use of force. As such, we were not able to determine whether BWC deployment was associated with a reduction in the likelihood that force was used during arrest events. Our analyses simply show that the frequency of arrests with force declined and cannot distinguish whether the observed decrease was simply a by-product of a general reduction in arrests.

Our evaluation was not a randomized experiment and, as a result, presents limited causal evidence of the impact of BWC deployment on Housing Bureau officer activities. Other limitations of our design include the absence of a pure control group, the brief timeframe covering the changeover of all PSAs to the use of BWCs (which may limit the utility of the case-crossover design), and the relatively short post-BWC follow-up period. Finally, the analyses suggest that procedural justice may have been enhanced by the presence of BWCs during Housing Bureau officer encounters with citizens as evidenced by reduced complaints against officers and diminished use of force by officers when making arrests. However, it is important to note that this study did not directly measure whether citizens perceived police behavior during encounters to be procedurally just before and after BWCs were deployed in the PSAs. As such, conclusions that BWCs improved procedural justice are speculative.

Despite these limitations, the evaluation findings suggest that BWCs, when deployed in the context of a court-mandated police reform regime, may help mitigate the harms generated by the "public housing to prison pipeline" that disproportionately incarcerates residents of Black and Hispanic neighborhoods with public housing present (Holder et al., 2022). Recent studies suggest that police officers are responsive to managerial directives (Mummolo, 2018; Rivera & Ba, 2023). Supervisory review of BWC footage also provides an opportunity to strengthen these management directives by identifying and addressing unlawful police actions during encounters with the public (White & Fradella, 2016). This study finds that the placement of BWCs on officers working in public housing settings can increase their compliance with department directives to reduce excessive enforcement actions and increase their documentation of stops with citizens. Coupled with videos of the encounters, the data generated by increased documentation of stops can be used

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to determine whether officers are obeying the law during their interactions with citizens. BWCs can be a very useful tool to reduce ongoing problems with excessive and unlawful stops in public housing areas. For BWCs to provide a useful tool to improve constitutional policing requires that police agencies be vigilant in reviewing footage and providing feedback to officers on appropriate use of discretion in deciding who to stop, the quality of police and civilian interactions, and the legality of arrests.

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CONFLICT OF INTEREST

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.

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ENDNOTES

[1] New York State law regulates police conduct more rigorously than federal law. 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] For instance, NYPD Housing Bureau officers were trained to understand the difference between mere "presence" in NYCHA areas (which is not a credible reason for approach a citizen) and "lingering" (presence in an NYCHA area for an unreasonable time period given the area's intended purpose), how to approach lingering persons to ask questions in a non-threatening and non-accusatory manner as to whether they have the authority to be in that area (Level 1 encounter), and how to lawfully conduct Level 2 (accusatory questions asked) and Level 3 encounters (official stop where the subject is not free to leave; a frisk and/or search may be conducted). See https://www.nypdmonitor.org/wp-content/uploads/2022/09/01-Recommendation-Regarding-Training-Materials-for-Housing-Bureau-Members.pdf (accessed 6/25/2024).

[3] https://www.nypdmonitor.org/wp-content/uploads/2022/09/15-Fifteenth-Report.pdf (accessed 4/16/2023).

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

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

[6] NYPD BWC policy specifies a variety of situations when 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. The NYPD monitored the deployment of BWC on its PSA officers and ensured that all officers wore cameras during their shifts over the course of the evaluation period. Unfortunately, the research team did not have access to data on the prevalence of interactions in which PSA officers failed to activate their cameras.

[7] NYPD policy states, "force may be used when it is reasonable to ensure the safety of a member of the service or a third person, or otherwise protect life, or when it is reasonable to place a person in custody or to prevent escape from custody" and further suggests that officers "will use only the reasonable force necessary to gain control or custody of a subject." Force options include physical force, less-lethal options (e.g., OC spray,

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conducted electrical weapons, or impact weapons), even deadly physical force, when justified by the threat of violence. See https://www.nyc.gov/assets/nypd/downloads/pdf/use-of-force/use-of-force-2021.pdf (accessed May 3, 2024). During the study period, NYPD officers were required to indicate whether any force was used when making arrests of suspected offenders on all arrest reports.

[8] Interior patrols (also known as vertical patrols) are used by NYPD Housing Bureau officers to control criminal and disorderly activity in the stairwells and hallways of NYCHA housing developments. Interior patrols are controversial. Indeed, concern over the use of this tactic increased after the tragic killing of Akai Gurley, a 28-year-old black male, by an NYPD officer on interior patrol in the NYCHA Louis H. Pink Houses in East New York, Brooklyn in November 2014. *Davis et al.* plaintiffs alleged that interior patrols were associated with increased stops and frisks of NYCHA residents and their guests. The settlement required officers to be trained in the proper manner of performing interior patrols that respects the rights of NYCHA residents and guests. As such, the number of interior patrols was included as an outcome measure in the PSA-level analyses to determine whether the BWC deployment was associated with changes in Housing Bureau officers' use of this controversial tactic.

[9] The independent NYPD Monitor was appointed by the U.S. District Court for the Southern District of New York to ensure that the NYPD's policing practices related to stops, frisks, and searches comply with the law. The Monitor Team reports to the court on the NYPD's progress (or lack of progress) on implementing the court-ordered reforms in the *Davis*, *Ligon*, and *Floyd* settlements. Stop report reviewers were lawyers with constitutional law expertise and former police executives with extensive experience in training officers in constitutional rules of law that apply to policing.

[10] Reviewers required that officers articulated specific facts that were corroborated and individualized to the person encountered to support that the stop was justified based on the reasonable suspicion of a crime. General statements such as the encounter occurred in a "high crime area" or personal characteristics such as age, race, and sex were insufficient to justify a stop. The stop narratives needed to include greater details, including fulsome descriptions of persons stopped and how those descriptions were related to victim accounts and radio transmissions. The assessments of the lawfulness of frisks and searches had similar requirements. Frisks must be based upon reasonable suspicion that the person was armed and dangerous. This could be established in one of two ways: Either the person was suspected of a violent crime, which permitted an automatic frisk; or, that that officer observed or heard something about the person that gave a reasonable suspicion that person was armed with a weapon. This could be the observation of a bulge that looks like a weapon, or a statement by a witness or the suspect that a weapon was observed or possessed. Under these circumstances, the officer was legally permitted to conduct a limited frisk of the area where the suspicious bulge was observed. If the frisk was permitted, and the officer felt what could be a weapon, then a lawful search of that area was warranted.

[11] The PSA command-level analysis reported no difference in total summonses after the BWC implementation. Given the overall 90% decrease in summonses in NYCHA housing between 2013 and 2019, it is possible that some Housing Bureau officers may have been marginally more likely to issue summonses when outfitted with BWCs relative to an aggregate PSA count that could include some summonses by non-Housing Bureau officers.

[12] https://www.nypdmonitor.org/wp-content/uploads/2023/09/13th-Report-and-Submission-Letter.pdf (accessed May 5, 2024).

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