Journal of Urban Economics 141 (2024) 103539

Contents lists available at ScienceDirect

Journal of Urban Economics

journal homepage: www.elsevier.com/locate/jue

Monitoring Police with Body-Worn Cameras: Evidence from Chicago

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ARTICLE INFO

JEL classification:
K42 H0
Body-worn camera
Body cam
Police complaint
Police use of force

ABSTRACT

Using data from the Chicago Police Department on complaints filed by civilians and reports of force filed by officers, this paper estimates the effect of body-worn cameras (BWCs) of officer and civilian behavior. Using a two-way fixed effects design, I find BWCs are associated with a 29% reduction in use-of-force complaints, driven by white officer-black civilian complaints. Additionally, I find a 34% reduction in officers reporting striking civilians and a large though less significant reduction in officer firearm usage, potential mechanisms for the reduction in complaints. Importantly, I find no change in officer injury or force from civilians. However, I find evidence of de-policing as officers make fewer drug-related arrests following BWC adoption.

1. Introduction

Body-worn cameras (BWCs) have become an integral part of a police officer's uniform. In fact, over 80% of US police departments with more than 1,000 officers have deployed BWCs (Hyland, 2018). These devices can assist both officers and civilians by recording incidents where officers interact with their community. These recordings create an observable log of events that a third party can view, removing hearsay between civilians and officers. BWCs even have had widespread usage beyond police officers with adoption among school security guards, prison guards, and the secret service (Nyczepir, 2021). However, due to the rapid adoption of this new technology, many public offices were found spending millions of dollars adopting BWCs before researchers were able to conduct rigorous analysis on their effect.

BWCs primarily act as a monitoring device that helps to support or exonerate officers from accusations of misconduct. This is especially important since officers operate with a high level of impunity making it difficult for complaints to be made against them without substantial evidence. This is illustrated by the fact that in Chicago, 97% of complaints against the police do not result in any disciplinary action (CPDP, 2021). However, equipping officers with BWCs can also lead to behavioral changes since both officers and civilians now know their actions are recorded potentially preventing violent incidents from occurring.

However, the net effect of monitoring police and civilian actions using BWCs is *a priori* unclear. BWCs can lead to reduction in force on the intensive margin, where officers use a lesser amount of force for the same incidents (e.g. using a TASER instead of a firearm) for fear of disciplinary action. This reduction in force would reduce injuries to civilians but could inadvertently increase injury to officers, if force is used prevent officer injury. Choosing the correct level of force is a difficult decision that officers face on a daily basis, where using either too much or too little force can have negative consequences. Conversely, BWCs can increase the level of force if, prior to BWCs, officers were restraining themselves from using force even if force is warranted. Adopting BWCs provides protection to officers against disciplinary action for unfounded complaints. This form of moral hazard can justify force as retaliatory action. This is especially problematic since police departments often award officers with discretion on when to activate their BWC. This discretion can lead to a biased log of events due to selection of which events are recorded. Understanding which officers turn on their cameras and which do not, is a highly important question when evaluating the effectiveness of a BWC program and a question that has not been answered in the literature.

This paper explores the channels in which BWCs affect officer and civilian interactions using data from the Chicago Police Department (CPD), one of the largest police departments in the US. I use a two-way fixed effects design that leverages differences in the timing of BWC adoption across the city of Chicago. I look to answer several questions. First, I explore how BWCs affect use-of-force by officers using civilian-filed complaints as a proxy for excessive force. Next, I turn to officer-reported force data. This supplemental analysis from the officer perspective complements complaints data by allowing me to control for the fact that BWCs may increase the likelihood that a civilian will file a complaint. It additionally allows me to explore other outcomes, such as officer firearm usage, TASER usage, officer/civilian injuries, and civilian force actions. This data differs from previous work that focuses on less-frequent high-cost police actions such as police-involved homicides (Kim, 2021) and misconduct case filings (Bollman, 2021).

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[1] Thank you to Heather Royer, Clément de Chaisemartin, and members of the UCSB applied economics reading group for their comments and suggestions

https://doi.org/10.1016/j.jue.2023.103539
Received 7 October 2021; Received in revised form 20 January 2023
Available online 20 February 2023

An additional worry that BWC critics have proposed is that BWCs could lead to police passivity. Officers equipped with BWCs may second guess involving themselves in interactions that have the potential to lead to external scrutiny, an action known as de-policing. As a result, we may see fewer arrests, fewer investigatory stops, and even potentially more crime. To explore this hypothesis, I also bring in officer arrest and crime reporting data. The combination of these three datasets provides me with a constellation of evidence on the effect of BWCs on officer and civilian actions.

I find that BWCs are associated with a reduction of 0.031 use-of-force complaints per district per day, a large 29% reduction from the pre-treatment mean. This effect is driven by a reduction in white officer-black civilian complaints. In an attempt to avoid conflating the reduction in complaints with a change in complaint filing behavior, I also study officer-reported force. I identify two possible mechanisms for the reduction in complaints within officer-reported actions. First, I find a reduction of 0.024 reports of officers striking a civilian per district per day, a 34% reduction from the pre-treatment mean. Second, I find a less significant but large reduction in firearm usage. While I find reduction in these two action categories, I find no significant reduction in total actions, suggesting that effort from officers are not reduced. Additionally, I find no significant difference in civilians resisting arrest or assaulting officers as well as no change in reported injuries to officers. Taken together, these results suggest that BWCs are an effective deterrence device against excessive use-of-force, importantly not at the expense of officer safety.

I additionally find a short-run increase in officer reports of crime that persist for one quarter that are not in response to a change in arrest behavior. This short-run change is likely due to an increase attentiveness by officers immediately following BWC's adoption. The fact that this change is short-lived, explains some of the large and inconsistent findings of earlier evidence of BWC's effects that use only short study periods. Lastly, BWCs are not associated with any change in officer arrest behavior in the aggregate. I obtain robust and precise null estimates for arrests for assault/battery and theft/robbery. However, exploring arrest behavior more deeply exposes that following BWC adoption, officers make 0.63 fewer drug-related arrests per district-day. This is a large 24% decrease. Several hypotheses for this reduction are discussed, one such hypothesis consistent with this decline is de-policing.

Since BWCs became a topic of national debate in 2015, scholars have worked to pin down their effects. Research until recently has relied on RCT evidence that use a small handful of volunteer officers (many have fewer than 100 officers) that have been randomized to use (or not use) BWCs. Researchers should be careful when extrapolating results from small police departments to larger ones that face very different circumstances. These articles also vary significantly both in terms of magnitude and sign of the measured effects. In a review of this literature Lum et al. (2019) finds that "the use of BWCs does not have consistent or significant effects on officers' use-of-force, arrest activities, proactive or self-initiated activities, or other measured behaviors." In fact, several papers find that BWCs increase use-of-force (Ariel et al., 2015), some find decreases (Ariel et al., 2015; 2017; Braga et al., 2017; 2018; Ready and Young, 2015), and others find no change (Jennings et al., 2015; Yokum et al., 2019). These differences in findings call into question whether BWCs are an effective or valuable policy intervention.

This paper adds to the literature in several ways. First, this paper leverages extensive information about officer actions, most importantly, use-of-force. Measuring frequency and intensity of use-of-force by officers helps to unpack the changes we see in complaints. This is especially important in this context, since BWCs can change the likelihood that a civilian files a complaint. This paper supports the findings of Zamoff et al. (2021) and Braga et al. (2022), which study how BWCs affect officer stops and citizen complaints and relate them to a developing literature on the determinants of officer use-of-force (Ba et al., 2021; Fryer Jr, 2019; Hoekstra and Sloan, 2020; Johnson et al., 2019; Weisburst, 2019). Second, this paper explores the effect of BWCs on civilians. In the past, data limitations have made this analysis difficult since civilian actions are often not recorded. In Chicago, officers report the amount of force used by civilians and when a civilian resists arrest or assaults an officer. This allows me to explore an important side of these two-sided officer-civilian interactions. Third, this paper provides evidence of BWCs effectiveness in a diverse new environment, Chicago, which has not been studied in the literature. The Chicago Police Department (CPD) is one of the largest in the United States and is responsible for policing 2.7 million people. Crime is committed frequently in Chicago, in 2019 there were a total of 25,545 violent crimes and 81,153 property crimes committed, this equates to almost 300 violent or property crimes per day (CPD, 2019). This rate of crime is above the national average but importantly is driven by a high rate of violent crime; Chicago has the seventh highest murder rate in the country (Monkovic and Asher, 2021). This data on use-of-force, citizen-filed complaints, and reports of crime, which document the same police force over the same period of time, allows a holistic approach to uncovering the effect of BWCs.

The remainder of the paper proceeds as follows. In Section 2 I provide background for BWCs in the US and detail their adoption into Chicago, I additionally detail several instances where BWCs have had large societal impacts. In Section 3, I review the multiple data sources that I use and the empirical methods of this paper. In Section 4, I present my results on complaint counts, officer reported force, and officer arrest behavior. I conclude with extensions for further research in Section 5.

2. Background

2.1. Body-Worn Cameras and Police

While some BWCs were in use prior, they became a topic of nationwide debate in 2014 following the protests over the shooting of Michael Brown in Ferguson, MO. This police shooting is one where eyewitness accounts and officer testimony differed greatly on the actions that took place. Most of nearly two dozen eyewitnesses claimed that Mr. Brown, who had already been struck by a bullet, turned around and surrendered with his hands up. The officer, on the other hand, claimed that Mr. Brown had turned around and charged at him. In total, 12 bullets were fired, resulting in the death of Mr. Brown. A grand jury chose not to indict the officer (Somashekhar and Kelly, 2014). With a lack of video recording, the inconsistency between witness accounts and officer testimony lead to nationwide outrage.

Protests over the shooting and the lack of indictment spread both in Ferguson and across the nation calling for increased accountability of officers and improvements in the relationship between officers and the minority community. The protests that followed brought the "Black Lives Matter" movement into nation-wide attention for the first time (Luibrand, 2015). Distrust over the officer's account of the events lead to the protest slogan, "Hands up, Don't shoot". BWCs have been proposed as a potential technology that could have been paramount in the legal proceeding that followed the shootings (Nunes, 2015).

Following the protests, then-President Barrack Obama announced a $75 million grant specifically for purchasing BWCs. The purchase of BWCs was meant to address a "simmering distrust" between officers and minority communities (Feeney, 2014). In the first year, the US DOJ disbursed $23.2 million from this grant to 73 agencies in 32 states. Among these cites were Chicago, Los Angeles, and Detroit (Bureau of Justice Assistance et al., 2015).

The rapid increase in BWC adoption was cataloged in a 2016 survey by the US Bureau of Justice Statistics. They found that 47% of the 15,328 law enforcement agencies in the United States had acquired BWCs. BWCs are more common in larger police departments; in fact, of the departments with over 1,000 sworn officers 81% had acquired BWCs, compared to less than half of departments with fewer than 1,000 sworn officers. Of the agencies that do not have BWCs, 77% of them give the reason of cost. While almost all agencies who had acquired them had deployed at least one BWC, only 60% had fully deployed them

Table 1
Empirical Studies of BWCs in US Police Forces.

Study	Size	Contains Volunteer Officers	Location	Identification Strategy	Duration	Result(s)
Katz et al. (2014)	56 BWCs per day		Phoenix, AZ	DID w/ 1 Treated District	15 Months	-34% complaints, +8% arrests
Ariel et al. (2015)	54 Officers		Rialto, CA	Shift-level RCT	12 Months	-90% complaints, -50% use of force
Jennings et al. (2015)	87 Officers (46 w/ BWC)	Yes	Orlando, FL	Officer-level RCT	12 Months	-65% complaints, -53% use of force
Ready and Young (2015)	100 Officers (50 w/ BWC)	Yes	Mesa, AZ	Officer-level RCT	10 Months	+23% citations, +14% stops, -7% arrests
Ariel (2016)	119 Officers w/ BWC		Denver, CO	DID w/ 1 Treated District	6 Months	+38% complaints, No change in use of force
Morrow et al. (2016)	56 BWCs per day		Phoenix, AZ	DID w/ 1 Treated District	15 Months	+7% arrests
Hedberg et al. (2017)	56 BWCs per day		Phoenix, AZ	DID w/ 1 Treated District	12 Months	-62% complaints
McClure et al. (2017)	60 Officers	Yes	Undisclosed	Officer-level RCT	6 Months	Unclear, slight reduction in arrests
Goodison et al. (2017)	84 Officers	Yes	Arlington, TX	Shift-level RCT	6 Months	-42% complaints
Braga et al. (2018)	416 Officers	Yes	Las Vegas, NV	Officer-level RCT	12 Months	-14% complaints, -12.5% use of force
Peterson et al. (2018)	504 Officers		Milwaukee, WI	Officer-level RCT	14 Months	-58% complaints, No change in use of force, arrests
Wallace et al. (2018)	149 Officers		Spokane, WA	Officer-level RCT	6 Months	No change in arrests, +15% stops
White et al. (2018)	200 Officers		Tempe, AZ	Officer-level RCT	6 Months	+13% stops
Park and Pang (2019)	64 Agencies		National Study (LEMAS)	TWFE	1-2 years, annual data	+5% arrests
Yokum et al. (2019)	2,224 Officers		Washington, DC	Officer-level RCT	7 Months	No chance in complaints or use of force
Kim (2021)	1,001 Agencies		National Study (LEMAS)	TWFE	≤1 year	-58% police involved homicide
	96 Agencies		New Jersey State	TWFE	4 Years	-20% use of force
Bollman (2021)	111 District Courts		Virginia State	TWFE	12 Years	-10% resisting or asasulting officer
Zamoff et al. (2021)	35,000+ Officers		New York City, NY	TWFE	12 Months	+17% stops, -20% complaints per stop, -16% arrests per stop

Notes: Table contains only a subset of studies of US police forces and does not contain meta-analysis papers. Duration in most cases refer to the duration of the study period and does not contain the pre-intervention period. Definitions of use-of-force and complaints vary, these results do not necessarily use the same measures of each outcome and are only meant to give an overview of their findings. This table borrows from Zamoff et al. (2021) and Lum et al. (2019).

to all officers. Still, in the US there was an estimated 414,504 officers equipped with 119,399 BWCs in 2016. Departments that had acquired BWCs gave the following most common reasons for doing so: to improve officer safety (82%), reduce/resolve civilian complaints (81%), improve evidence quality (79%), reduce agency liability (78%), and improve officer/agency accountability (74%) (Hyland, 2018).

In general, the public supports BWCs, with at least 49% of respondents to a YouGov poll strongly supporting their adoption in every age category, race category, and political ideology category. However, despite widespread support for BWCs, the lack of rigorous analysis on their ability to reduce excessive use-of-force is surprising.

2.2. Prior Literature

In recent years there has been a flurry of research on BWCs in the fields of criminology, criminal justice, and law to unpack how BWCs affect officer and civilian behaviors. However, despite the large number of studies, empirical evidence on the effect of BWCs on the behaviors of officers and civilians does not have consistent findings. Several studies have found a reduction in officer use of force (Braga et al., 2017; Ready and Young, 2015; Ariel et al., 2017; Braga et al., 2018; Kim, 2021). However, many other studies have found null effects (Yokum et al., 2019; Jennings et al., 2015) and even some finding positive effects (Ariel et al., 2015).

Empirical studies of BWCs in US police forces are summarized in Table 1. While not a comprehensive list of all papers, this list is meant to be representative of the current literature on BWCs. Of these 18 studies, 5 use volunteer officers. Volunteer officers represent a threat to external validity, since these volunteer officers are likely different from the average police officer and those who use force excessively likely would not volunteer to be monitored. Additionally, many studies listed in Table 1 contain 100 or fewer officers that are given BWCs. The issues that these smaller departments face and the types of officers who volunteer to wear a BWC are likely different from those who commit the most egregious offenses in large cities, such as Chicago.

Despite inconsistent initial results, a collection of recent papers has provided more clarity on BWCs. Two papers study BWCs used by the NYPD and find a decrease in citizen complaints and an increase in stops using quasi-experimental methods (Zamoff et al., 2021) and a large sample RCT (Braga et al., 2022). However, they both find no change in use-of-force by officers. Additionally, Braga et al. (2022) indicate that a change in stops could be driven by a change in officer documentation rather than a substantive change in behavior, highlighting the need for both officer-recorded and citizen-recorded data on police actions. While theses papers do not find a reduction in use-of-force, recent work has found reductions in use-of-force by officers in New Jersey (Kim, 2021) and by civilians in Virginia (Bollman, 2021).

The work most closely related to my article is by Zamoff et al. (2021), which studies the BWC adoption by the New York City police department using a quasi-experimental design (two-way fixed effects). They find that BWCs are associated with a 17% increase in officer-reported investigatory stops, driven by non-violent and non-weapon stops, and a 20% decrease in complaints per stop (10% decrease in complaints when using stops as the dependent variable instead of dividing by the changing stop rate). This result is suggestive of two main findings. First, that BWCs lead to fewer complaints and second, that police officers are not

reducing their proactive policing activities. These two results together are indicative that BWC are succeeding in their intended purpose, to improve the relations between officers and civilians without reducing the effort of officers.

This paper builds on the findings of Zamoff et al. (2021) in several ways. First, this paper contains more information about officer actions, most importantly, use-of-force. Measuring use-of-force by officers helps to unpack the changes we see in complaints. This is especially important in this context, since BWCs can change the likelihood that a civilian files a complaint. This paper supports the findings of Zamoff et al. (2021), which study how BWCs affect officers stops and citizen complaints, and relates them to a developing literature on the determinants of officer use-of-force. Second, this paper explores the effect of BWCs on civilians. In the past, data limitations have made this analysis not possible since civilian actions are not recorded. In Chicago, officers report the amount of force used by civilians and when a civilian resists arrest or assaults an officer. This allows me to explore an important side of these two-sided officer-civilian interactions. Third, this paper provides results using a new police department, Chicago, which has not yet been studied. Since US police departments are highly decentralized, heterogeneous treatment effects between different departments would not come as a surprise making recreation of prior studies in new departments highly relevant for policy makers.

2.3. The Chicago Police Department

Chicago is an interesting setting to study BWCs for several reasons. For one, Chicago is the second largest police force in the country with almost 14,000 employees, 12,000 of which are sworn officers as of 2016. Additionally, with over 6,800 BWCs deployed, they have the second largest deployment of BWCs of any department (behind only the NYPD). The demographics of sworn officers in Chicago are mixed with 42% non-Hispanic White, 18% Hispanic, and 15% Black. 78% of officers are male.[2]

The CPD is not without scrutiny. An investigation that began in 2015 by the Department of Justice Civil Rights Division painted a bleak picture of the state of the CPD. With regards to the trust between police officers and civilians they stated: ”The City [of Chicago] and CPD acknowledge that this trust has been broken. (...) It has been broken by systems that have allowed CPD officers who violate the law to escape accountability. This breach in trust has in turn eroded CPD’s ability to effectively prevent crime”.

The DOJ also found that use-of-force by CPD officers is used ”frequently” and is ”often unconstitutional”. They claimed that officers repeatedly engage in ”tactically unsound and unnecessary foot pursuits, and that these foot pursuits too often end with officers unreasonably shooting someone—including unarmed individuals”. Accountability for these use-of-force incidents was also lacking. The DOJ found ”[t]he potential for inappropriate coordination of testimony, risk of collusion, and witness coaching during interviews is built into the system”. In their conclusion, the DOJ recommended full adoption of BWCs to all officers.

However, BWCs should not be considered a panacea for all problems that a police department faces. Within the CPD, usage of BWC and proper handling of their recordings is paramount to their effectiveness. While BWCs can serve as substantial evidence in claims of excessive use-of-force incidents, they must be activated in order serve this purpose. The DOJ acknowledges that alongside BWCs, policies that delineate officer’s responsibilities to record videos as well as policies that dictate how videos will be stored.

2.4. Chicago and Body-Worn Cameras

In October 2014, 17-year-old Chicago resident Laquan McDonald was shot in the back by a CPD officer. The officer was later charged with second degree murder after dashboard camera footage showed him firing while Mr. McDonald was walking away, contradicting his previously filed report. In response to the shooting, protests erupted calling for an increase in transparency and accountability. Within one month, the City of Chicago began testing BWCs for their officers. During a pilot program starting January 2015, only a handful of volunteer officers from District 14 wore 30 BWCs and there was confusion about who was required to wear them and for how long (Briscoe, 2016).

After conclusion of the pilot program, CPD began its rollout of BWCs to officers in June 2016, over one year later, with stricter guidelines on usage. BWCs were rolled out in two main waves with the first 7 districts adopting in 2016 and the next 15 in 2017. While a district’s historical use-of-force was a consideration, rollout was not solely based on this level of force. Section 4.4 contains several robustness checks that explore whether the non-random ordering of treatment could be driving the main results. However, there is little evidence that this non-random ordering had a substantial effect.

Since it took several years after the death of Laquan McDonald for BWCs to be adopted, we can rule out that a reduction in use-of-force following BWC’s adoption is in response to protests, an effect often seen from BLM movements (Campbell, 2021). The dates of implementations are reported in Table 2 as well as a map of the Districts in Fig. 1. After full implementation, Chicago had the largest deployment of BWCs in the nation at the time. The program was estimated to have cost $8 million in 2017 (when 15 districts adopted BWCs) and will cost $6.5 million per year for upkeep of the program (Newman, 2016). This upkeep is a small fraction (0.4%) of the CPD’s $1.8 billion budget (2020) and is equivalent to the fully loaded annual cost of 50 additional officers (Chalfin and McCrary, 2018).

In addition to BWCs, officers received training on how to activate their device, when to do so, and what policies govern it. In order to record an incident, an officer must activate their BWC by pressing a button on the device. Once activated, the previous 30 seconds before activation are saved and live recording begins afterwards. By law, officers are required to activate their BWC for almost all law-enforcement-related encounters and maintain recording until either the the officer is no longer engaged in the incident, the victim of the crime requests the BWC be disabled, or when interacting with a confidential informant or witness. Prohibited content that requires BWCs to be disabled includes strip searches, medical facilities, court hearings, and within private residences if there is not suspicion that a crime has taken place. When disabling a BWC, officers are required to audibly state the reason of deactivation.

While these policies make activation of BWCs clear for the majority of encounters, there is still some discretion left up to the officer. For example, officers are not required to activate their BWC if they believe that doing so could endanger themselves or others. Disciplinary action can be (and has been) taken if an officer repeatedly fails to follow BWC policy, though anecdotally disciplinary action is reserved for extreme cases.[3] That being said, if an officer intends to break the law or violate the rights of a citizen, they may choose not to activate their BWC if they believe the costs of doing so are less than the disciplinary action that would follow. Understanding who uses their BWC is a very important when exploring the effect of BWCs implementation.

All recordings are required to be saved a minimum of 90 days. A video is flagged if it contains an incident in which an officer fires a weapon or uses force, contains death or great bodily injury, or by the discretion of the officer or the officers supervisor, then it is to be saved

[2] Authors calculation’s from the Law Enforcement Management and Administration Survey (LEMAS) 2016.

[3] Author’s exploration of complaint files, several instances of disciplinary action for misuse of BWCs is present.

Table 2
BWC Implementation Dates and District Observables.

Patrol District	Date of Implementation	Use of Force Complaints per Day	(Rank)	Sworn Officers	(Rank)
Shakespeare (14th)	June 1, 2016‡	0.042	(19)	243	(22)
Austin (15th)	June 13, 2016	0.096	(10)	350	(7)
Wentworth (2nd)	June 29, 2016	0.067	(14)	323	(14)
South Chicago (4th)	July 8-13, 2016	0.124	(5)	327	(13)
Ogden (10th)	July 25, 2016	0.120	(6)	356	(6)
Gresham (6th)	August 4, 2016	0.189	(2)	341	(8)
Deering (9th)	August 18, 2016	0.084	(11)	336	(11)
Central (1st)	March 10, 2017	0.075	(13)	313	(16)
Near North (18th)	March 31, 2017	0.077	(12)	360	(5)
Englewood (7th)	May 1, 2017	0.168	(3)	426	(2)
Harrison (11th)	June 5, 2017	0.205	(1)	462	(1)
Chicago Lawn (8th)	October 2, 2017	0.105	(7)	394	(3)
Rogers Park (24th)	October 16, 2017	0.031	(20)	272	(17)
Lincoln (20th)	October 23, 2017	0.025	(21)	247	(20)
Town Hall (19th)	October 30, 2017	0.061	(16)	389	(4)
Morgan Park (22nd)	October 30, 2017	0.059	(17)	271	(18)
Grand Crossing (3rd)	November 6, 2017	0.105	(8)	321	(15)
Jefferson Park (16th)	November 20, 2017	0.052	(18)	266	(19)
Calumet (5th)	November 20, 2017	0.127	(4)	332	(12)
Albany Park (17th)	November 27, 2017	0.025	(22)	246	(21)
Near West (12th)	December 4, 2017	0.062	(15)	339	(9)
Grand Central (25th)	December 4, 2017	0.099	(9)	337	(10)

Notes: Use of force complaints per day is the mean for the years 2014 to 2019. Sworn Officers is as of 02/2017, source: Chicago Inspector General, (Link). ‡ District 14 started a pilot program in January 2015 but did not implement department wide until this date, because of this imprecision, District 14 is not used in analysis.

for a minimum of two years. While videos are the property of the department, they can be requested by the public for several reasons. All videos that are flagged are required to be available for release from a Freedom of Information Act (FOIA) request. All video footage, flagged or not, is required to be disclosed to a subject of an encounter or the subjects legal team. Viewing BWC videos can be done by CPD officers, but tampering with specific videos is not possible by individual officers, and digital logs are kept of each video viewed. (5 ILCS140/7.5(cc), 2016).

BWC videos have had impacts on the legal proceedings that involve officer's use of force. One such example in Chicago is that of Adam Toledo, a 13-year-old who was shot and killed on March 29th, 2021. Initial reports were that Adam Toledo raised a gun at the officer before being shot, but this rhetoric was later disputed after BWC footage of the incident was released that showed Adam Toldeo disposing of his firearm before turning to face the officer. Former Illinois State Representative Luis Gutiérrez discussed the effect that the BWC footage had on him saying, "But you know what I thought? I said, the kid had a gun. He pointed it at the police officer. And then I saw the video. I didn't see a gun. What I saw was a frightened 13-year-old kid getting shot by a police officer."

In the case of Adam Toldeo, the officer maintained recording with his BWC throughout the incident. In cases such as these, BWCs have the ability to aid in litigation procedures when they fully capture the events that occur. However, not fully capturing the entirety of an event may lead BWC footage to be unhelpful and potentially biased. Zamoff (2019) find that in use-of-force cases that involve BWC footage, having BWC footage greatly increases defendants likelihood of success on summary judgement. However, Zamoff also finds that in cases where BWC footage is incomplete, partial footage lowers the likelihood of defendants' success (compared to no footage). This result highlights the potential bias the BWC footage has, when an officer has discretion on when to use and not use their BWC.

Additionally, Cubukcu et al. (2021) find that BWCs in Chicago led to a significant decrease in the dismissal of citizen-filed complaints against officers. It is often that citizen-filed complaints are not investigated due to insufficient evidence. This finding indicates that BWCs do in fact increase the likelihood an officer faces disciplinary action, given the additional evidence that BWCs provide. Quite importantly, before BWC implementation, the likelihood of complaints being dismissed was unequal between race groups. After BWC implementation, these disparities were vastly reduced.

3. Data & Methods

3.1. Complaints Data

To explore the frequency of officer misconduct, I use an administrative dataset of recorded complaints against officers of the Chicago Police Department (CPD). These complaints are filed through the Civilian Office of Police Accountability, a civilian oversight agency in charge of monitoring the CPD. This dataset was obtained from the Citizens Police Data Project, a news organization that sued the CPD for access to complaint data under the Freedom of Information Act. These records contain the type of allegation filed (e.g. use-of-force, tardiness, inadequate service, etc.) as well as the date, time, and circumstances of the incident. In most, but not all cases, information about officer that was accused and the civilian complainant are included. In total, there are 17,977 complaints between 2014 and 2019, 4,282 of which are use-of-force complaints. Summary statistics of these complaints are reported in Table 3.

While counts of citizen complaints are not a perfect measure of actual misconduct, they serve as a proxy for the true level of misconduct. Rozema and Schanzenbach (2019) have shown that civilian complaints are strong indicator of legitimate police misconduct and a strong predictor of future misconduct by accused officers. This is despite potentially differing perspectives from civilians and officers on what constitutes appropriate conduct. Still, citizens complaints are often considered a lower bound of the true amount of force that officers use (Ba, 2018).

3.2. Tactical Response Reports

The next dataset used consists of officer-reported use-of-force. These reports of use-of-force are submitted internally as Tactical Response Reports (TRRs). According to CPD policy, a TRR must be filed if a civilian alleges injury due to officer actions or the civilian actively resists, flees, uses force against an officer, or physically obstructs an officer (Chicago Police Department, 2020). The filing of a TRR automatically triggers a review and approval process by an officers supervisor, a process that is usually completed within 20 minutes of the event.

Table 3
Descriptive Statistics, 2014 - 2019.

	Mean	SD	Min	Max
Body Worn Camera	0.438	0.494	0.00	1.00
Dependent Variables, Complaints				
All Complaints	0.391	1.296	0.00	82.00
Use of Force	0.093	0.497	0.00	16.00
Substance Use	0.003	0.054	0.00	3.00
Crime/Bribe	0.006	0.095	0.00	8.00
Domestic	0.007	0.124	0.00	9.00
Personnel	0.098	0.572	0.00	45.00
False Arrest	0.035	0.363	0.00	13.00
Illegal Search	0.058	0.508	0.00	39.00
Other Complaints	0.086	0.572	0.00	27.00
Dependent Variables, Tactical Response Reports				
TRR Reports Filed	0.397	0.990	0.00	14.00
Civilian Struck	0.053	0.291	0.00	6.00
Firearm Discharge	0.003	0.069	0.00	4.00
Taser Discharge	0.034	0.200	0.00	4.00
Subject Contained	0.320	0.860	0.00	13.00
Member Presence	0.272	0.846	0.00	14.00
Other Action	0.160	0.591	0.00	11.00
Dependent Variables, Reports of Crime				
All Crimes	33.669	12.305	1.00	146.00
Assault/Battery	8.674	4.674	0.00	37.00
Theft/Robbery	11.795	6.091	0.00	116.00
Drug Crimes	1.354	2.574	0.00	55.00
Sex Crime	0.285	0.864	0.00	17.00
Other Crime	10.621	4.726	0.00	66.00

Notes: Observation level is district-day. Total observations: 46,011.

For this analysis, I use all TRR reports filed over the years 2014 to 2019 for non-juvenile suspects. Each TRR may contain several actions, such as firearm usage (by either party), suspect take-down, suspect punched, among others. The benefit of this dataset is that it includes a wide range of use-of-force levels from lethal to mild. In total, there are 18,790 TRR actions from on-duty officers and sergeants over the sample period. Summary statistics of TRRs are reported in Table 3 where they are broken down by officer and subject characteristics.

3.3. CPD Crime Statistics

To observe arrest and reported crime outcomes I use data from the CPD. This data includes the universe of all arrest and reports from CPD officers for non-juvenile offenders. This data is publicly available and contains 356,493 arrests and 1,549,150 reports between 2014 and 2019. Summary statistics of crime reports are reported in Table 3. Note that this dataset does not include minor crimes (e.g. vandalism or loitering) and reports of crimes with no arrest do not include narcotics (since without an arrest, the presence of narcotics is difficult to quantify).

3.4. Empirical Strategy

I exploit district-level variation in the timing of BWC implementation to identify their impacts on various outcomes. I estimate the following two-way fixed effects model:

$$y_{dt} = \beta_0 + \beta_1 BWC_{dt} + \Gamma_d + \Pi_t + \delta_1 BWC\_Anticipation_{dt+1} + \varepsilon_d \quad (1)$$

Where d indexes each of the 22 Chicago policing districts, except district 14, and t indexes time (in days). I omit District 14 from my analysis since they had BWCs before their implementation date and had a 'soft' implementation late in 2016, though results are robust to its inclusion and are presented for completeness. My outcome of interest, y_{dt}, measures several counts of complaints, TRRs, or crimes throughout this paper, for example all complaints, use-of-force complaints, use-of-force complaints by younger or older officers, TRR outcomes, etc. To overcome data constraints, complaints here are assigned to the district in which the incident occurred rather than the officer's assigned district, though both analysis are shown in Section 4.1. The variable BWC_{dt} is a variable that equals to 1 if district d has a implemented BWCs at time t and 0 otherwise. Also included are district fixed effects (Γ_d) and day-by-month-by-year fixed effects (Π_t). Lastly, due to anticipatory effects in the month leading up to BWC adoption I include a 1 month lead to BWC adoption. This variable ($BWC\_Anticipation_{dt+1}$) takes a value of 1 in the month before BWC adoption and a 0 everywhere else. This variable does not account for the effect of BWC announcement (since all BWC announcements came several months in advance) but controls for any effect of increased observation due to BWC training and BWC installation that occurred just before adoption.[4] I cluster the standard errors at the district level (Bertrand et al., 2004). Due to the small number of clusters, I also report p-values using wild-bootstrap methods with 2000 replications and Rademacher weights (Cameron et al., 2008).

The coefficient of interest is β_1, which measures a weighted sum of average treatment effects of BWCs on our outcome y_{dt}. Identification comes from district-level differences in the timing of BWC that is described in Table 2. The use of a two-way fixed effects model is appropriate here since policing districts within Chicago are plausibly similar, an assumption that is less likely in designs that compare outcomes in separate cities or states.

There are assumptions that must be made in order to interpret the results from Eq. (1) as a causal treatment effect. First, the rollout of BWC to districts was not correlated with differential trends in my outcomes. Indeed, in Table 2 districts who had higher levels of force used received BWCs earlier. This potential of non-random assignment is dealt with in several ways, first, I use an event study design to explore the possibility of differential trends in my outcomes leading up to BWC adoption. This assumption of parallel trends is explored by estimating:

$$y_{dt} = \beta_0 + \gamma_{-7} D^{-7}_{dt} + \left[\sum_{i=-6, i\neq -1}^{6} \gamma_i D^{i}_{dt} \right] + \gamma_7 D^{+7}_{dt} + \Gamma_d + \Pi_t + \delta_1 BWC\_Anticipation_{dt+1} + \varepsilon_d \quad (2)$$

[4] There is a slight decrease in the number of use-of-force complaints in the one month leading up to BWC adoption that can be seen in Appendix Figure A.2. $BWC\_Anticipation_{dt+1}$ is meant to account for this anticipatory effect. Aggregated event studies are robust to the exclusion of this anticipation term.

Table 4
OLS Estimates of the Effect of BWCs on Complaint Counts, Split by Complaint Category.

	Dependent Variable: Complaint Counts							
	(1) All	(2) Use-of-force	(3) Substance/Alcohol	(4) Domestic	(5) Personnel	(6) False Arrest	(7) Illegal Search	(8) Other
Body-Worn Camera	-0.033	-0.031***	-0.001	0.000	-0.001	-0.008	0.020	0.020
	(0.037)	(0.011)	(0.001)	(0.002)	(0.022)	(0.008)	(0.013)	(0.012)
N	46,011	46,011	46,011	46,011	46,011	46,011	46,011	46,011
Mean Dep. Var. (Pre)	0.363	0.107	0.003	0.009	0.077	0.040	0.029	0.089
Wild Bootstrap p	0.441	0.012	0.642	0.933	0.942	0.355	0.183	0.136

*** Significant at 1% level ** Significant at 5% level * Significant at 10% level.

Notes: The dependent variable is a daily count of complaints. Complaints span 2014 to 2019 and include complaints filed against the Chicago Police department that specify the incident's district (excluding District 14). Regressions are estimated via OLS and contain district and day-by-month-by-year fixed effects and a one month lead. Standard errors clustered at the district-level are shown in parentheses. Significance stars are associated with the cluster-robust standard errors. Mean of the dependent variable while $BWC = 0$ (pretreatment) are presented for each column. Wild bootstrap p values using Rademacher weights and 2000 replications are shown for the coefficient of interest.

Where D^{i}_{dt} is a set of indicators that equals 1 if there is are BWCs i quarters from day t in district d. The time periods 7 or more quarters from enactment are pooled together into a single indicator. These pooled time periods are represented by D^{-7}_{dt} and D^{+7}_{dt}. I also omit the quarter before implementation. Here a quarter is considered 91 days. I bin days into quarters since many complaints and officer actions are low frequency events. By binning together multiple time periods, I am able to maximize the power of each dynamic estimate. Similar to Eq. (1), day-by-month-by-year, district fixed effects, and a 1 month lead are included.

Lastly, a threat to identification is the presence of negative weights in the weighted sum of treatment effects that β_1 is identifying. If present, negative weights on district-day groups could lead to the estimate of β_1 to differ from the true average treatment effect both in terms of magnitude but could even differ in sign (De Chaisemartin and d'Haultfoeuille, 2020). Importantly, this is only an issue if treatment effects are not consistent across early and late adopting districts (since weights always add to 1, putting negative weights on late adopters and higher weights on early adopters only leads to different findings under heterogeneous treatment effects) (De Chaisemartin, d'Haultfoeuille, 2020). To reduce the presence of negative weights I restrict the sample to 2014 to 2017 cutting down on the post-treatment period where all units are treated. This restriction leads to fewer than 1 month of time in which all units are treated and greatly reduces the sum of negative weights from -0.38 to -0.04. Under this restriction, treatment effects are remarkably similar, shown in Appendix Table A.5, for both Use-of-Force Complaints and Strike TRRs. Since the calculated average treatment on the treated (ATT) is robust to changes in the weighting, it is unlikely that this ATT is averaging vastly different effect sizes. This also motivates some confidence that heterogeneous treatment effects are not a problem in this model. I explore heterogeneous treatment effects more directly in Section 4.4 where I find treatment effects do not differ greatly over time.

4. Results

4.1. Complaints Results

I first examine the effect of BWC implementation on complaint counts by estimating Eq. (1) with several complaint categories. The results for each of these categories are presented in Table 4. In Column 2 of this table I find a significant reduction of 0.031 use-of-force complaints per day in districts that have BWCs. This change is equivalent to a large 29.0% reduction from the pre-treatment mean and is suggestive that BWC were very effective at reducing use-of-force complaints in Chicago. Annually, this equates to 281 fewer use-of-force complaints per year across the city. The standard errors, clustered at the district level, allow me to rule out a reduction of less than 8% with 95% confidence. This finding is consistent with the hypothesis that BWCs increase the cost of excessively using force for police officers. By increasing the probability of detection, through BWC monitoring, we see officers responding by reducing their level of force.

In addition to a reduction in use-of-force complaints, BWCs are associated with an increase in complaints for illegal search, shown in Column (7). Now that civilians are armed with BWC footage, they could be increasing their complaint filing, emboldened with this new evidence. While this effect is less precise than the result for use-of-force complaints, it similarly represents a large change (69%). Other than use-of-force and illegal search complaints, BWCs are not associated with changes in other complaint categories. These separate categories act as a placebo test of sorts, to test that these changes are not due to administrative changes in complaint filing or changes in civilians ability to file complaints.

To further test whether this effect is driven by differing pre-treatment trends, I conduct an event study analysis in Fig. 2 by estimating Eq. (2) with use-of-force complaints as the outcome. In this figure I find no evidence of differential pre-trends in the quarters leading up to BWC implementation. This event study analysis does suggest that districts did not react immediately, as we see declines strongest after the first quarter. This can be explained by officers not being able to adjust their routine instantaneously. I conduct the same event study with district-specific linear time trends in Appendix Figure A.3 and find no difference when adding trends.

One goal of BWCs was to improve the relationship between police officers and the minority community. To explore whether BWCs had heterogeneous effects based of race, I separate each use-of-force complaint into categories based on the race of the officer and the race of the complainant in Table 5. I find a significant reduction in white officer-black civilian complaints (Column (2)). This finding is consistent with similar findings in labor economics, that increasing monitoring can inadvertently reduce race-based discrimination (Parsons et al., 2011) as well as the finding from Cubukcu et al. (2021), that found BWCs reduce inequalities between the adjudication of citizen filed complaints. In addition to a reduction in white officer-black civilian complaints, I identify a reduction in complaints where the officer race is not identified. This could be due to BWCs increasing the ability of Civilian Office of Police Accountability to identify officers using recorded videos.

Several other specifications are shown in the Appendix where I find similar reductions in use-of-force complaints (relative to their means) between older and younger officers (Table A.1), male or female officers (Table A.1), and no difference in internal complaints (complaints filed by officers or superiors against officers) (Table A.2). Sensitivity analysis is also shown in Appendix Figure A.1, where I show results are not driven by any single district by estimating an district-level leave-one-out model.

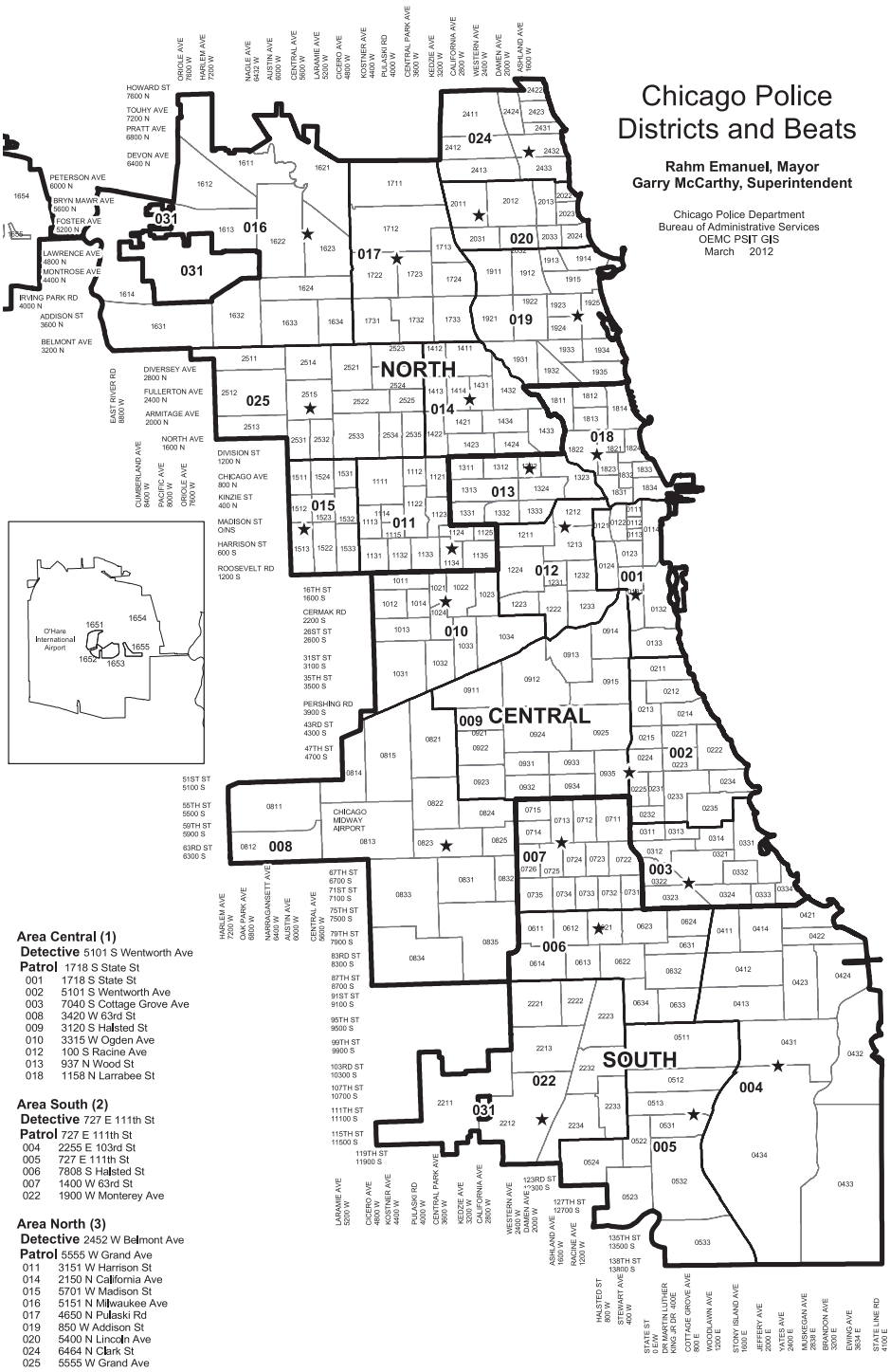

Fig. 1. Map of the City of Chicago with Districts and Beats Labeled.
Note(s): District numbers labeled in bold. Each District's police department building is marked by a star. The Civilian Office of Police Accountability office is located in District 13. Source: WWTW Chicago.

Lastly, I vary sample selection and treatment assignment decisions to explore the sensitivity of the reduction in use-of-force complaints. Table 6 presents these estimates.

In Column (1) of Table 6 I include District 14, which held the BWC pilot before implementing BWCs district-wide. The inclusion of District 14 adds noise to the data, since this district was not sharply treated like the others but does not change the estimates magnitude to a large degree. Next, in Column (2) I expand the the years of data with two additional years of pre-treatment. The results are almost identical to my preferred sample. Lastly, in Column (3) I use the grouping of police districts into three areas, North, Central, and South and include area-specific linear time trends. These areas are shown in Fig. 1 Once again, the results shown are almost identical.

Next, I vary the method used to aggregate complaints. Thus far, complaints have been assigned to the district in which the incident took place. An alternative specification is to group complaints by the district of the offending officer. This alternative specification allows me to observe complaints filed against officers who are assigned BWCs, rather than geographic regions (districts) in which BWCs are in effect. This does not affect the majority of complaints since they are filed against officers within their own district (within district complaints account for 83% of complaints where an officer identified). However, using officer

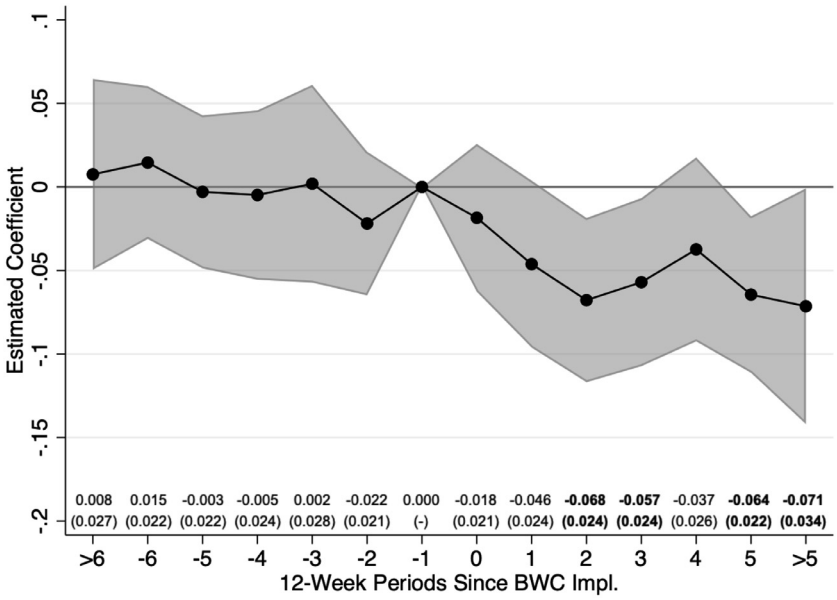

Fig. 2. Event Study for the Effect of BWC on Use-of-Force Complaints.
Notes: Observation level is daily, the variable for "0" equals 1 in the 84 days (12 weeks) after BWCs are implemented and 0s elsewhere, and so forth. Estimated with OLS with day-by-month-by-year fixed effects, district fixed effects, and 1 month lead. 12 weeks before BWC implementation is omitted. 95% confidence intervals shaded, standard errors clustered by district. Coefficients and standard errors are shown above the x-axis, **bold** estimates are significant at a 5 percent level. Mean of pre-treatment dependent variable = 0.107.

Table 5
OLS Estimates of the Effect of BWCs on Use-of-Force Complaints, Split by Officer and Complainant Race.

	Dependent Variable: Use-of-Force Complaints									
Officer Race:	White			Black			Hispanic			Unknown
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Civilian Race:	All	Black	White	All	Black	White	All	Black	White	All
Body-Worn Camera	-0.013*	-0.015***	0.000	-0.004	-0.005	0.001	-0.003	-0.001	-0.002	-0.010***
	(0.007)	(0.005)	(0.002)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)
N	46,011	46,011	46,011	46,011	46,011	46,011	46,011	46,011	46,011	46,011
Mean Dep. Var. (Pre)	0.043	0.027	0.007	0.016	0.011	0.002	0.019	0.011	0.003	0.026
Wild Bootstrap p	0.099	0.025	0.822	0.106	0.119	0.324	0.304	0.696	0.127	0.002

*** Significant at 1% level ** Significant at 5% level * Significant at 10% level.
Notes: The dependent variable is a daily count of use of force complaints. Complaints span 2014 to 2019 and include complaints filed against the Chicago Police department that specify the incident's district (excluding District 14). Regressions are estimated via OLS and contain district and day-by-month-by-year fixed effects and a one month lead. Standard errors clustered at the district-level are shown in parentheses. Significance stars are associated with the cluster-robust standard errors. Mean of the dependent variable while $BWC = 0$ (pretreatment) are presented for each column. Wild bootstrap p values using Rademacher weights and 2000 replications are shown for the coefficient of interest.

information has one major downside; complaints without an accused officer only contain the district in which the incident occurred. Complaints that are missing the offending officer make up a significant portion of my sample (32%). In Columns (4) and (5) of Table 6 I deal with these missing-officer complaints in different ways. In Column (4) I assign complaints without an accused officer to the district that the incident takes place. In Column (5) I drop complaints where I do not observe an accused officer. Both of these specifications are consistent with my main findings. The result with these specifications represent a 29% and 27% reduction, respectively.

4.2. TRR Force Results

Next, I explore the effect of BWCs on officer-reported actions. Table 7 provides the two-way fixed effects estimates from Eq. (1) with various officer actions as the dependent variable. I find that BWCs are associated with a reduction in officers striking civilians ('Strike' TRRs) in Column (2). This reduction of 0.024 Strike TRRs is equivalent to a 33.8% reduction from the pre-treatment mean. The magnitude of this effect is remarkably similar to the reduction in use-of-force complaints seen in Section 4.1. The matching magnitude suggests that officers striking civilians may be one of the main mechanisms for the reduction in use-of-force complaints. This potential mechanism is an important finding, since several similar studies find reductions in complaints with no corresponding change in use of force (Peterson et al., 2018; Zamoff et al., 2021; Ariel, 2016), a finding that suggests that BWCs act on civilian complaint filing rather than police actions.

Next, in Column (3) of Table 7 I find a reduction in firearm usage by officers. This result is large but less precise. Still, this result provides some promising evidence that BWCs can reduce firearm usage by officers. Firearm usage is an example of force at the most extreme level and can be exceptionally costly to the public, especially if it is motivated by race-based prejudice rather than to prevent officer injury. In fact, I find no significant evidence that BWCs are associated with a change in officer injuries (Column (7)).

Next, in Fig. 3 I conduct an event study analysis by estimating Eq. (2) with Strike TRRs as the outcome. I find no evidence of differential pre-trends in the quarters leading up to BWC implementation. Additionally, the shape of the results mirror the shape of my event study analysis for use-of-force complaints (Fig. 2), providing further evidence of the relationship between the two.

In addition to an effect on officers, BWCs may have a civilizing effect for civilians. However, qualitative studies on BWCs have noted that officers often fail to inform citizens that they are being recorded and civilians rarely react to BWCs (Braga et al., 2017). In Panel B of Table 7 I study officer-reported civilian actions. I find no significant effects of BWC on these civilian actions. This result is in line with the hypoth-

Table 6
OLS Estimates of the Effect of BWCs on Use-of-Force Complaints, Robustness Table.

	Dependent Variable: Use of Force Complaints				
	(1)	(2)	(3)	*Treatment Assigned by*	
	Including District 14	Including Years 2012-2019	Including Area-Specific Linear Time Trend	(4) Officer's District (Except missing officer)	(5) Officer's District (Drop if missing officer)
Body-Worn Camera	-0.024*	-0.041***	-0.030***	-0.031***	-0.019**
	(0.012)	(0.015)	(0.010)	(0.013)	(0.009)
N	48,202	61,362	46,011	46,011	46,011
Mean Dep. Var	0.104	0.152	0.093	0.108	0.071
Wild Bootstrap p	0.080	0.015	0.011	0.009	0.041

*** Significant at 1% level ** Significant at 5% level * Significant at 10% level.
Notes: The dependent variable is a daily count of use of force complaints. Regressions are estimated via OLS and contain district and day-by-month-by-year fixed effects and a one month lead. Standard errors clustered at the district-level are shown in parentheses. Significance stars are associated with the cluster-robust standard errors. Mean of the dependent variable while $BWC = 0$ (pretreatment) are presented for each column. Column (3) contains area-specific linear time trends, these areas are North, Central, and South and are marked in Fig. 1. Wild bootstrap p values using Rademacher weights and 2000 replications are shown for the coefficient of interest.

Table 7
OLS Estimates of the Effect of BWCs on Tactical Response Reports.

	(1) All Reports	(2) Strike	(3) Firearm	(4) Taser or Pepper Spray	(5) Subject Contained	(6) Officer Present	(7) Officer Injured
Panel A: *Officer Actions*							
Body-Worn Camera	-0.003	-0.024**	-0.004*	0.005	0.008	0.051	-0.010
	(0.033)	(0.009)	(0.002)	(0.006)	(0.028)	(0.031)	(0.010)
N	46,011	46,011	46,011	46,011	46,011	46,011	46,011
Mean Dep. Var. (Pre)	0.436	0.071	0.003	0.050	0.350	0.421	0.087
Wild Bootstrap p	0.927	0.018	0.047	0.431	0.786	0.110	0.378
	(1) Assault/ Battery	(2) Deadly Attack	(3) Civilian Resisted	(4) Fled	(5) Injured		
Panel B: *Civilian Actions*							
Body-Worn Camera	-0.018	-0.004	-0.001	0.000	-0.017		
	(0.019)	(0.003)	(0.031)	(0.017)	(0.013)		
N	46,011	46,011	46,011	46,011	46,011		
Mean Dep. Var. (Pre)	0.211	0.012	0.420	0.121	0.102		
Wild Bootstrap p	0.356	0.234	0.967	0.984	0.240		

*** Significant at 1% level ** Significant at 5% level * Significant at 10% level.
Notes: The dependent variable is a daily count of TRRs. Strike TRRs is defined by the author and contains all TRRs that specify the following actions by the officer: *Punch, slap, elbow strike, knee strike*, or *baton hit*. Resisted TRRs is defined by the author with actions by the civilian: *did not follow verbal direction, pulled away*, or *stiffened (dead weight)*. Categories are **not** mutually exclusive, a single report may contain multiple officer actions categories. TRRs span 2014 to 2019. Regressions are estimated via OLS and contain district and day-by-month-by-year fixed effects and a one month lead. Standard errors clustered at the district-level are shown in parentheses. Significance stars are associated with the cluster-robust standard errors. Mean of the dependent variable while $BWC = 0$ (pretreatment) are presented for each column. Wild bootstrap p values using Rademacher weights and 2000 replications are shown for the coefficient of interest.

esis that BWCs directly affect officers actions and have little or no effect on civilians' level of force.

4.3. Crime Reports Results

Thus far it is not clear whether the reduction in force by officers, seen in the previous two sections, is a net positive or negative change to social welfare. While a reduction in injury to civilians is a positive change, a change in use-of-force by officers due to de-policing may diminish this benefit. De-policing, the act of officers reducing their efforts in response to negative or external scrutiny, can arise following reforms (such as BWCs). I investigate the presence of de-policing by exploring the arrest behavior of officers following BWC adoption in Table 8.

In Panel A Column (1) I find no evidence that BWCs are associated with a reduction in total arrests. In Columns (2) through (7) I break down these arrests into types. I find no significant change in arrests for both Assault/Battery and Theft/Robbery. In fact, I can reject at a 95% level any reduction in Assault/Battery arrests by more than 4% and a reduction in Theft/Robbery arrests by 9%. However, I do find a significant reduction in arrests for both narcotics and for cannabis possession in Columns (4) and (5). In Column (4) I find a reduction in narcotics arrests by 22% and in Column (5) I find a reduction in cannabis possession arrests by 26%.

To test the validity of these two-way fixed effects estimates, I conduct an event study analysis in Fig. 4 for all drug-related arrests. While Fig. 4 provides some evidence of pre-trends, there is a clear structural break in the dynamic effects observed that supports the two-way fixed effects findings of approximately a 25% reduction in drug-related arrests. There are several possible reasons for this reduction.

First, On July 29th 2016, Illinois State Senate Bill 2228 decriminalized possession of small amounts of cannabis (<10 grams). This policy change occurred during the rollout of BWCs across Chicago. While this change affected the city as a whole (making the time fixed effects account for any uniform change) there may have been district-specific interactions that could bias these estimates. That being said, when removing any arrests for possession of small (or medium) amounts of cannabis

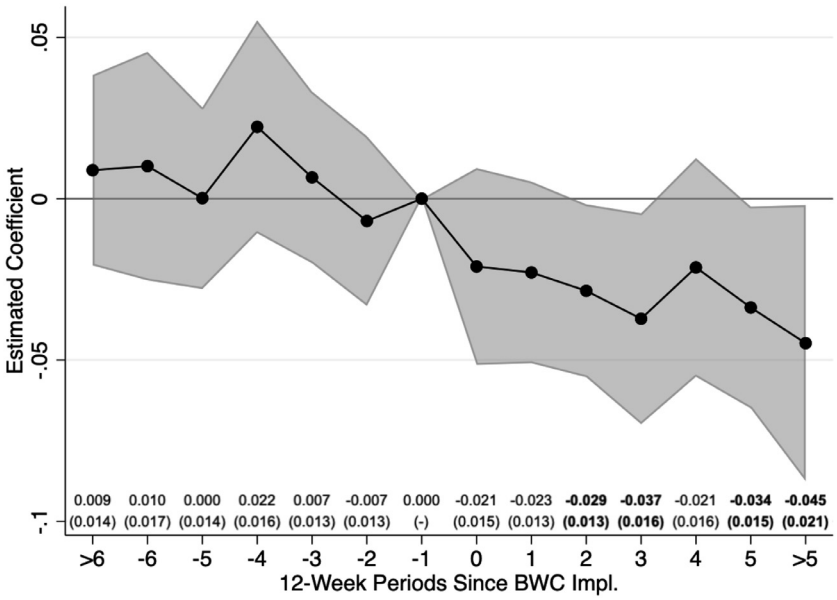

Fig. 3. Event Study for the Effect of BWC on TRRs with *Strike*.

Notes: Observation level is daily, the variable for "0" equals 1 in the 84 days (12 weeks) after BWCs are implemented and 0s elsewhere, and so forth. *Strike* contains all TRRs that specify the following actions by the officer: *Punch, slap, elbow strike, knee strike*, or *baton hit*. Estimated with OLS with day-by-month-by-year fixed effects, district fixed effects, and 1 month lead. 12 weeks before BWC implementation is omitted. 95% confidence intervals shaded, standard errors clustered by district. Coefficients and standard errors are shown above the x-axis, **bold** estimates are significant at a 5 percent level. Mean of pre-treatment dependent variable = 0.071.

Table 8
OLS Estimates of the Effect of BWCs on Arrests and Reports or Crime.

	(1) All Arrests	(2) Assault/ Battery	(3) Theft/ Robbery	(4) Narcotics	(5) Cannabis Possession	(6) Sex Crimes	(7) Other
Panel A: Counts of Arrests							
Body Worn Camera	-0.581*	0.021	0.027	-0.306**	-0.323**	0.039	-0.038
	(0.335)	(0.049)	(0.067)	(0.118)	(0.129)	(0.029)	(0.144)
N	46,011	46,011	46,011	46,011	46,011	46,011	46,011
Mean Dep. Var. (Pre)	8.251	1.891	1.193	1.394	1.256	0.183	2.333
Wild Bootstrap p	0.093	0.657	0.703	0.009	0.028	0.165	0.820
	(1) All Reports	(2) Assault/ Battery	(3) Theft/ Robbery	(4) Narcotics	(5) Cannabis Possession	(6) Sex Crimes	(7) Other
Panel B: Reports of Crimes							
Body Worn Camera	0.395	0.584***	0.248	-0.307**	-0.324**	0.039	0.155
	(0.594)	(0.150)	(0.470)	(0.119)	(0.129)	(0.031)	(0.161)
N	46,011	46,011	46,011	46,011	46,011	46,011	46,011
Mean Dep. Var. (Pre)	33.397	8.283	11.736	1.398	1.257	0.288	10.434
Wild Bootstrap p	0.507	0.003	0.610	0.005	0.023	0.196	0.348

*** Significant at 1% level ** Significant at 5% level * Significant at 10% level.

Notes: Crimes span 2014 to 2019 and include all incidence of crimes collected by the City of Chicago. A single offender can be arrested for multiple crimes at one time, this is counted as 2 arrests but occurs only approximately 1% of total arrests. Regressions are estimated via OLS and contain district and day-by-month-by-year fixed effects and a one month lead. Standard errors clustered at the district-level are shown in parentheses. Significance stars are associated with the cluster-robust standard errors. Column 6 includes all crimes not counted in columns 2 through 6 of this table. Mean of the dependent variable while $BWC = 0$ (pretreatment) are presented for each column. Wild bootstrap p values using Rademacher weights and 2000 replications are shown for the coefficient of interest.

the effect prevails. Nor does this fully explain why the decriminalization of small amounts of cannabis would reduce arrests for heroin or methamphetamine, as it does.

Another potential explanation is that the reduction in drug-related arrests could be due to de-policing. Officers equipped with BWCs could be compelled to think more critically about what constitutes reasonable suspicion, since their actions can now be more easily scrutinized. With fewer searches, narcotics and drug-related paraphernalia would be less likely do be found. This result is consistent with Ready and Young (2015), that find officers equipped with BWCs are *half* as likely to initiate a stop-and-frisk search. This reduction in police searches may lead to a reduction in arrests for narcotics. Still, the effect of BWCs on de-policing is an active area of research that needs more exploration.

In Panel B of Table 8 I explore reports of crimes. These reports are filed by officers but are often at the request of civilians. In Column (2) I find a significant increase in reports of assault/battery but a null finding for arrests of this type. Turning to an event study design for this outcome in Fig. 5, I find that this is solely a short-run change in assault/battery reports. It is unlikely that this change in reporting is due to a change in the frequency of crime, but rather a change in the reporting habits of officers, now being more diligent to report offenses. An officer who is being recorded may feel more compelled to properly report each offense that they observe since their actions can be observed by a third party. This finding is similar to a behavioral change seen in Boivin and Gendron (2021), that finds officers provide more accurate report writing after viewing BWC footage. This short run increase in reporting could explain the findings from other BWC studies that find increased officer-initiated stops, especially since often these BWC studies use short study periods (Ready and Young, 2015; Wallace et al., 2018; White et al., 2018). In Columns (4) and (5) the reports for narcotics and cannabis

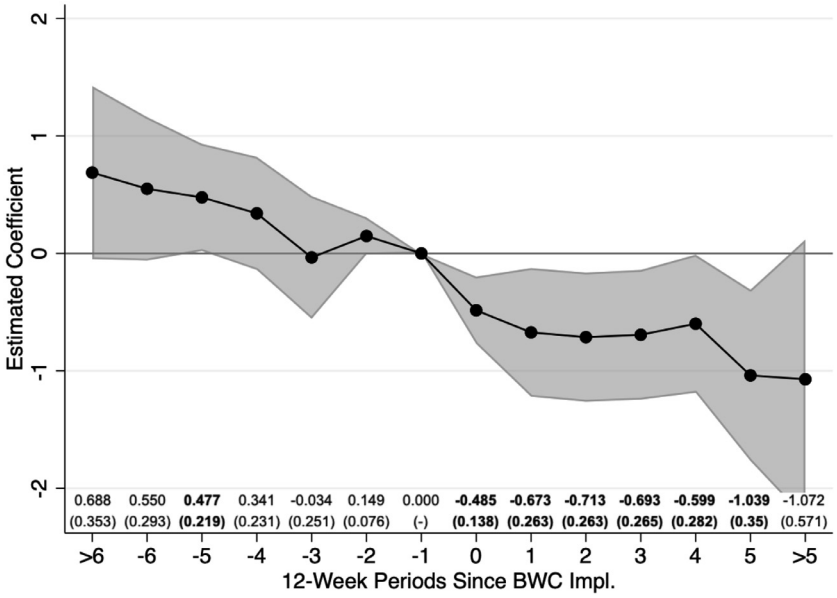

Fig. 4. Event Study for the Effect of BWC on Arrests for Narcotics and Cannabis Possession.
Notes: Observation level is daily, the variable for "0" equals 1 in the 28 days after BWCs are implemented and 0s elsewhere, and so forth. Estimated with OLS with day-by-month-by-year fixed effects, district fixed effects, and 1 month lead. 12 weeks before BWC implementation is omitted. 95% confidence intervals shaded, standard errors clustered by district. Coefficients and standard errors are shown above the x-axis, **bold** estimates are significant at a 5 percent level. Mean of pre-treatment dependent variable = 2.655.

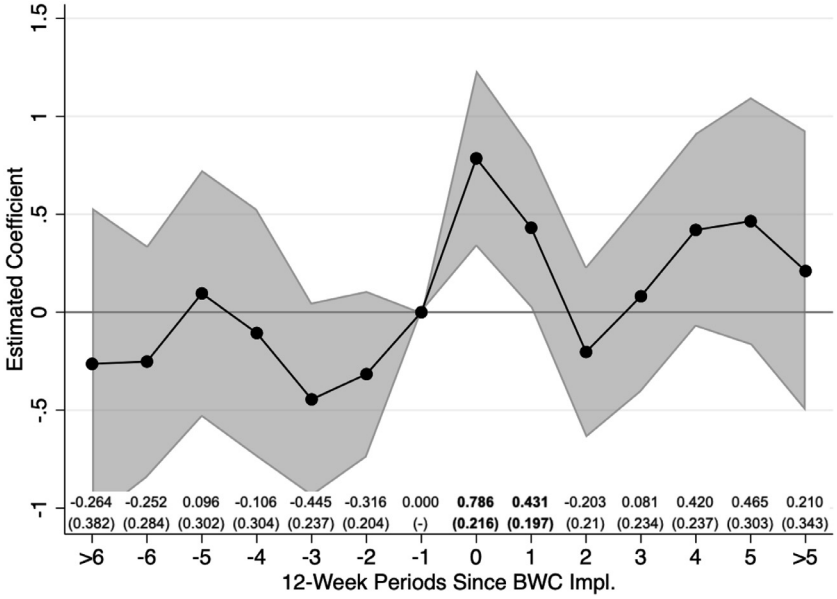

Fig. 5. Event Study for the Effect of BWC on Reports of Assault/Battery Crimes.
Notes: Observation level is daily, the variable for "0" equals 1 in the 28 days after BWCs are implemented and 0s elsewhere, and so forth. Estimated with OLS with day-by-month-by-year fixed effects, district fixed effects, and 1 month lead. 12 weeks before BWC implementation is omitted. 95% confidence intervals shaded, standard errors clustered by district. Coefficients and standard errors are shown above the x-axis, **bold** estimates are significant at a 5 percent level. Mean of pre-treatment dependent variable = 8.283.

possession mirror that of arrests and provide no additional information (since crime reports do not include drug related offenses if no arrest is made).

4.4. Robustness

In this subsection, I present several robustness checks to access the validity of my results. First, I show that my results are not driven by a single district. I conduct a "leave-one-out" analysis where I exclude one district from my sample before estimating Eq. (1). These results are shown in Appendix Figure A.1 for both Use-of-Force complaints (Subfigure (a)) and Strike TRRs (Subfigure (b)). In both figures I find similar effects to my full sample.

Next, I explore the ordering of rollout. While the ordering of BWC rollout is not random, it is unlikely that the choice of ordering is driving the main results for several reasons. First, since BWCs are announced months in advance of installation, it is unlikely that rollout decisions were made based on short-run temporal changes but more likely based on historical fixed differences or long-run historical trends. Any fixed differences between districts that could be driving the order of rollout are presumably captured by the inclusion of district-specific fixed effects in Eq. (1). Additionally, any long-run district-specific linear trends are controlled for with the inclusion of district-specific linear time trends in Figure A.2 (use of force complaints) and Figure A.3 (Strike TRRs), where I find similar effects to the main specification. Second, I conduct event study analyses to explore the possibilities of differential trends in the outcomes in the period of time before BWCs are adopted. If, for example, BWCs are adopted in response to changes in force used by officers, I would likely find one of the outcomes to have significant pretrends. Because I find no evidence of pretrends, I find the assumption of parallel trends likely to hold. Third, the order of rollout could pose a problem in the presence of heterogeneous treatment effects between early and late adopters. This could overstate the true treatment effect if early adopters (with higher weights) have substantially larger effects. Heterogeneous treatment effects are explored in Appendix Table A.3 where districts are divided into early and late adopters based on whether they rolled

out BWCs in the first wave in 2016 or in the second wave in 2017.[5] I find no evidence that early and late adopters had significantly different treatment effects both in Strike TRRs (Column (2)) and Use-of-Force Complaints (Column (4)). I find that, while the first wave (where force is used more) had stronger effects, they do not differ significantly from a model where the treatment effect is assumed to be equal across waves (Columns (1) and (3)).

Third, I explore the robustness of these results to alternative difference-in-differences estimators, specifically, an event study framework proposed by Callaway and Sant'Anna (2021). This estimation strategy (CS) estimates "group"-specific treatment effects where "groups" are districts who adopt BWCs within the same quarter. Average treatment effects are then constructed by averaging these group-specific treatment effects to ensure that unequal weighting of certain districts is not driving two-way fixed effects results or masking significant pretrends. In this case weights are equal for each district. Included as Appendix Figure A.5 are two event studies that use this estimation technique for use-of-force complaints and TRRs with strike (Subfigure (a) and Subfigure (b), respectively). Across both figures, the two-way fixed effects results fall within the confidence intervals of the CS event studies in all but 1 quarter. In both cases, standard errors are larger, which is unsurprising since this estimation strategy reduces the amount of variation that is explored. The event study for use-of-force complaints follows a similar pattern as the main two-way fixed effects results while the strike TRRs are more attenuated.

5. Conclusion

BWCs have become commonplace for police officers with the majority of officers in large departments equipped with one. However, whether or not these devices reduce excessive use-of-force instances between officers and civilians is still not known. Using a combination of datasets that document police and civilian actions in Chicago, I estimate the relationship between civilian-filed complaints, officer-reported force, civilian and officer injuries, and officer arrests.

I find that BWCs were an effective technology to reduce use-of-force by police officers without increasing injury to officers. The reductions that I find are large and are driven by black civilian-white officer interactions. I proxy officer use-of-force with civilian-filed complaints, however, reductions across officer-reported actions tell a similar story. BWCs are associated with less reports of officers striking civilians and less firearm usage. Importantly, these reductions in officer actions are not seen in other categories, such as officers containing civilians, indicating that officers are reducing their level of force on the intensive margin rather than removing themselves from interactions entirely.

However, this paper is not without its limitations. First, I find that police officers make fewer drug-related arrests when equipped with BWCs. While a reduction in excessive use-of-force by officers can have measurable positive impacts, if those reduction are at the expense of crime prevention, then the benefit from BWCs would be reduced. Second, this paper focuses on the Chicago Police Department, one of the largest in the country. Policymakers need to be careful when using results from one location and applying the results to another. It may be that smaller departments, which deal with a lower level of crime, have lesser benefits of BWCs than larger ones. More research is needed in order to understand both treatment effect heterogeneity between departments and overall average treatment effects for the nation as a whole.

In a meta-analysis of several BWC papers, Williams Jr et al. (2021) estimate a benefit-cost ratio of BWCs to be 4.95. If as much as one quarter of these benefits are to the city, then BWCs could possibly pay for themselves. In fact, using the estimated costs from Williams Jr et al. (2021), back of the envelope calculations suggests that benefits of BWCs in Chicago far outweigh the costs of upkeep for the program.[6] However, despite the benefits that BWCs offer and their ability to reduce instances of force between officers and civilians, they are not enough to fully eliminate all excessive force. This is a problem that needs addressing as the City of Chicago has paid millions in legal fees due to police misconduct. In 2018 alone, Chicago paid out more than $113 million for police misconduct lawsuits with an average of one lawsuit every two days (Newman, 2019).

CRediT authorship contribution statement

Toshio Ferrazares: Conceptualization, Data curation, Formal analysis.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2023.103539

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[5] BWCs were announced in two waves with the first 7 receiving BWCs in 2016, followed by the remaining 15 in 2017. While districts in the first wave had higher levels of force used (on average) there does not appear to be ordering within each of the two wave. This relationship is shown in Appendix Table A.4.

[6] This calculation is shown in Appendix Table A.6 and uses *Strike TRRs* as a substitute for *non-fatal force*. I only account for reductions in non-fatal force and a reduction in complaints as those outcomes are most similar to my findings. This should be seen as a lower bound as this cost benefit ratio does not account for the potential loss of life that can be prevented by BWCs, especially given the reduction in firearm usage. The total estimated benefit is $27,742,206 annually, approximately 4 times the annual upkeep cost of BWCs in Chicago.

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