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Facilitating police reform: Body cameras, police-involved homicides, and law enforcement outcomes☆

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ABSTRACT

Body-worn cameras (BWCs) have emerged as a crucial reform to restore police legitimacy. However, there remains limited evidence on the conditions under which BWCs reduce use of force and affect broader agency-wide outcomes. Using a quasi-experimental event study design, I analyze data from 593 U.S. police agencies to estimate the effects of BWC adoption. I find that reductions in police-involved homicides are heterogeneous—concentrated in regions with higher prior levels of such incidents and in agencies with stricter activation requirements, with no measurable change in low-incident regions or agencies with weaker policies. This study also provides the first evidence on agency-wide outcomes, finding no significant trade-offs in overall arrest or crime rates. These findings offer insight into when BWCs are most likely to enhance police accountability and performance.

1. Introduction

Recent high-profile and controversial police use-of-force incidents have sparked protests across Western societies and ignited calls for police reform. Within these debates, officer body-worn cameras (BWCs) have emerged as a potential tool for police reform. By providing video documentation of police encounters with community members, BWCs are intended to strengthen accountability and improve relations between law enforcement and the public.

Despite growing interest in BWCs as a tool for police reform, whether they offer a viable solution to the ongoing crisis of police legitimacy remains an open question. Critics argue that BWCs may have limited impact if current practices leave little room for improvement or if officers retain broad discretion over camera activation. The introduction of BWCs may also reduce proactive policing efforts, as officers—wary of making errors that could be captured on camera—become less assertive in their crime control activities. Empirical findings are also mixed: while some randomized controlled trials (RCTs) in specific agencies found significant reductions in use of force (e.g., Ariel et al., 2015; Jennings et al., 2015), more recent large-scale evaluations report null effects (e.g., Yokum et al., 2019; Peterson et al., 2018), raising doubts about the general effectiveness of BWCs.

These mixed findings and ambiguous theoretical predictions underscore the need to better understand the conditions under which BWCs influence police behavior and outcomes. I address this gap through a cross-agency, quasi-experimental event study that takes advantage of the staggered rollout of BWCs nationwide. This approach offers several advantages that address key limitations of prior research, which has largely relied on single-agency RCTs. First, it draws on a large and more representative sample of agencies, rather than the narrow subset that typically self-selects into research participation (Allcott, 2015), which enhances the generalizability of the findings. It also allows for heterogeneity analysis across diverse implementation contexts. Second, it avoids the spillover effects that may have contributed to null findings in single-agency RCTs, where officers with and without BWCs often work side by side and are supervised under the same management (e.g., Ariel et al., 2015). Third, it enables the first systematic assessment of broader agency-wide outcomes, such as crime rates, which are critical for evaluating the full welfare implications of BWC adoption.

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A central element of this approach is the use of newly available data on the precise dates of BWC adoption, combined with high-frequency monthly data on policing outcomes. These data allow me to link adoption timing with sharp changes in police behavior and performance. This empirical strategy takes advantage of administrative variation that occurred during the rapid nationwide expansion of BWCs, a period often marked by logistical uncertainty and administrative confusion.

I gather BWC adoption data from the Body-Worn Camera Supplement to the Law Enforcement Management and Administrative Statistics (LEMAS) Survey, a novel 2016 survey conducted by the Bureau of Justice Statistics. The survey asked police chiefs across the U.S. whether their departments had adopted BWCs and, if so, the specific year and month of adoption. On the national level, there is no reliable governmental source of use of force data, so I rely on crowd-sourced data on police-involved homicides, which represent the most extreme form of the use of force, spanning from 2013 to 2020. I use Mapping Police Violence (MPV), which is compiled from the three largest crowd-sourced databases and is currently, to my knowledge, the most comprehensive accounting of police-involved homicides. Additionally, to study crime and enforcement outcomes, I incorporate data on arrests, index crimes, and felony assaults against officers, from the Uniform Crime Reporting (UCR) database maintained by the Department of Justice.

The main analysis focuses on 593 agencies that adopted BWCs between January 2014 and June 2016 and deployed them to at least 40 % of their officers. This threshold is chosen to capture agencies with substantive exposure and is empirically motivated by observed differences between full and incomplete implementation. Using data from one year before and after adoption, I estimate a roughly 22 % decline in police-involved homicides, though the estimate is imprecisely measured.

Disaggregating the average effect reveals significant variation across agencies. I explore two sources of variation informed by prior research. A meta-analysis by Lum et al. (2020), based on a small set of single-agency RCTs, finds stronger effects of BWCs in departments with pre-existing issues and more stringent activation policies. My results generalize this insight using a larger and more representative national sample. I find that agencies in regions with higher baseline levels of police-involved homicides experienced significantly greater reductions—about 38 % (p-value: 0.044). In contrast, agencies in low-baseline regions saw no measurable change. I also examine how BWC policy design influences outcomes. Agencies with stricter activation requirements experienced a statistically significant reduction in police-involved homicides of 0.016 per month—equivalent to 32.7 % of the pre-adoption mean (p-value: 0.031). In contrast, departments with more discretionary activation policies did not experience a significant change. Together, these findings suggest that both the policing context and internal BWC policy can play a critical role in shaping the effectiveness of BWC implementation.

When analyzing heterogeneity by the race of the civilian, I find that the reductions in police-involved homicides are driven primarily by incidents involving White, rather than Black individuals. One possible explanation is that BWCs influence officer behavior differently depending on the race of the civilian. In particular, I observe an increase in index arrests of Black civilians following BWC adoption, which is consistent with the interpretation that BWCs may have prompted more assertive policing toward Black individuals.

I use several strategies to examine alternative explanations for the observed reductions in police-involved homicides. First, I leverage high-frequency monthly data to disentangle the effects of BWC adoption from other policy changes that may have occurred concurrently. Given that BWC implementation typically unfolds over an 18-month period, the monthly data allow for a granular assessment of trends surrounding the time of adoption. The presence of stable pre-trends mitigates concerns about endogeneity due to contemporaneous reforms, such as changes to training protocols or use-of-force policies. Second, to assess whether BWC adoption was bundled with other reforms, I compile data on police training purchase orders, police chief appointments, and other internal policy changes. These analyses show little temporal overlap with BWC rollout. Third, I address the concern that high-profile use-of-force incidents may have generated intense social pressure to adopt BWCs immediately. I conduct robustness checks excluding agencies that adopted BWCs in response to external mandates or experienced high-profile incidents near the time of adoption. Across all these tests, the results remain consistent, suggesting that the observed reductions are not driven by concurrent reforms or external shocks.

I then explore mechanisms underlying the decline in police-involved homicides. I find no evidence of a pullback in policing—discretionary arrest levels remain stable. Nor do reductions appear to result from changes in civilian behavior: data on felony assaults against officers show no significant decline. However, survey responses from the LEMAS data suggest that police chiefs widely believe BWCs improve officer professionalism and are useful for supervision. Moreover, the reductions in police-involved homicides are concentrated in encounters with unarmed civilians, consistent with BWCs deterring less clearly justified uses of force. Together with the heterogeneity analysis, these findings point to changes in officer tactics and behavior as the primary mechanism.

Finally, I find no significant changes in overall crime rates or serious arrests following BWC adoption. Pulling together the findings, a cost-benefit analysis suggests that BWCs can be beneficial for agencies with high rates of police-involved homicides and strong activation mandates. For agencies with fewer such incidents or weaker enforcement policies, estimated savings based solely on the value of a statistical life do not outweigh the costs of implementation.

This paper most directly contributes to the literature that examines the role of BWCs in policing. A growing body of academic research has investigated their potential impact on police accountability, effectiveness, and community relations (Lum et al., 2019).[1] Among other BWC studies, the one that bears the closest resemblance to this paper is a concurrent study by Miller and Chillar (2021). They, too, explore the effects of BWC adoption on police-involved homicides in a nationwide setting using a difference-in-differences (DID) methodology at a yearly frequency, identifying significant reductions in police-involved homicides. However, my paper differs in three significant respects. First, I conduct a thorough examination of potential alternative events that could coincide with BWC adoption. Using monthly data in the nationwide setting, I focus specifically on the time immediately before and after adoption to narrow down the possible confounding factors. Additionally, I incorporate data on purchase orders, high-profile incidents, and other departmental reforms to assess whether BWC adoption coincided with any significant changes within the agencies. Second, I investigate the heterogeneity of results to shed light on the varied findings found in the existing literature. Third, I delve into the trade-off between accountability and crime control by examining changes in crime rates and exploring whether there was any withdrawal of policing effort following BWC adoption.

Outside the BWC literature, this paper contributes to several broader strands of research. First, it adds to the literature on police inputs and crime outcomes. Most existing work in this area focuses on the effects of increasing police staffing levels (Levitt, 1997; McCrary, 2007; Miller and Segal, 2019; Mello, 2019). Recent papers, however, have evaluated innovations in policing, such as the use of computerization (Garicano and Heaton, 2010) and DNA databases (Doleac, 2017). By studying an input that has been introduced primarily to decrease the use of force and the "side-effects" of policing, this paper differs from previous examinations of inputs applied to improve general crime and clearance rates.

Second, this study contributes to the literature on police use of force and accountability. Prior work has explored racial disparities in use of

[1] Several studies have examined how BWCs may influence officers' enforcement activities, including their use of force (Ariel, 2016; Braga et al., 2018; Jennings et al., 2015; Yokum et al., 2019), citizen complaints about officer conduct (Hedberg et al., 2017; Peterson et al., 2018), and arrest rates (Ariel, 2016; Ready and Young, 2015).

force (Fryer Jr., 2018), the role of civilian oversight (Ba, 2017; Rivera and Ba, 2019), and predictive methods to identify high-risk officers (Rozema and Schanzenbach, 2019). Annan-Phan and Ba (2023) examine how patrol environments influence the likelihood of deadly force. This paper builds on these contributions by evaluating a widely adopted accountability tool that can be systematically implemented and scaled across agencies.

More broadly, this paper contributes to a large body of literature on agency issues in the public sector. Incentivizing government employees is particularly challenging due to difficulties in performance measurement and constraints on contracting (Finan et al., 2017). Enforcement agencies, in particular, face distinct hurdles (Khan et al., 2016; Prendergast, 2001; Shi, 2009; Ba, 2017; Rivera and Ba, 2019; Devi and Fryer Jr., 2020). This paper provides some of the first evidence that suggests an intense monitoring tool can improve accountability in this high-discretion context. At the same time, the findings underscore that the effectiveness of such tools depends critically on context and institutional design.

Finally, this paper relates to the literature that examines technology adoption, work organization, and performance (e.g., Bresnahan et al., 2002; Hubbard, 2003; Athey and Stern, 2002; Acemoglu et al., 2007; Aral, Brynjolfsson, and Van Alstyne, 2012). BWCs represent a monitoring tool that, more than those examined in prior studies, enables comprehensive observation of employee behavior—but also raises heightened concerns about intrusiveness (Bernstein, 2012).

The remaining sections of this paper are structured as follows. In Section 2, I provide institutional details about BWCs and their nationwide expansion. Section 3 outlines the data sources used, covering BWC adoption status, police-involved homicides, and other performance measures. In Section 4, I outline the empirical strategy employed to estimate the causal effects of BWC adoption. Section 5 presents the main findings related to police-involved homicides. Section 6 delves into the mechanisms underlying the reduction in police-involved homicides. In Section 7, I examine the effects of BWCs on crime control outcomes and present a cost-benefit analysis. Finally, I conclude the paper in Section 8.

2. Background and previous studies of body-worn cameras

2.1. BWCs as new technology in US policing

BWCs have attracted considerable support from both the public and policy makers. This wide-ranging endorsement has sparked significant policy changes nationwide, making BWCs an integral part of modern policing. In 2014, the Obama administration proposed a subsidy of $263 million to aid local law enforcement agencies in purchasing 50,000 BWCs. This substantial federal funding and grants from state and local governments have facilitated the widespread adoption of BWCs in the US. By 2016, they had been fully deployed by 60 % of local police departments and 49 % of sheriffs' offices (Hyland, 2018). Ongoing debates consider further nationwide expansion of BWCs, which underscores their increasing role in enhancing transparency and accountability in policing.

Although BWCs are not a new technology, their design and functionality have evolved over time, incorporating recent technological advancements. Widespread adoption in the US did not occur until public demand increased following a series of high-profile and controversial officer-involved incidents in New York, Missouri, Illinois, and Ohio in the latter half of 2014 (Maskaly et al., 2017). Fig. 1 illustrates the growth of BWC program coverage across the US. After a modest initial uptake, BWC adoption increased sharply in 2014, reaching a population-weighted share of 67 % of agencies by June 2016. This trend reflects agencies' rush to equip officers with BWCs in an effort to avoid controversies.

Fig. 2 displays the locations of 1001 agencies that adopted BWCs up until June 2016 when the LEMAS survey was conducted. The map highlights the widespread geographical distribution of BWC adoption across the US. It also demonstrates that agencies of varying sizes and

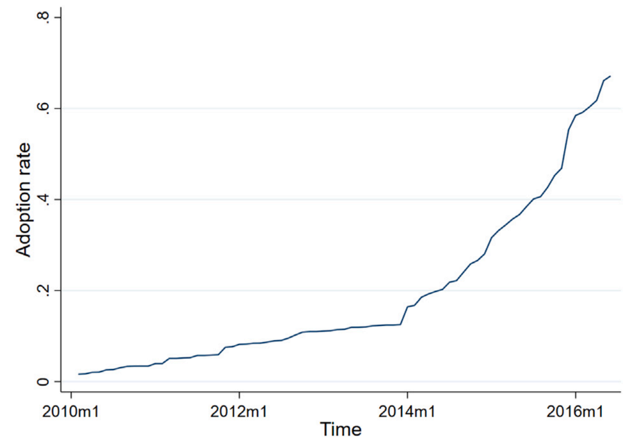


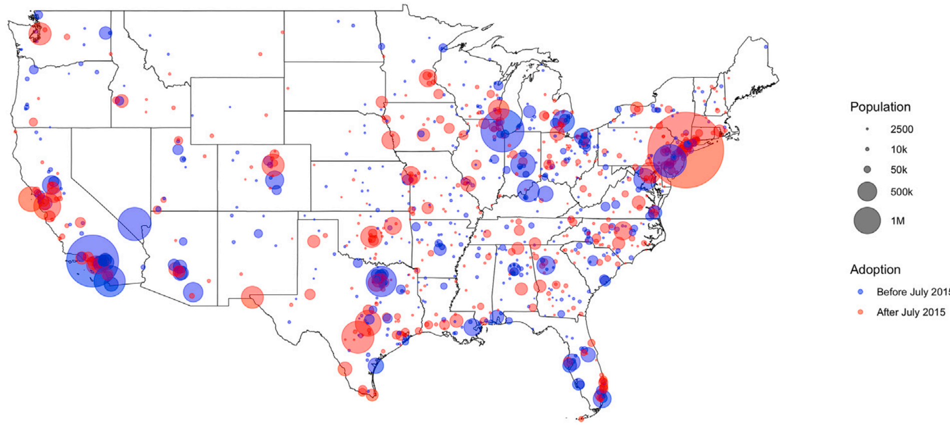
Fig. 1. National trend of BWC adoption. This figure plots the nationwide adoption trends of BWCs by local agencies. The solid trend line is the population weighted proportion of agencies that adopted BWCs by each point in time. In LEMAS data, which is the main source of data for BWC adoption, I consider agencies that mentioned they obtained at least one BWC.

racial diversity had embraced BWC technology by that time. The bottom panel focuses on 593 adopters in the main analysis set, those who adopted BWCs at a rate of more than 40 % of their force.

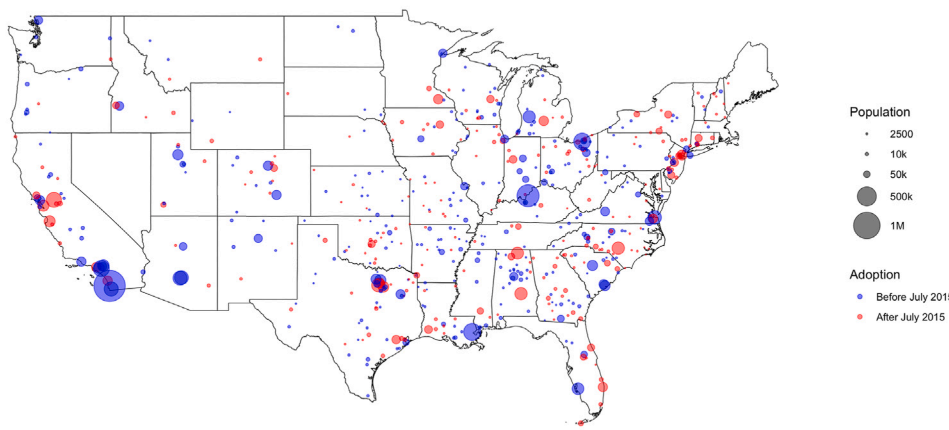
As a more mobile recording device than previous technologies like car dashboard cameras, BWCs have the potential to reshape police–citizen interactions. BWCs can be worn on the front of an officer's uniform or clipped to headgear. Policies regarding when to record vary significantly across departments. For instance, Table A.1 shows that while the vast majority (94 %) of agencies require activation during traffic stops, only about half mandate activation during public order policing, transporting offenders, or special operations. Furthermore, only around a quarter require activation during public events.

2.2. Implementing BWCs 2014–2016

With the advent of BWCs around 2014, police departments, unions, and city councils faced complex challenges posed by this new technology. This was a period in which there was not much information about what a BWC program would entail. In 2014, the US Department of Justice and the Police Executive Research Forum released a report titled "Implementing a Body-worn Camera Program: Recommendations and Lessons Learned (Police Executive Research Forum, 2014)." In 2015, the Bureau of Justice Assistance followed up with an implementation checklist for agencies (BWC Toolkit), highlighting the complexities of starting a BWC program—from developing a plan (program costs and identifying funding sources) to engaging stakeholders like city leadership, community groups, and unions, as well as forming working groups, defining BWC policies, and managing procurement and testing. These documents reflect the significant knowledge gaps during the period of rapid expansion, gaps that were later addressed through the experience of adopters and the availability of these resources. Police Executive Research Forum (2014) highlighted the need for thoughtful deployment of BWCs and the consideration of several difficult questions. Among the concerns were issues regarding the privacy of crime victims, necessitating policies to determine when and where recording should occur and what footage could be made public. BWCs also raised questions about officer supervision, with some officers fearing constant monitoring of their actions. Moreover, leaders grappled with the significant financial costs of implementing BWCs and managing recorded data. For instance, in Federal Way, CA, a police department with 134 officers, the BWC program required an initial allocation of $1.1 million and an annual budget of $450 K, amounting to 1.4 % of the total annual budget and 3.8 % of the budget for Field Operations.



(a) Any Adopters



(b) Main Adopter Sample

Fig. 2. Map of body camera adopters sample January 2014–June 2016. The top panel displays all adopters included in the sample, with the area of each circle representing the variation in population sizes. Early adopters (those who adopted before July 2015) are marked with blue circles, while late adopters (those who adopted after July 2015) are indicated by red circles. The bottom panel focuses on the main adopter sample, those who adopted BWCs at a rate of more than 40 % of their force. This plot follows the same color-coding and circle-size representation. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

In the period of rapid nationwide BWC expansion, these administrative challenges hindered agencies from controlling the timing of BWC purchases and adoption. Lengthy adoption timelines have been documented in various sources, and administrative delays can further extend these timelines. For instance, the police chief of Naperville, IL, estimated the process to take approximately 18 months: "the process of studying devices, requesting proposals, training and installation typically takes 18 months, based on other police departments' timelines." That was the length it took before it could write "a check for the new equipment."[2]

For a concrete example, it may be helpful to consider the timeline of the BWC program implemented by the Rochester Police Department (RPD), NY.[3] In January 2014, the RPD began researching the costs and best practices of BWCs. By December 2014, the mayor and police chief announced plans to start a BWC program. Between April and June 2015, the City Council conducted a survey seeking public input, and in June 2015, the City Council approved $2 million to launch the program and submitted a federal grant application for an additional $600,000. In July 2015, the city issued a request for proposals from vendors for cameras and data management solutions. Limited testing of the cameras and management systems began in September 2015. By January 2016, the RPD was developing BWC policies, and full deployment began in July 2016, incrementally rolling out over the next seven months. This timeline, from initial exploration to full implementation, reflects a 9-month testing phase and a 19-month rollout, which aligns with the experience described by the Naperville chief.

However, additional administrative factors can introduce further delays. Examples from Portland, OR, and Nashville, TN, illustrate how important policy and budget considerations can influence the timing of BWC adoption. In 2014, Portland began studying BWCs after a grand jury decision in the Michael Brown shooting case in Ferguson, MO. Portland commissioners insisted on resolving policy and privacy matters before purchasing the equipment.[4] Similarly, in Nashville, BWCs were

[2] https://www.chicagotribune.com/suburbs/naperville-sun/ct-nvs-naperville-police-body-cameras-st-0913-20200911-bbticmklbbadzhcxjludf2qfoi-story.html.

[3] https://www.cityofrochester.gov/RPDBodyWornCamera/.

[4] https://www.oregonlive.com/portland/2014/12/portland_city_council_debates.html.

prioritized in 2016, but in 2019, the mayor delayed the program due to a budget crisis and concerns over downstream costs. These anecdotal accounts underscore how policy and budget factors can impact the timing of BWC adoption, providing a unique context where adoption timing can serve as an identifying source of variation.

3. Data

3.1. National data on police-involved homicides

There are currently two government-funded datasets on police-involved homicides, neither of which is reliable: justifiable homicides voluntarily reported in the Supplemental Homicide Reports (SHR) under the UCR system of the FBI; and the Department of Justice's arrest-related-death (ARD) dataset. Researchers have long pointed out that these datasets contain substantial omissions (Klinger, 2012; Schwartz and Jahn, 2020; Edwards et al., 2019).[5]

In the wake of controversial use-of-force incidents, journalists and independent researchers have worked to fill these gaps in data needs. They have collected media mentions and crowd-sourced cases and cross-examined them using social media, obituaries, criminal records databases, and police reports. I use Mapping Police Violence (MPV), which is compiled from the three largest and most comprehensive crowd-sourced databases and is currently, to my knowledge, the most comprehensive accounting of police-involved homicides. MPV began in 2013 and contains incident-level information on cases in which civilians die after being intentionally harmed by police officers. The data fields include the agency responsible for the death, circumstances surrounding the death, and demographics of the victim. I aggregate the data at the agency-month level and merge it with the other data.

As a crowd-sourced dataset, MPV likely under-reports the true extent of police-involved homicides. However, it is the best data available on this important issue, and according to at least one measure, it seems to be reliable: the BJS estimates that in 2015, around 1200 police-involved homicides occurred, while the MPV identifies 1106 cases during this same period (Banks et al., 2015).

One of MPV's data sources, Fatal Encounters (FE), extends back to 2000. However, the quality of this data source deteriorates as one moves backwards from 2013 because data on incidents before 2013 was gathered retroactively. Reliable data gathering and cross-examination of cases are difficult for historical events; crowd-sourced inputs are most likely to be reliable for current events. Complicating matters further, prior to 2013, the general public paid less attention to the use of force and the media was less likely than it is today to report police-involved homicides. This data problem is evident in Appendix Fig. A.2, where FE ascends markedly until 2013. Consequently, my analysis of police-involved homicides in the nationwide sample begins in 2013, when better use-of-force data became available.

3.2. Data on BWC adoption

This study uses the differential adoption timing of law enforcement agencies to quantify the effects of BWCs. I gather agencies' adoption decisions from the Law Enforcement Management and Administrative Statistics (LEMAS) survey that has been administered by the Bureau of Justice Statistics (BJS) every three years since 1987. Garicano and Heaton (2010) use the LEMAS survey to construct a panel dataset of police departments and examine the effects of information technology on law enforcement productivity. Similarly, I employ the Body-Worn Camera Supplement (LEMAS-BWCS), which was administered for the first time in 2016 (see Appendix Section B). Released in 2019, this novel data has not been used extensively in academic research. This survey was distributed to all agencies that employed more than 100 officers and to a nationally representative sample of smaller law enforcement agencies in the US. The data include about a quarter of the total agencies extant in 2016. The LEMAS-BWCS contains responses from heads of agencies on topics that range from the current status of BWC use and reasons for adoption to obstacles they faced in BWC implementation. Most importantly for the purposes of this research, it indicates when (year and month) agencies obtained BWCs (Question 11 in the survey).

Since the LEMAS-BWCS survey focused specifically on agencies' experiences with BWCs, all agencies responded to the question regarding whether they had acquired BWCs. Of the agencies that indicated they had implemented BWCs, each provided a date for when they first obtained the cameras. Furthermore, all but 5 of the 1001 adopters reported the number of BWCs they had in service.

Because the LEMAS-BWC supplement is fielded by the Department of Justice to the chiefs of police agencies that have established data relationships, the data are likely to be fairly reliable. Nonetheless, because the data are new, I performed a validation exercise.[6] To the extent that there is a measurement error in dates reported by the chiefs' offices, the nonclassical measurement error would bias the estimates toward zero (Aigner, 1973; Dynarski, 2003; Khwaja and Mian, 2005).

3.3. Data on other law enforcement outcomes

Data on index crimes and arrests come from the Uniform Crime Reporting (UCR) database maintained by the Department of Justice. Index crimes are serious offenses tracked by the FBI because they are most likely to be reported and allow for comparison across different departments. They include homicide, forcible rape, robbery, burglary, aggravated assault, larceny, motor vehicle theft, and arson. Arrests encompass arrests for index crimes as well as arrests for other less serious offenses (non-index arrests), and I use disaggregation by these two categories. The UCR database also includes data on felony assaults against police officers. The previous literature that has used this data documents substantial inconsistencies and inaccuracies, particularly at the monthly level (Neal and Rick, 2014; Mello, 2024), and it notes the importance of cleaning this data thoroughly. My data cleaning method resembles that used by other researchers. The specific procedures used are described in more detail in Appendix Section A.C.

4. Empirical strategy

4.1. Event study around adoption

My primary empirical approach exploits the staggered adoption of BWCs by law enforcement agencies using time variations within the sample of adopters. The availability of data on the exact adoption dates of BWCs and high-frequency monthly data on police activities further helps finely tease out the treatment effects of BWC adoption from alternative explanations.

The primary objective of this paper is to credibly attribute changes in policing variables of interest to the adoption of BWCs by different agencies. However, it is important to note that BWC adoption is not random and may be influenced by variables that could be linked to the outcomes of interest. Table 1 compares the characteristics of departments based on their adoption status. The adopting agencies in this

[5] Currently only 750 of approximately 17,985 agencies voluntarily submit justifiable homicides (as defined by agencies) to the SHR, and the Bureau of Justice Statistics reports that the ARD misses between 30 and 50 % of deaths (Banks et al., 2015; Finch et al., 2019). The BJS claims that data from the ARD do not meet BJS' quality standards and it suspended data collection in 2014. The ARD does not overlap with the time period studied in this paper. Although the SHR covers the study time period, I do not use this data because it suffers from substantial omission and is likely to under-report cases that the public might regard as not justifiable.

[6] I collected the earliest dates on which agencies purchased or piloted body cameras. To do this, I randomly selected 200 agencies from the list of agencies that said they had body cameras on LEMAS. Of the 64 agencies that I located sources, the median difference between the collected dates and the LEMAS data is one month (with a mean of 6 months).

Table 1
Comparison of adopters and non-adopters.

	Adopt vs non-adopt			Adopt: pre- vs post- 06/2015		
	(1) Adopters	(2) Non-adopters	(3) Diff	(4) Pre	(5) Post	(6) Diff
Population size (10 K)	7.61	3.19	4.43***	7.08	8.15	−1.07
	(33.98)	(8.51)	(1.10)	(26.51)	(40.13)	(2.15)
Resident % White	76.17	81.79	−5.61***	76.37	75.98	0.39
	(20.38)	(18.46)	(0.81)	(20.68)	(20.10)	(1.29)
Resident % Black	13.67	8.67	5.00***	13.78	13.56	0.22
	(18.71)	(15.37)	(0.72)	(19.22)	(18.20)	(1.18)
Resident % Hispanic	13.60	10.88	2.72***	12.49	14.72	−2.23
	(18.02)	(15.74)	(0.71)	(17.40)	(18.56)	(1.14)
Resident % male	48.73	48.76	−0.03	48.67	48.78	−0.10
	(3.86)	(3.24)	(0.15)	(4.12)	(3.59)	(0.24)
Household income ($1 K)	48.97	56.69	−7.72***	48.03	49.92	−1.89
	(21.99)	(25.02)	(0.97)	(23.05)	(20.84)	(1.39)
% College attainment	24.17	27.62	−3.45***	23.75	24.59	−0.84
	(14.13)	(16.66)	(0.63)	(14.48)	(13.78)	(0.89)
% in poverty	17.73	14.04	3.69***	18.21	17.24	0.98
	(9.35)	(9.09)	(0.38)	(9.65)	(9.02)	(0.59)
Index crime rate per 1 K	2.71	2.17	0.54***	2.61	2.81	−0.20
	(1.84)	(1.71)	(0.08)	(1.80)	(1.87)	(0.12)
Citizen deaths per 100 k	0.03	0.01	0.02	0.03	0.03	−0.01
	(0.28)	(0.14)	(0.01)	(0.23)	(0.33)	(0.02)
Officers (10 s)	18.40	5.68	12.72**	17.06	19.75	−2.70
	(127.50)	(16.47)	(4.05)	(86.40)	(158.52)	(8.08)
N	1001	1379	2380	502	499	1001

Notes: Table compares the characteristics of departments based on their adoption status. The data on agencies are derived from the five-year estimates of Census places and county subdivisions in the 2013 American Community Survey. The “adopting agencies” in my sample are those that implemented BWCs between January 2014 and June 2016, and I compare them with agencies that did not adopt BWCs by June 2016. Then, for the next three columns the table cuts the set of adopters in half around the median time of adoption (June 2015). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

context are those that adopted BWCs in 2014 or later and obtained at least one BWC. The data for these agencies are sourced from the five-year estimates of Census places and county subdivisions in the 2013 American Community Survey. Adopting agencies bear distinct characteristics from those of non-adopting agencies. For instance, compared to the cities of non-adopting agencies, those with adopting agencies, on average, have higher minority populations by 5.6 percentage points,[7] higher index crime rates by 0.54 per 1000 capita, higher poverty rates by 3.7 percentage points, and a lower proportion of college graduates by 3.5 percentage points.

Instead of comparing agencies that adopted BWCs to those that did not, I focus on comparing among agencies that adopted at different points in time. While adoption timing may not be random, I rely on the assumption of “parallel trends” in baseline potential outcomes. This means that among agencies that adopted BWCs, baseline variables would have evolved similarly in the absence of BWC adoption, and BWC adoption acted as a shock to the policing outcomes. This shock existed because it was hard to control the timing of adoption that stemmed from bureaucratic process of approval (as discussed in Section 2.2) and was unlikely to coincide with other changes in the departments. This assumption is more likely to be valid in a short period of time.

In Table 1, I cut the set of adopters in half around the median time of adoption (June 2015) and separated the adopters by whether they adopted before or after June 2015. Across all the variables that I previously examined, the adopters exhibit much closer similarity to each other than between adopters and non-adopters. The differences that existed such as population, proportion White, proportion Black, income, college attainment, poverty, and prior police-involved homicides close their gaps. None of the variables have statistical differences. These findings indicate that early- and later adopters are comparable.[8]

Beyond observable factors, I also assess how agencies could differ along unobservable characteristics by treatment status and adoption date. As noted by Montiel Olea et al. (2021) and Leung-Gagné (2024), unobservable determinants such as “patterns of officer discretion, policy variation, and organizational features and policing’s role in the social structure” can meaningfully influence outcomes. These dimensions are inherently more difficult to measure. To explore them, I leverage the 2016 LEMAS core survey, which was fielded the same year as the BWC supplement, to examine organizational characteristics that are typically unobservable in other data sources. The overlap between agencies that appear in both datasets is 31 %. Of this smaller set of agencies, I examine training, community policing, written documentation of use of force, and civilian complaint boards. In Table A.3, I compare these dimensions between adopters and non-adopters, as well as between early and later adopters. I find that the differences are notably smaller, particularly concerning the written documentation required for force types. Among adopters, management practices around the use of force are more comparable, which supports the DID approach where the comparison sets are similar.

It is important to note that the empirical strategy in this paper does not rely on randomness in adoption timing. Instead, I rely on a weaker assumption that there are parallel trends between the comparable groups, and that BWC adoption acted as a shock to policing outcomes. Although having comparable baseline characteristics is not a requirement for a DID design, the similarity between the two groups increases confidence that the outcomes would have evolved similarly in the absence of BWC adoption.

[7] It is worth noting that among adopting agencies, White individuals do not necessarily constitute the majority; in fact, 13.6 % of adopting agencies have less than 50 % White populations.

[8] Table A.2 shows similar patterns the main adopter sample adopting more than 40 %.

In the event study, I focus on the 12 months before and after adoption to observe changes in policing variables. The police-related homicides data range from January 2013, when the MPV data start, to 2020. To conduct a balanced event study one year before and one year after adoption, I analyze agencies that adopted BWCs between January 2014 and June 2016. The reason for ending in June 2016 is because the LEMAS survey was conducted at that time, and there is no further information available about adopters beyond that date.

To estimate the effects of BWC adoption in regression form, I estimate the following regressions for agency j and month t:

$$Outcome_{jt} = \sum_{\tau=-12}^{12} \beta_\tau D_{jt}^\tau + \phi_j + \delta_t + \epsilon_{jt}, \tag{1}$$

where D_{jt}^τ is a dummy variable that indicates whether the agency adopts BWCs in τ months. ϕ_j and δ_t denote agency and calendar-month fixed effects. The main coefficients of interest β_τ estimate the divergence in outcome variables net of changes in other adopting agencies after adjusting for the covariates. I weight all my regressions by the population count the agency serves, and I normalize the event study coefficient for $\tau = 1$ to be zero. I cluster standard errors at the agency level.

To summarize the results, I pool the estimates pre- and post- event in the following fashion:

$$Outcome_{jt} = \beta Post_{jt} + \phi_j + \delta_t + \epsilon_{jt}, \tag{2}$$

where I replace the event time variables with a single indicator variable $Post_{jt}$ that takes a value of 1 if month t is after adoption. In the regressions, I check the identifying assumption by observing any pre-trends that preceded the adoption of BWCs, or $\tau < 0$.

In recent econometrics literature, researchers have identified potential issues with negative weights in multiple-period difference-in-difference estimators. This becomes particularly relevant when treatment timing is staggered and there is heterogeneity in treatment effects within units over time or between groups of units treated at different times (Athey and Imbens, 2022; Borusyak and Jaravel, 2018; Callaway and Sant'Anna, 2020; Goodman-Bacon, 2019). In light of these concerns, I adopt two approaches. First, I use the approach of Deshpande and Li (2019) and compare the evolution of outcome variables of early adopters to those of later adopters. This stacked DID design provides an organic control group and does not use already-treated units as a control. Second, I use the approach suggested by Callaway and Sant'Anna (2020) and estimate the group-time average treatment effect, defining groups based on the month when agencies adopted BWCs.

4.2. Data restriction based on adoption rate

The decision to adopt BWCs, as recorded in the data, is not binary. This set of agencies includes those that might have adopted BWCs but only deployed the technology to a small portion of their force, which may not have a meaningful impact, particularly on outcomes as serious as police-involved homicides. To address this and achieve greater precision, I focus on examining the effects of BWC implementation at agencies that adopted the technology at rates above a certain threshold.

I define agencies with substantive exposure as those that deployed BWCs to 40 % or more of their officers by June 2016, relative to the total number of officers in 2016. I use a data-driven procedure to justify this cutoff, detailed in Appendix Section A.C.2. This includes comparing the distribution of adoption rates between agencies that completed full deployment and those with incomplete rollouts, and identifying the threshold that best differentiates the two groups. To further support this approach, I present separate estimates across finer adoption-rate bins and other heterogeneity dimensions. I also present results across a broad range of potential cutoff values.

4.3. BWC expansion trajectory

This paper focuses on adopters that adopted at 40 % or greater sample as agencies with meaningful exposure to BWCs. However, because the analyses estimate the effects of BWCs in the 12 months after an agency first adopted BWCs, not the 12 months after an agency reached meaningful level of deployment, it is important to understand the scale at which agencies expanded their BWC programs within the first year. To assess the speed of expansion and the potential extent of underestimation in my results, I use two approaches, each with its own advantages and limitations.

First, using the LEMAS data, I examine how much agencies expanded their BWC programs within the first year of adoption. Since the LEMAS survey was conducted in June 2016, focusing on agencies that adopted BWCs between June 2015 and June 2016 provides insight into their initial rollout. Among these agencies, the median adoption rate within 12 months was 55 %. In contrast, for agencies that adopted BWCs before June 2015, the median adoption rate by June 2016 was 75 %. While agencies adopting within one year of the survey had lower initial coverage, their rollout within that period was still substantial. A caveat here is that agencies that adopted later in the sample may have had a different adoption trajectory.

Second, I gathered additional data on BWC adoption from newspapers and online sources, focusing on agencies in the largest LEMAS strata (i.e., those with more than 100 officers). This restriction ensures data coverage, as changes to larger agencies are more likely to be mentioned in news reports. I further limit the data collection to agencies that adopted BWCs for at least 40 % of their force, aligning with the main threshold used in the paper. Among the 80 agencies meeting these criteria, I successfully found rollout information for 76.

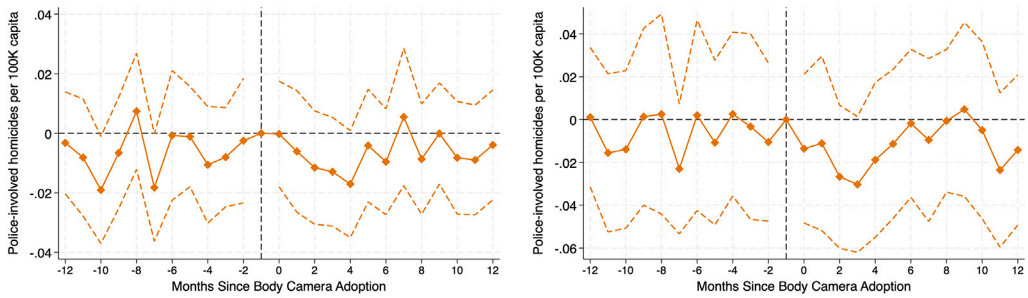
Of the 76 agencies with rollout data, I found reported initial deployment rates for 61 agencies. The median initial deployment level was 58 %. This suggests that a substantial share of BWCs was deployed at the outset. The interquartile range was 27 %–75 %. Additionally, I collected information on planned subsequent rollouts for 23 agencies. Incorporating these plans, the estimated median adoption rate within one year of initial deployment was 71 %. For comparison, the median adoption rate for these agencies in the LEMAS data was 72 % by June 2016, closely matching the estimates from my collected data. Together, these findings suggest that a significant portion of BWC adoption occurs within the first year, and this study likely reflects a substantial share of the adoption level recorded in the June 2016 LEMAS survey.

5. Results on police-involved homicides

5.1. Effects on police-involved homicides

Using a balanced panel of 1001 agencies that adopted BWCs between January 2014 and June 2016, I estimate Eq. (1). The event-time estimates, β_τ, are presented in the left panel of Fig. 3. When analyzing the entire set of 1001 agencies that adopted BWCs at any rate, we do not observe significant changes before and after adoption. In Fig. 3, I present the event study estimates for the main adopter sample of agencies that deployed BWCs to at least 40 % of their forces by June 2016, when the LEMAS survey was conducted. In the sample with a 40 % adoption rate, no noticeable pre-adoption trend can be observed, but a sudden drop in police-involved homicides is observed after adoption, although overall we have noisy estimates. This decrease is estimated to be 0.008 (p-value: 0.18), representing a 25 % reduction from the pre-adoption mean in the three months preceding the adoption. At the adoption rate of 40 % or more, the confidence interval allows us to rule out effect sizes larger than −59 % and +9.3 %. As discussed in Section 4.2, for my main analysis I focus on agencies that adopted in a meaningful way at a rate 40 % or higher and later present robustness for an extensive list of different thresholds.

One advantage of analyzing BWC adoption at the national level is the ability to observe heterogeneity based on the environments in which BWCs were implemented. I conduct heterogeneity analysis informed by previous studies on BWCs. In a meta-analysis, Lum et al. (2020) identify moderating variables that could explain differences in findings across various studies, including agency characteristics and differences in study



(a) All Adopters (b) Main Adopter Sample: Adopting 40% or more

Fig. 3. Effects of BWCs on police-involved homicides. The figure presents the event study estimates for police-involved homicides, using data from Mapping Police Violence. In the first panel, the sample includes all agencies that adopted BWCs at any rate. In the second, panel, I restrict the sample to agencies that deployed BWCs to at least 40 % of their officers. All regressions include time fixed effects and agency fixed effects, and are clustered at the agency level.

methodologies, such as degrees of spillover effects.[9] Regarding agency characteristics and policy, they identify two key moderators. First, reductions in complaints were greater in agencies that had troubles and concerns prior to BWC adoption. This finding is also echoed by White (2019), who notes that "troubled agencies that adopt BWCs may see the Rialto-like declines in use of force and citizen complaints because there is much room for improvement. Highly professional agencies with robust employee selection, training, policy, supervision, and accountability processes will probably not experience those same large declines because there is less room for improvement." Second, agencies with more stringent activation requirements had greater reductions in the use of force.

To explore heterogeneity based on the potential for improvement, I analyze prior police-involved homicides, which are intended to capture unobservable management and cultural factors that influence excessive police-involved homicides. These factors include "selection practices, training, promotion standards, disciplinary procedures, and organizational culture," changes to which would be "followed by decreases in police homicides (Montiel Olea et al., 2021). I calculate residualized prior police-involved homicides, after accounting for prominent predictors such as population, racial composition, gender distribution, income, poverty rate, educational attainment, and index crime rates. I then rank the Census divisions based on these residualized values.[10]

The resulting rankings are consistent with prior work. Leung-Gagné (2024) also studies risk adjusted police-involved homicides. After accounting for trauma care access, violence against officers, and crimes, in 711 departments serving 50,000 or more residents, the author finds that deadliness is strongly associated with location in Western Census divisions (west of the Mississippi) and more racially segregated jurisdictions. I also find that Western Census divisions have higher prior levels of police-involved homicides. Leung-Gagné (2024) attributes these unexplained factors there to "patterns of officer discretion, policy variation, and organizational features and policing's role in the social structure, which lend itself to opportunities for intervening in police department practices, policing writ large, and social policy to reduce police homicides without reducing officer safety and effectiveness."

I separately examine the impact of BWCs in divisions with high and low police-involved homicides.[11] Fig. 4 reveals clear disparities: divisions with higher prior levels of police-involved homicides experienced significantly larger reductions, while agencies in regions with low baseline levels saw virtually no effect from BWC adoption. Among the former group, the decline is 0.019 (p-value: 0.044), equivalent to 46.3 % of the pre-adoption mean, whereas the latter group exhibited null declines. The confidence interval rules out effect sizes greater in magnitude than −93.6 % and effect sizes less in magnitude than −1.2 %.

I further explore the role of BWC activation policy. In agencies that have implemented BWCs, officers still have discretion over when to activate and record with BWCs. Table A.1 highlights considerable differences in activation policies. For example, while the vast majority (up to 94 %) of agencies require activation during one-on-one engagements with civilians, fewer agencies mandate activation for other scenarios that still involve officer discretion, such as public event policing, special operations, transporting offenders, and public order policing. The requirement drops to 26.4 % for policing public events.

I categorize agencies into two groups based on their activation policy: those that required officers to activate BWCs in 7 or more types of events out of 10 possible choices, and those with less stringent requirements.[12] As shown in Fig. 4, agencies with more comprehensive activation requirements drive the results, with a decrease of 0.016 (37.2 % of the pre-adoption mean; p-value: 0.031) in police-involved homicides, while those with less stringent requirements did not experience a significant drop.[13] The confidence interval excludes effect sizes greater than

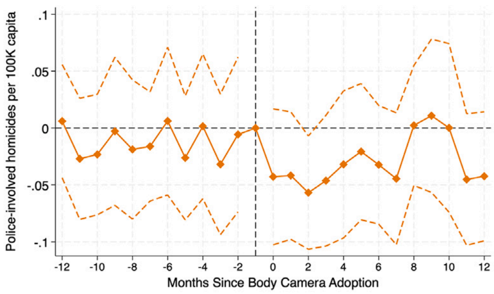
[9] Single-agency studies face challenges controlling for these effects when BWCs are randomized to individual officers or shifts. In such setups, officers with BWCs often work alongside control group officers without them. When BWCs are randomized by shifts, the same officers may alternate between working with and without BWCs on different days, complicating the assessment of their true impact. For example, some studies that report significant reductions in the use of force (e.g., Ariel et al., 2015) also observe similar reductions in the control group compared to the pre-intervention period, indicating substantial spillover effects.

[10] An alternative way of grouping agencies based on their predicted propensity to experience police-involved homicides. However, most police departments in the sample report no homicides in a given year, and 82 % have zero over the entire sample period. The maximum any single agency experiences is three. This poses two challenges compared to classification using Census divisions. First, given the rarity of police-involved homicides, directly predicting an agency's risk adds considerable noise. Even if some agencies have an underlying risk, their observed homicide count is often zero, leading to potential misclassification as low-risk. Second, classifying agencies based on their own police-involved homicide history makes the distinction somewhat mechanical—departments with low or zero homicides cannot experience further declines, as they are already at the lower bound.

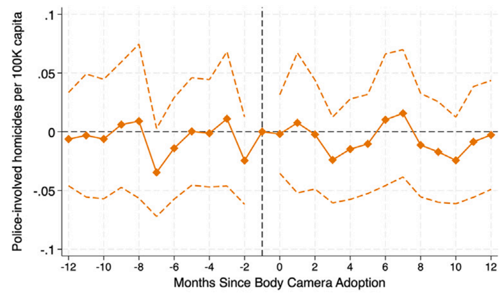
[11] Census divisions with low prior levels are: East South Central, Middle Atlantic, South Atlantic, East North Central, and New England. Census division with high prior levels are Pacific, Mountain, West South Central, and West North Central.

[12] The survey asks in what situations officers are required to turn on their body-worn cameras? The options provided are as follows: responding to routine calls for service, traffic stops, officer-initiated citizen contact, firearms deployments, public order policing, policing public events, criminal investigations, special operations, executing arrest or search warrants, transporting offenders.

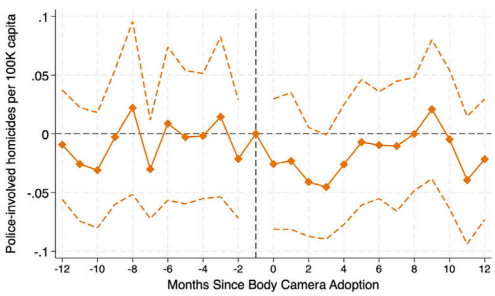
[13] Given the heterogeneity, one may consider adjusting p-values for the heterogeneity analyses. Applying a Bonferroni correction for the subgroups defined by



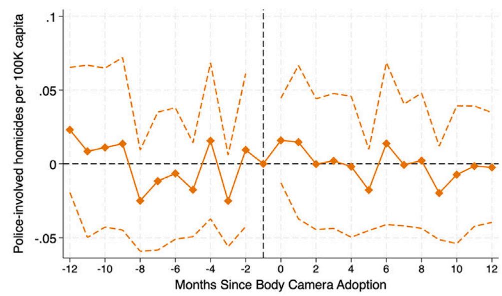
(a) Prior Levels: High



(b) Prior Levels: Low



(c) Required On: High



(d) Required On: Low

Fig. 4. Effects of BWCs on police-involved homicides: heterogeneity. The figure presents the event study estimates for heterogeneity analysis of police-involved homicides, focusing on agencies that adopted BWCs for at least 40 % of their officers. The top two panels examine whether agencies located in regions with high prior police-involved homicides experienced different effects from agencies in other areas. To do this, I adjust city-level police-involved homicides based on baseline (2013) city characteristics such as race, population, gender, education levels, poverty, and crime rates. Then, I calculate the population-weighted mean of these residuals by census division and separately examine the impact of BWCs in divisions with high and low police-involved homicides. In the bottom panels, I explore whether agencies that adopted stricter activation requirements experienced different effects from BWC adoption. I categorize agencies into two groups based on their activation policy: those that required officers to activate BWCs in seven or more types of events out of ten possible choices, and those with less stringent requirements. All include time FEs, and agency FEs, and the data are clustered at the agency level.

−71.2 % and less than −3.4 %. This underscores the importance of an agency's BWC policy in shaping the effects of BWC implementation. Together with the heterogeneity by prior incident levels, they help explain the variation observed in earlier RCTs. The broader and more representative agency sample used in this study (see Appendix Section A.E) strengthens the generalizability of these insights.

5.2. Heterogeneity by civilian race

Because BWCs have been introduced as a policy tool in response to police violence against minority citizens, it is important to highlight and further analyze heterogeneity based on the race of the civilians. In Table 2, I break down citizen deaths by racial groups. The pre-adoption means are 0.019 for Whites and 0.081 per 100,000 capita for Black individuals. For White individuals, police-involved homicides drop by around 0.009 after adoption. However, for Black individuals, we do not estimate a significant drop in police-involved homicides. While we estimate a 25 % drop for Hispanics, this does not reach statistical significance.

One potential explanation for this racial heterogeneity is that BWCs may influence police behavior through different channels depending on the racial group. The theoretical effects of BWCs on the use of force are ambiguous. BWCs may reduce the use of force if officers improve their skills or become more cautious to avoid mistakes. Conversely, the use of force might increase if officers view BWCs as a protective measure in controversial situations, leading them to be more assertive in their policing activities.

To explore this further, I examine whether BWC adoption leads to differential changes in arrest rates for Black and White individuals. Hispanic arrests are not analyzed, as this data is not coded in the UCR. As shown in Table A.4, I find that index arrests increase more for Black arrests than for White arrests.[14] This finding aligns with the explanation that police officers may perceive BWCs as protective when interacting with Black civilians, resulting in more assertive policing. It is possible that officers, who may have been hesitant to use severe force prior to BWC adoption felt more confident doing so afterward, believing that the BWCs would provide protection for their actions. This does not imply that problematic policing against Black civilians did not exist. The period under study coincided with heightened public outcry over use-of-force incidents involving minorities, which may have made officers more fearful of using force.

5.3. Sensitivity checks for results on police-involved homicides

I examine the sensitivity to alternative thresholds more directly by re-estimating treatment effects using a broader set of adoption thresholds. In Table A.5 Column 1, I present estimates across nine different thresholds for all agencies. The coefficients suggest that agencies adopting above certain lower thresholds (e.g., 20 %–60 %) show similar magnitudes, although they are mostly imprecise. This similarity occurs because agencies with higher adoption rates tend to have larger treatment effects. Fig. A.3 shows the results of re-estimating treatment effects across more granular groups of ten deciles of adoption rates. The coefficients

prior levels of police-involved homicides, I multiply the p-value by 2 results in an adjusted p-value of 0.088 for agencies with high police-involved homicides. Similarly, for the subgroups defined by activation requirements, multiplying the p-value by 2 results in an adjusted p-value of 0.062 for agencies with high activation requirements. More discussion can be found in Section A.D.1.

14 A more disaggregated results on arrests can be found in Table A.16.

Table 2
DID effects on police-involved homicides in the US.

	DID	Dep. mean	Num. city	Obs.
Main adopter sample	−0.008 (0.006)	0.032	593	32,022
By agency type:				
Prior levels: high	−0.019** (0.007)	0.041	278	15,012
Prior levels: low	−0.000 (0.009)	0.024	315	17,010
Required on: high	−0.016** (0.007)	0.043	315	17,010
Required on: low	0.007 (0.009)	0.016	278	15,012
By race:				
White	−0.009* (0.005)	0.019	593	32,022
Black	0.011 (0.022)	0.081	593	32,022
Hispanic	−0.005 (0.012)	0.020	593	32,022

Notes: Table shows the DID estimates of police-involved homicides in the national data. All estimates and means are at the monthly level. The sample includes agencies adopting between January 2014 and June 2016. All regressions include time FE and agency FEs, and standard errors are clustered at the agency level. The regressions are weighted by population the agency serves. For outcomes by racial groups, the weights are the corresponding population of the racial group. Also the table shows DID estimations of the effects on police-involved homicides by different types of agencies. See Fig. 4 for definitions of agency types. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

indicate that agencies adopting at low rates (e.g., up to 30 %) do not show reductions. The first decile to show a negative estimate is the 30 %–40 % group, and in deciles with adoption rates above 50 %, all but one show negative estimates.

In Table A.5 Column 2, I present DID estimates for each threshold, focusing on agencies within the group of census divisions that have high prior police-involved homicides. Across all thresholds from 20 % upward, the coefficients remain relatively stable and are larger than those observed for the 10 % adoption rate. A range of thresholds (20 %–60 %) has similar magnitudes and is statistically significant at the 5 % level. A similar pattern is observed in Column 3 when restricting the analysis to agencies with stringent activation requirements (activation requirements in 7 situations or more); the coefficients for the 30 %, 40 %, 50 %, and 60 % thresholds are higher than the 10 % threshold and are statistically significant. This analysis serves as a useful robustness check, showing that a broad range of cutoffs (20 %–60 %) indicating meaningful exposure yields similar effects.

I also present robustness checks using different thresholds of activation requirements in Table A.6. We see that, using higher thresholds (e.g., 8 or 9) for activation requirements tends to yield larger DID coefficients, and these are larger than all other thresholds below.

I assess the sensitivity of the DID results using various alternative specifications. While we have utilized population-adjusted outcomes, another approach is to adjust by the number of officers. One might argue that an agency hires additional officers based on the need for policing activities, and changes in policing and risk incidents could be influenced by the size of the department. Table A.7 presents the results from a specification that uses police-involved homicides per officer as the dependent variable, rather than per capita. In the main adopter sample, police-involved homicides per 100,000 officers decrease by 4.6, or 27.4 % of the pre-adoption mean, which aligns with the magnitude observed in my primary findings. However, this measure is imprecisely measured. On the other hand, I find that the reductions are mostly driven by agencies located in regions with prior high levels of police-involved homicides (55.5 % of the mean; p-value: 0.026), and agencies with high requirements for activating cameras (45 % of the mean; p-value: 0.016). Given that the outcome variables include a non-significant amount of zeros, I implement the Poisson pseudo-maximum likelihood (PPML) estimator for my main analysis. As a pseudo-maximum likelihood estimator, PPML does not require the data to follow a Poisson distribution (Santos Silva and Tenreyro, 2010). As seen in Table A.8, the estimates obtained through this approach are consistent with the main estimates, confirming the robustness of my findings.

I also conduct several tests to see how the results change when I exclude very large departments. Very large and very small departments are not directly comparable. The largest cities have significantly larger populations than other cities, and when weighted by population, they can disproportionately influence the results. I re-run the analysis dropping agencies with populations in the millions. The results do not change much from the main table (Table A.9). Because the sample still contains relatively large agencies such as San Diego and Montgomery County police departments that have population in the millions I further drop agencies at the 99th percentile in that sample (population size greater than 318,189). I find that the results are similar in Table A.10.

Finally, recent contributions to the method of difference-in-differences (DID) literature have pointed out potential problems with staggered treatment timings (Goodman-Bacon, 2019; Callaway and Sant'Anna, 2020; Borusyak and Jaravel, 2018; Sun and Abraham, 2021; De Chaisemartin and D'Haultfoeuille, 2018). To address this concern, I use two methods.

First, I follow the approach of Deshpande and Li (2019) and compare the evolution of outcome variables of early adopters to those of later adopters. To conduct a balanced event study one year before and one year after adoption, I create 18 *separate* datasets for each month between January 2014 and June 2015 in which BWC adoption occurred. In each dataset, agencies that experienced the current adoption are labeled as treated agencies while agencies that adopt after over a year are labeled as clean control agencies (not yet treated and would not be treated until one year later). Event times are specified as relative to the month of the current adoption timing of the treatment agencies. Also, event months of the control group agencies are defined relative to the current adoption timing of the corresponding treatment agencies in the dataset. Finally, I stack all the datasets into one dataset.

For illustration of the stacked sample, I refer the reader to Fig. A.4. Treatment agencies that adopted BWCs in August 2014 are matched with control agencies that adopted BWCs one or more years after August 2014 (i.e., bottom gray bar adopting between August 2015 and June 2016). Event months for the control agencies are given the event month of agencies adopting in August 2014, and these treatment and control agencies are compared 11 months before and after that time, as shown in the top gray bar. Note that throughout the comparison period, the control agencies did not adopt BWCs. This stacked DID design provides an organic control group and does not use already-treated units as a control. Table 3 presents the results from this analysis.[15] The magnitudes of declines in police-involved homicides are similar, and these effects are primarily driven by agencies in regions with high prior levels (37 % reduction; p-value: 0.14) and agencies that required more stringent activation (41.3 %; p-value: 0.04).

Second, I follow the corrective method introduced by Callaway and Sant'Anna (2020). This procedure involves estimating DID for each

[15] The estimating equation is the following:

$$Outcome_{jt} = \gamma Treated_j + \sum_{\tau=-11}^{11} \alpha_\tau D_{jt}^{\tau} + \beta(Treated_j \times Post_{jt}) + \phi_j + \delta_t + \epsilon_{jt}, \quad (3)$$

where D_{jt}^{τ} is a dummy variable that indicates whether the agency adopts BWCs in τ months. $Treated_j$ is an indicator variable equal to 1 if agency j is a treated agency that adopts BWCs early. ϕ_j and δ_t denote agency and calendar-month fixed effects. The main coefficients of interest β estimate the divergence in outcome variables net of changes in control agencies after adjusting for the covariates.

Table 3
Stacked DID: effects on police-involved homicides.

	DID	Dep. mean	Num. city	Obs.
Main adopter sample	−0.010 (0.007)	0.034	593	92,049
By agency type:				
Prior levels: high	−0.016 (0.009)	0.043	278	38,045
Prior levels: low	−0.006 (0.010)	0.028	315	54,004
Required on: high	−0.019** (0.009)	0.046	315	49,519
Required on: low	0.010 (0.010)	0.015	278	42,530

Notes: Table shows the stacked DID estimates of police-involved homicides in the national data. All estimates and means are at the monthly level. The sample includes agencies adopting between January 2014 and June 2016. All regressions include time FEs, and agency FEs, and standard errors are clustered at the agency level. The regressions are weighted by population the agency serves. Also the table shows DID estimations of the effects on police-involved homicides by different types of agencies. See Fig. 4 for definitions of agency types. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

adoption month cohort with control groups that have not yet adopted BWCs. In Fig. A.5, I find that there were visual trend breaks around BWC adoption. Table A.11 summarizes the results. The estimates are generally larger and more imprecisely estimated compared to the main results, but the overall patterns remain consistent. I also find that the reductions in police-involved homicides are still driven by agencies located in regions with higher prior levels of police-involved homicides and those that required more stringent activation policies.

These additional analyses are consistent with my main results that BWC adoption led to reductions in police-involved homicides, particularly in agencies with higher prior levels of police-involved homicides and more stringent activation policies.

5.4. Were there concurrent changes?

I consider potential factors other than BWC adoption that could account for the observed reductions in police-involved homicides. A concern might be that agencies could have simultaneously implemented BWC adoption along with other reforms, such as enhanced training or modifications to use-of-force policies. These parallel initiatives could drive the observed changes in use of force. Furthermore, a high-profile use-of-force incident might exert considerable social pressure to both adopt BWCs and reduce police-involved homicides concurrently. For these alternative explanations to account for the results, however, the timing of such changes would need to align precisely with BWC adoption. This is unlikely, as BWC adoption usually involves complex bureaucratic and logistical processes that take over a year on average. Nevertheless, to address this hypothesis, I consider in turn the possibility of reform bundling and simultaneous events.

5.4.1. Reform bundling

A key concern is that BWC adoption may have been bundled with other reforms, such as officer training or internal policy changes. To assess this, I use historical purchase order data from GovSpend, a commercial vendor that compiles procurement records for government agencies. Additional details about the data and matching procedures are provided in Appendix Section A.D.2.

I focus on 112 BWC-adopting agencies for which complete purchase order data are available for the 12 months before and after adoption. Fig. 5 shows a clear ramp-up in BWC-related purchases leading into the adoption month, consistent with phased implementation. After the BWC adoption month, more BWC purchase orders are observed. These purchases might include BWC gear and evidence storage systems sold by BWC contractors, or they could be due to repeat orders where agencies purchased BWCs in several installments. For example, among the 49 agencies that made at least one BWC-related purchase, 30 made multiple purchases—suggesting repeat orders or related procurement of accessories and storage systems. In contrast, training purchase trends remain flat. These patterns suggest that BWC adoption generally occurred as a standalone intervention rather than as part of broader reform packages.

Additionally, I consider whether non-purchase reforms may have influenced officers' in-field behavior. As detailed in Appendix Section A.D.2, I examine leadership changes and policy reforms. Using news sources, I track police chief appointments and policy shifts for 80 large agencies. The median gap between BWC adoption and these changes is substantial, and excluding agencies with overlapping timing yields results similar to the full sample—suggesting these reforms do not drive the main findings. I also assess whether increased documentation requirements, such as investigatory stop reports, reduced enforcement activity. Disaggregating arrests by offense type (following Chalfin et al., 2022), I find no evidence of a decline in quality-of-life or non-index arrests. These results indicate that simultaneous internal reforms are unlikely to explain the observed effects of BWCs.

5.4.2. High-profile incidents and public pressure

I also consider the possibility that a use-of-force incident can create intense social pressures to adopt BWCs and reduce police-involved homicides in the same month. While adoption within such a short time frame is unlikely, extraordinary events could force police chiefs to shorten implementation times.

To assess this possibility, I begin by excluding approximately 10 % of agencies that reported adopting BWCs in response to external mandates (e.g., legislative or judicial pressure) in the LEMAS data. The results remain substantively unchanged—or even slightly stronger (see Table A.12)—suggesting that these agencies are not driving the main findings.

Next, I identify agencies associated with high-profile use-of-force incidents using Google Trends data, following Cheng and Long (2022). Of 4209 civilian deaths in Mapping Police Violence (2013–2016), I matched 32 incidents that generated significant online attention to BWC-adopting agencies. The median gap between each incident and BWC adoption was 17 months, suggesting that even in the most high-profile cases, agencies did not adopt BWCs immediately. As a further check, I exclude agencies that experienced high-profile incidents or Black Lives Matter protests within three months prior to BWC adoption, using data from Elephrame, which compiles information from news outlets, social media, and activist organizations. Again, the results remain consistent (Table A.13).

Finally, existing literature provides an additional benchmark. A well-established literature (e.g., Shi, 2009; Premkumar, 2019; Cheng and Long, 2022)—has found that enforcement activities drop sharply immediately following high-profile incidents. In contrast, this paper finds no such immediate reduction in arrests, further suggesting that high-profile incidents are not the driver of the observed effects.

Taken together, the evidence does not suggest that BWC adoption was bundled with broader reform packages or that it coincided with police scandals that exerted downward pressure on police-involved homicides.

6. Mechanisms behind the reduction in police-involved homicides

6.1. Changes in policing effort

I investigate potential mechanisms underlying the reduction in police-involved homicides, drawing on the framework in Appendix Section A.B. One possibility is that officers reduced their policing efforts to avoid the added scrutiny and accountability associated with being monitored.

A measure of policing efforts is non-index arrests per capita, which includes arrests over which officers have wide discretion, such as drug-related offenses, simple assault, and public order offenses. Fig. 6 shows that before adoption, there is no marked pre-trend. After adoption, there

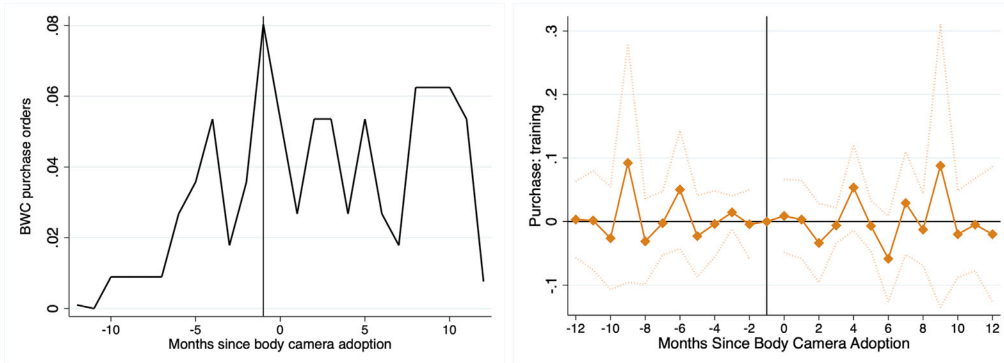
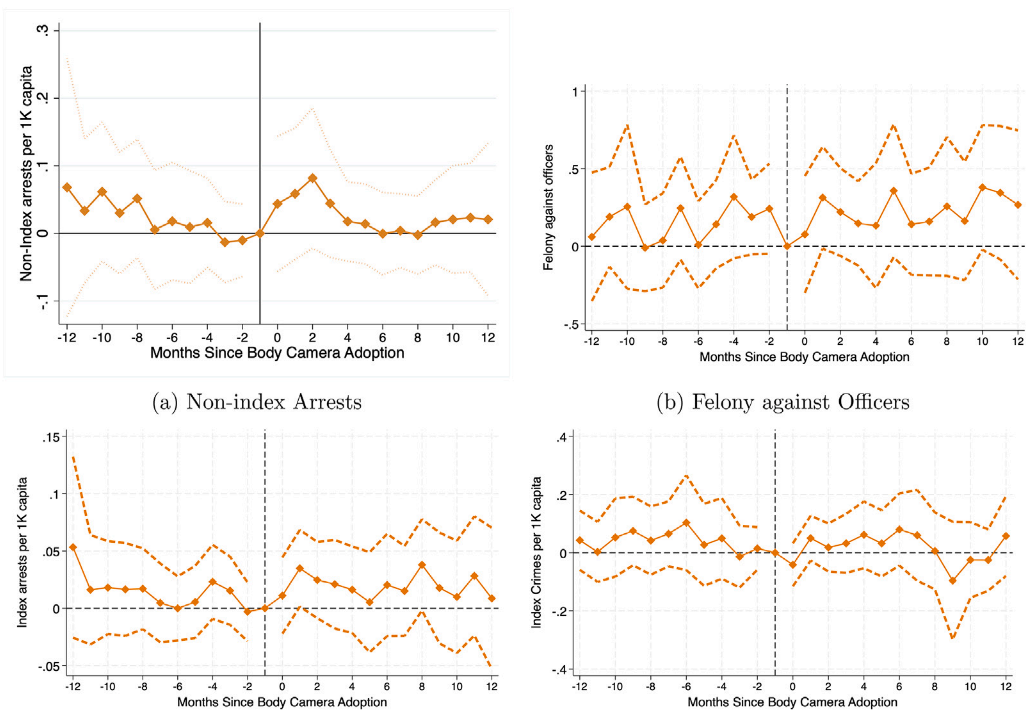


Fig. 5. Purchase orders near BWC Adoption. In the first panel, I plot the raw (simple) averages of whether an agency has a purchase order related to BWCs in a given month. I graph the averages for agencies that adopted BWCs between 2014 and June 2015. In the second panel, I plot the difference-in-difference estimates for purchase orders related to police training around the timing of adoption. The sample includes adopters adopting by more than 40 % of the police force only. All regressions include time FE, and agency FE and cluster at the agency level.



(a) Non-index Arrests (b) Felony against Officers

(c) Index Arrests (d) Index Crimes

Fig. 6. DID effects of body cameras on policing—national. I estimate the effects of BWC adoption on index crimes, index arrests, non-index arrests, and felony against officers in the national data. I restrict to agencies that deployed BWCs to at least 40 % of the officers. All regressions include agency fixed effects and time fixed effects, and standard errors are clustered on the agency level. The regressions are weighted by population.

was a slight short-term increase that, after three months, returned to previous levels. This short-term increase may be the Hawthorne effect arising from officers' awareness of increased monitoring from supervisors. This result shows that the reduction in police-involved homicides does not stem from reduction in policing efforts.

6.2. Improvement in citizen behavior

Another potential explanation for the observed reductions in police-involved homicides centers on changes in citizen behavior. The presence of BWCs may deter hostility by making civilians aware that their actions are being recorded and could be used as evidence. This heightened accountability could encourage more cooperative behavior during police encounters. In turn, officers may respond with less force if they anticipate fewer confrontational interactions. While direct information about the circumstances surrounding police-involved homicides is not available, I use an indirect method to examine assaults on police. If the results are indeed driven by citizens becoming less hostile, we would expect to observe a decline in assaults on police primarily initiated by subjects. However, Fig. 6 shows that we do not observe a decrease in felonies against officers. This evidence is consistent with the hypothesis that the reduction in police-involved homicides is not primarily driven by a reduced danger from citizens.

6.3. Improvement in officer behavior

A likely explanation for the observed reduction in police-involved homicides is that officers and departments adapted their tactics to avoid such incidents. This could occur if officers perceive higher costs of errors due to increased oversight and lower costs of skill development. BWCs serve as a tool for continuous monitoring, which pressures officers and agencies to improve their policing practices. To verify this mechanism, I draw on responses from police chiefs in the LEMAS survey. In the survey, 61 % of chiefs agreed that BWCs improved officer professionalism, 39 % agreed that BWCs help identify instances of officer misconduct, and 72 % agreed that BWCs are useful for officer supervision.

In addition, the heterogeneity analysis based on policy environment also supports the hypothesis of officer behavior change. Agencies that imposed stricter activation requirements, thus leaving less discretion to officers, experienced greater declines in police-involved homicides.

Finally, improvements in officer tactics are likely to be most pronounced in situations where the justification for using force is less clear. To explore this further, I analyze incident-level data on police-involved homicides from the Mapping Police Violence database. Between 2013 and 2019, in cases where the responsible agency was a local police department, 70.2 % of the subjects involved in homicides were reportedly armed; 16 % were unarmed; 6.3 % involved a vehicle as the weapon; and in 7.5 % of cases, the status was unclear. I examined heterogeneity in DID effects by separately estimating the treatment effects in encounters involving armed subjects with weapons (e.g., sword, knife, gun, and baseball bat) compared to those involving unarmed subjects (e.g., toy, toy weapon, cell phone, stick, unarmed, and vehicle cases). The results, shown in Table A.14, indicate that encounters with unarmed subjects experienced larger reductions in force. The point estimates for reductions in unarmed encounters are negative across all types of agencies considered in the heterogeneity analysis. In contrast, reductions in armed encounters were smaller and not statistically significant, though some reductions were observed in specific agency types. Additionally, some of the overall reduction in armed encounters is offset by increases in police-involved homicides in agencies with low activation requirements.

This evidence suggests that BWCs have the potential to reduce the use of force, particularly in situations where there is room for improvement in tactics, such as unarmed encounters. Overall, the findings suggest that BWCs played a role in shaping officer decision-making.

7. Effects on crime and cost-benefit analysis

To provide a more complete picture of the overall welfare impact of BWCs, I explore their effects of BWCs on index arrests and crimes. A key concern is whether reductions in police-involved homicides come at the cost of diminished public safety.

An important performance measure for policing is index arrests, which involve responses to serious crimes, such as aggravated assaults, robbery, and murder. We observe in the second panel of Fig. 6 that compared to pre-adoption, after BWC adoption, index arrests do not change significantly. The trends for index crimes also show little change surrounding BWC adoption. Table A.15 summarizes the results for arrests and crimes. Overall, the findings consistently indicate that there are no statistically significant changes in index crimes and arrests following BWC adoption. For index arrests, those with low prior levels of police involved homicides and low activation requirements had more positive index arrests than the other departments. For agencies with low activation requirements, the estimate of 0.065 (12 % of the mean) obtains a statistical significance. This may suggest that officers viewed BWCs as protective against misconduct allegations, enabling more assertive policing.

Finally, I conduct a cost-benefit analysis that synthesizes the welfare-related findings. Using the value of a statistical life to estimate potential savings from reduced fatalities, I find (Appendix Section A.F) that BWCs can be cost-effective, especially for large urban agencies with high baseline rates of police-involved homicides and strict activation mandates. For departments with lower levels of force or weaker enforcement policies, however, the savings from fewer deaths alone may not be enough to justify the implementation costs.

8. Conclusion

Since the early 2010 s, BWCs have played a prominent role in efforts to improve police accountability and transparency. Currently, policy debates revolve around whether BWC use should be universally mandated for all police–civilian interactions. However, evidence from prior single-agency studies has been mixed, raising questions about whether the observed effects in some settings reflect generalizable impacts or isolated outcomes. Moreover, these studies have offered limited insight into the broader, agency-level implications of BWC adoption, particularly on enforcement behavior and crime.

This study addresses those limitations by analyzing the effects of BWC adoption across a large and more representative set of agencies. Using a cross-agency quasi-experimental design, it provides the first systematic evidence on how BWC effects vary across different policing environments and policy implementations. The findings reveal that BWCs are more likely to reduce police-involved homicides in agencies with higher baseline levels of force and more stringent activation requirements. At the same time, crime and arrest rates remained largely unchanged, suggesting no major trade-offs. These results underscore the importance of targeted implementation strategies that account for local conditions and the institutional design of BWC programs.

As for future work, several avenues warrant further exploration. While this study focuses on police-involved homicides due to their broad coverage across agencies, the rarity of such events limits the precision of the estimates. Future research should investigate more frequently occurring outcomes, such as lower-level uses of force, to better understand the full impact of BWCs on police–civilian interactions.

This study also finds that reductions in police-involved homicides were concentrated among White civilians, with limited changes observed for Black civilians. Some of this disparity may stem from more assertive policing, as suggested by the increase in index arrests of Black civilians. Further research is needed to assess whether the benefits of BWCs can extend to minority civilians, for example, in reducing lower-level or unjustified uses of force, and to address societal demands to reduce excessive policing of minority civilians.

Finally, the finding that agencies were able to implement BWCs without reducing overall policing activity is encouraging. While external oversight measures can sometimes lead to officer disengagement, reforms that are internally supported and not driven by scandal may enhance both accountability and performance. Recent work (Rivera and Ba, 2019; Devi and Fryer Jr., 2020; Prendergast, 2020; Jordan and Kim, 2025) has underscored the importance of distinguishing between types of oversight. Continued research on how to design accountability measures that improve police behavior without suppressing proactive policing remains essential.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at doi:10.1016/j.jpubeco.2025.105424.

Data availability

Data will be made available on request.

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