De-escalation technology: the impact of body-worn cameras on citizen-police interactions*

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Abstract

We provide experimental evidence that monitoring of the police activity through body-worn cameras reduces use-of-force, handcuffs and arrests, and enhances criminal reporting by the police. Stronger treatment effects occur on events *ex-ante* classified as low risk. Monitoring effects are moderated by officer rank, which is consistent with a career concern motive by junior officers. Our results stand in sharp contrast with previous literature which, due to often used coarser designs, showed muted or null body-worn camera effects on use of force. We show that these designs are likely to suffer from attenuation biases. Overall, our results show that body-worn cameras robustly de-escalate citizen-police interactions.

Keywords: police, use-of-force, technology, field experiment
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1 Introduction

The idea that the state alone has the right to use or authorize the use-of-force is one of its defining characteristics (Weber, 1946). For many citizens around the world, police forces are the primary visible representation of the state's monopoly on violence. Yet, the legitimacy and public confidence in the police is under strain worldwide especially in the wake of allegations of excessive use-of-force.[1] Police body-worn cameras (henceforth, BWC) have been hailed as a technological solution to increase scrutiny and oversight of the police. In this paper, we present experimental evidence showing that police BWCs effectively work to de-escalate police-citizen interactions, and improve the overall accuracy of police reporting.

We implemented a randomized controlled trial in a context that is more representative of the challenges faced by police forces in the Global South: the state of Santa Catarina in Brazil. Our experiment was designed to seed a random allocation of body-worn cameras at the granular police *dispatch* – the relevant unit of analysis in our study.[2] We view such dispatch-level data as the "natural" unit of analysis as it is the level at which citizen and police interactions unfold, and use-of-force and its (de)escalation may occur.

We find that BWCs trigger both notable improvements in the accuracy of police reports and, in contrast with much of the existing literature, significant improvements in interactions between citizens and the police marked by significant declines in use-of-force. For example, concerning reporting, dispatches treated with a camera present were 9.5% more often referred to the main investigative body, and police reports, on average, included 20.1% more victims. Importantly, treated dispatches saw a decline in the likelihood of use-of-force by 61.2%. A negative interaction index following Anderson (2008) – which also combines charges of contempt, disobedience or citizen resistance, and use of handcuffs or arrests –, was reduced by 47%.

We further document that the treatment effects are primarily concentrated in

[1] See New York Times (2020), Confidence in police is at a record low, Gallup survey finds, August 12, 2020, https://www.nytimes.com/2020/08/12/us/gallup-poll-police.html, accessed 10.08.2021.

[2] Throughout the paper, we also interchangeably refer to a dispatch as an "event".

events that *prior to police being dispatched* were classified as relatively *low risk* by virtue of there being either no weapons reported on the scene, there being no injuries, nor there being any material risk of general unrest as judged by the police. This suggests that cameras affect the situation dynamic by *preventing* the escalation of tension that would counterfactually unfold.

We next explore why cameras appear to work. In our design, both *whether* a camera is present and *who* carries it is random. This allows us to study whether who carries the camera matters. We find evidence of stronger de-escalation effects and increased compliance with the polices' BWC standard operating procedures if the officer wearing the camera is relatively junior. This suggests that BWCs might work by empowering low-rank officers to monitor their higher-ranked peers, implying that dynamic incentives and career concerns may be important factors driving their effect. This peer monitoring effect goes beyond the usually suggested mechanisms of BWCs reducing negative interactions due to the improved monitoring of both citizen and police behavior that they enable.

We also find that BWC have larger effects on areas with higher baseline use of force, consistent with cameras being effective in dispatches where use of force are more likely to occur. This suggests that cameras may show highest benefits in places where police-citizen interaction are relatively strained in the baseline.

Lastly, we attempt to shed some light on why our results appear to stand in contrast with much of the existing literature, which has mostly found null or very muted effects of BWCs – in particular on use-of-force (see for example the meta-analysis by Lum et al., 2020 and Williams et al., 2021). Naturally, the differences could simply arise because this paper is among the first to provide evidence of BWCs effectiveness in the context of a lower-income country in which citizen and police relations may structurally benefit more from BWCs (vis-a-vis the US and the UK which have been almost exclusively the focus of the existing work).[3] Yet, we show that a more likely explanation for the failure of existing studies to identify effects is due to the research designs and, in particular, the outcome measurement

[3] Magaloni (2019) marks an exception studying a BWC randomized controlled trial in a neighborhood of Rio de Janeiro, Brazil. Overall, they note very low compliance and little camera footage being produced.

and empirical evaluation strategies adopted. In fact, our research design nests a broad class of commonly used evaluation strategies or outcome measurement approaches that have been employed across experimental BWC studies. This allows us to replicate our own study at coarser levels of analysis or when employing different empirical strategies for evaluation. We find indeed that the estimated BWC treatment effects are much more muted or disappear altogether when mimicking the coarser evaluation approaches commonly used in the literature. The exceptionally granular data used in this study enables us to document that contamination, in addition to noise introduced in outcome measurement when moving from event-level to coarser designs, in combination are the likely culprits.

This paper contributes to the literature that studies mechanisms that can prevent police misconduct (Chassang and Miquel, 2019; Rozema and Schanzenbach, 2019; Shi, 2009). Harris et al. (2017) show that acquiring tactical weapons has a positive effect on citizen-officer interactions, reducing both complaints and assaults against officers. Relatedly, Owens et al. (2018) investigates the effects of training on improving citizen-officer interactions. Importantly, Hoekstra and Sloan (2022) show that race is an important determinant of police use-of-force, which could be substantially reduced if allocation of officers to dispatches took that into account.

We also contribute to the understanding of police interventions that aim to build trust or improve citizen relations and reduce crime. The meta-study Blair et al. (2021) finds no effects of community-policing intervention across different sites, including the state of Santa Catarina, Brazil. Magaloni et al. (2015) and Ferraz et al. (2016) show that another community policing program in Brazil known as UPP (*Pacifying Police Units*) had a positive effect decreasing violence but in the very specific context of territories dominated by drug trafficking gangs. Blattman et al. (2021) find little evidence that increased police presence and improved services more generally reduce crime in aggregate. Bove and Gavrilova (2017) show that militarized policing can deter street-level crime. Studying a reform that increased the probability that police could include offenders in DNA database, Anker et al. (2021) show substantial effects on the reduction of recidivism through increased surveillance capacity. Agan et al. (2021) show that non prosecution of nonviolent misdemeanour reduces defendants' criminal complaints. Bollman (2021) studies the effect of BWC on judicial outcomes and finds that even though the BWC do not alter the total number of outcomes in a particular jurisdiction, it does have an effect of reducing the number of charges that arise out of the interactions with the police, such as resisting arrest, assaulting an officer, among others.

Finally, we contribute to a broader debate on the productivity effects of monitoring actions, employer-employee agency problems, and alignment of employees' incentives to that of the general organization, for which police officers are just one example. For related work, see for example Ornaghi (2019) on civil service reform and Bertrand et al. (2020) and Xu (2018) studying bureaucrats more broadly. Specifically about the police, Banerjee et al. (2021) and Kapustin et al. (2022) discuss the importance of management quality on police misconduct and other policing outcomes. Relatedly, Battiston et al. (2021) shows how career incentives play an important role in worker's decision to communicate and their productivity. Our results suggest that career incentives also play a role concerning the use of the body-worn camera and how police officers work.

We proceed as follows. Section 2 provides the context, presents details about the intervention and discusses the data and measurement approach. Section 3 provides the main results and robustness checks that could threaten the validity of our estimates. Section 4 situate our results in the light of previous literature, and corroborate our experimental evidence using observational data. Section 5 concludes and discusses policy implications of the experiment results.

2 Context, Intervention and Data

Context Brazil is one of the most violent countries in the world – in 2018, the homicide rate was 27.4 homicides per 100 thousand inhabitants compared to 5.0 and 1.2 in the US and the UK, respectively.[4] We implemented the BWC intervention in the state of Santa Catarina, Brazil. Santa Catarina exhibits a homicide rate that is three times higher than the US and 12 times higher than the UK. We collaborated with the Igarapé Institute and the Santa Catarina state Military Police (PMSC), the main police body responsible for patrolling, responding to emergen-

[4] See United Nations Crime Trends Survey, available at https://dataunodc.un.org/.

cies, and manning the 911 hotline. It is the most visible element of the policing institutional infrastructure in Brazil. Five police precincts participated in the study: Florianópolis, São José, Biguaçu, Tubarão and Jaraguá do Sul. Those sites were chosen given their easy accessibility from the police headquarters in Florianópolis and to represent a variety of settings in terms of socio-demographic characteristics and of baseline violence levels.[5]

Intervention and Design Figure 1 provides an overview of the experimental design starting with the project timeline in Panel A. Panel B illustrates the two layers of randomization and how this induces variation at the dispatch level. Out of the roster of sworn police officers per precinct we obtained in July 2018, we randomly selected 1/3 of the officers to be in the treatment group and 2/3 to the control group across 40 stratification blocks. In total we have 150 officers assigned to wear BWCs and 295 control group officers. We stratified by precinct, officer activity, rank, previous internal investigations, and gender. Treated officers would always wear a camera if their 12-hour shift falls on days that – due to our second layer of randomization – were not selected to serve as *blackout days*. In every week during the fourteen weeks of the experiment, two days were randomly selected to serve as blackout days with the randomization stratified by day of week, providing us with across shift variation to camera exposure. Control officers were mandated not to wear a camera in any shift. Those two layers of randomization induce random allocation of cameras in the police dispatches, our primary unit of analysis. We consider our treatment to be the event level exposure to cameras – that is, if there is at least one officer in it wearing a camera. Since the vast majority of dispatches involve more than one officer, our sparser one-in-three officer-level randomization was calibrated such that approximately half of the dispatches post-treatment would have a body-worn camera, maximizing power and in sharp contrast with the existing literature which typically assigns cameras to more than 50% of the officers participating – we will elaborate on this in our discussion about the literature

[5] A map of the experimental locations is provided in Appendix Figure A1, while Appendix Table A2 studies site demographics. In Section 4 we also show that study sites did not present diverging pre-trends from non-experimental precincts.

in Section 4.[6]

Panel C of Figure 1 displays the number of dispatches we observe by day over the project period along with a moving average of the number of dispatches that had at least one officer with a camera present. Panel D provides the tabulation of the number of dispatches across the two layers of randomization. Out of the population of events that did not occur on blackout days – 13,274 dispatches – around 58% have had an officer present that was wearing a camera in the respective shift, in line with our simulations.

Integrity of research design The integrity of the research design was protected by a host of precautions. Cameras and docking stations were kept in the armory of the police precincts that officers visit at the start and end of each shift to collect and return their service weapon and equipment. Further, the blackout days were randomly selected at the start of the experiment but only communicated directly to the armory the evening before to avoid potential selection around the blackout days. Further, dispatch operators were blind to whether dispatch units are carrying a BWC. We find no evidence suggesting that there was significant non-compliance or other issues that could affect the integrity of the experiment which we discuss in the robustness checks. We further describe the implementation details in Appendix A.

Throughout the implementation, the research team had strong backing from the police leadership. Following recommendations on best-practice that were informed by past research suggesting low compliance with BWC use (which we discuss in Section 4 in more detail), a standard operating protocol (SOP) was developed, mandating that every dispatch involving an interaction with a citizen should be recorded, with few exceptions such as sensitive or covert operations. Officers were required to inform citizens verbally that "the dispatch was being recorded, according to police protocol" whenever the situation allowed.

The research team never had access to any recordings due to individuals' privacy concerns and the the sensitive nature of such data. Thanks to our integration into processes and the police IT systems we were nevertheless able to measure

[6]Appendix Figure A2 shows the result of the simulation which suggests around 50% of the events would count as treated with the 1/3 to 2/3 allocation.

compliance at the individual dispatch level as we outline further below.

Data We primarily draw on three main data sources. The first is *dispatch-level data* which is facilitated by PMSC fully digital data backend called PMSC mobile. The data captures the universe of all events that were attended by any PMSC officers. These events typically would originate from 911 calls, from self-initiated calls due to routine operations (such as patrolling), or due to scheduled activities (e.g. the execution of court orders), although 91.8% of them are the result of police being dispatched to a call of service through the central dispatch service. Our main outcome dataset contains a total of 17,665 events that span over the experimental period ranging from September 3rd 2018 to December 10th 2018 (see Panel D in Figure 1). It includes information regarding i) timing of the event (call, arrival at the scene and end of the event); ii) geographic information (the precise GPS location and full address); iii) event classification and reporting (dispatch opening and closing classifications, internal prior risk assessment, the facts that were reported during the interaction, and an indicator if the event generated a formal police report); iv) use-of-force (physical, non-lethal or lethal-force and number of victims, arrests and handcuffs deployed) and v) the hashed identifiers of officers that attended the event. We can merge this data with the serial number of the camera that has been assigned to treatment officers at the start of each shift.

In order to capture the treatment status of a dispatch, we merge the event information along with the officer identifiers to the individual camera log files. These log files along with the recordings cannot be tampered with. The information is extracted from the device in the armory after cameras are handed back at the end of each shift for charging. The log files are subsequently transmitted to the research team. As the log files provide both the serial number of the device and all information on when and for how long the camera was activated, we can match this to event information to capture whether recordings actually take place. This level of data access and end-to-end measurement of compliance marks a significant improvement vis-a-vis the existing literature.

Further to the dispatch data, we observe a range of officer characteristics such as their job title, rank, gender, the date of admission to the police along with the number of internal investigations that have involved the specific officer. These characteristics were also used to inform the stratification of the camera random assignment and were further used to explore heterogeneous effects.

Outcome measurement We study two broad sets of outcomes in addition to measuring compliance directly: reporting and police-citizen interactions around an event. Reporting is measured by i) if the event generates a formal police report, usually forwarded to the Civil Police which is responsible for investigative work preparing the formal judiciary charges, and by ii) if there was any victim, which is a measure of diligence and discretion of police activity. For the interaction margins we focus on i) a measure of citizen behavior, if there was any filing of contempt, disobedience or resistance charges towards police officers, ii) if any use-of-force was deployed (either physical, non-lethal or lethal, but excluding handcuffs and arrests) and iii) if there were any arrests or deployment of handcuffs. We further create an inverse covariance-weighted index combining these three outcomes following Anderson (2008), which we call Negative Interaction Index.[7]

3 Empirical Framework and Resutls

We next present the empirical framework, the main results, and carry out a range of further empirical tests to speak to the robustness of the results and that may help guide our interpretation of the main mechanisms driving the effects.

3.1 Specification and Main Results

Specification In what follows, we use the following empirical specification to study the effect of the presence of BWC across a set of outcomes measured at the police dispatch level.

$$y_{ibdw} = \beta \times \text{Treated Event}_i + \eta_{bw} + \tau_d + z_{ibdw} + \sum_{j=1}^{n} \phi_{o_j(i)} + \epsilon_{ibdw} \quad (1)$$

[7]The use-of-force outcomes were registered in the pre-analysis plan. The reporting margin is considered as part of an exploratory analysis. We detail the pre-registration of this study in Appendix Section C.

In this specification i indicates an event attended by a police dispatch, b is the police precinct, d is the day and w is the week of intervention. For our main specifications, we consider that Treated Event$_i = 1$ if at least one officer forming the dispatch that attended event i was assigned to wear a camera. This implies that our estimates capture an intention-to-treat effect. We include police precinct-by-week fixed effects (η_{bw}), day-of-the-week fixed effects (τ_d) and stratification bins controls ($\sum_{j=1}^{n} \phi_{o_j(i)}$). We also control for the number officers on the event z_{ibdw}. In our initial specifications, we exclude blackout days and focus exclusively on comparing treated with control events. The disturbance ϵ_{ibdw} is clustered at the police precinct-by-week level. We also include randomization inference p-values that are free from assumptions about the structure in the disturbance term.

Results Panel A of Table 1 presents the main findings of the paper. In column (1), we document that dispatches treated by at least one camera present saw notably more camera recordings. On average, 24% of the treated events were recorded. Virtually none of the control group events have any camera recording linked to them. It is not surprising that not all dispatches have a recording attached: the SOP required the use of cameras only if there were interactions with citizens, which does not occur in all dispatches.

We next study the impact of BWC on police reporting behavior across columns (2) and (3). We find that, on average, dispatches treated with a BWC present are reported 3.1 percentage points more often to the Civil Police, capturing a 9.4% increase. We also find that share of events in which a victim is reported increases significantly in the treatment group by 2.8 percentage points – capturing a 20.1% relative increase. These results suggest that BWCs successfully affect officers' reporting behavior, overall improving these margins.

Columns (4) to (7) of Table 1 present results pertaining to the different margins of interactions between citizens and the police that were measured. The results of Panel A show that BWCs reduce the negative interaction index by 0.37 percentage points, representing a decrease of nearly 47%. Furthermore, we find that filing of charges against citizens, the use-of-force by the police, and the use of handcuffs or arrests decreased substantially – respectively, by 28.2%, 61.2%, and

5.9% –, although only the effect on use-of-force is statistically significant at conventional levels. If police officers were under-reporting their use-of-force and the BWC works as an incentive for them to report truthfully, we would interpret the results of Panel A Table 1 as a lower bound to the true effect of BWC on use-of-force. The substantive decline in use-of-force marks a notable contrast with the existing literature that has typically found muted or no effects. We revisit this in light of previous work in Section 4.

3.2 Heterogenous treatment effects and mechanisms

We carry out a range of exercises that can speak to the underlying mechanisms at play that may drive our treatment effects. We explore heterogeneous effects in the subsequent panels of Table 1.

Event risk classification In Panel B, we study whether effects are primarily concentrated in events that were classified *ex-ante* as low risk. This assessment is done prior to officers being dispatched to the event. An event is classed as high risk if any of the following condition is met: there are individuals with life threatening injuries; the suspect is still on site; the suspect was armed; and whether there is a broader risk of broader disturbance to peace. An event is considered low-risk if the response is negative to all these questions. The results presented in Panel B of Table 1 suggest that the effects of BWCs improving citizens and police interactions are fully driven by events that are ex-ante classified as low risk. For those events, the negative interactions index is reduced by 51%. No BWC effects are detected among events that are judged to be high-risk ex-ante: the negative interaction index points to a much smaller and non statistically significant reduction of 8.8%. This suggests that BWCs may avoid escalation of situations. In high-risk events which have already escalated prior to dispatch, the presence of a camera itself may not affect the situational dynamic. This occurs despite the fact that the reporting margin is strongly affected by the presence of cameras in high-risk events. Taken together, those results suggest that cameras indeed serve as a way to de-escalate conflicts, diffuse tensions, and ensure a better cooperative environment on both sides.

Number of cameras on site Panel C documents that the treatment effects are larger with more cameras on site. This suggests that the extensive and intensive margins of monitoring matter. We find that, relative to dispatches with one camera, dispatches with two or more cameras were recorded 8 percentage points more often, the likelihood of a police report increases by 1.4 percentage points, and the number of victims in report increases by 1.5 percentage points. Those reporting effects are accompanied by a further reduction in the negative interaction index – promoting a further drop of 25.9%. In particular, the use-of-force falls by 79.8% when dispatches are treated by two cameras, also representing how increasing the intensity of the treatment increases the magnitudes of the effects, although with decreasing marginal returns to scale. This effect, however, is only statistically significant at the 10% level.

Who wears the camera In Panel D of Table 1, we explore if the effect is heterogeneous by the characteristics of the officers wearing the camera. We leverage on the fact that not only the event-level exposure to camera is random, but also the officer who is carrying it, allowing us to explore heterogeneous effects by their characteristics. We classify police officers in two categories, either a low-ranked "soldier" category or a higher-ranked category, for corporal or above ranks given the military-wise hierarchy structure at PMSC. We show that the rank of the officer assigned to hold the camera matters to explain the treatment effects. The BWC treatment effects in both reporting and interaction margins are only present when an officer with a soldier rank is holding a camera in the dispatch unit. Importantly, compliance with the protocol appears to be notably lower when higher-ranked officers carry the camera: dispatches appear to be recorded 24.3% less often compared to dispatches in which junior officers are assigned to wear the camera.

These effects are consistent with career concerns being effective mediators of compliance and treatment effects: early-career officers can be more likely to show behavioral improvements and protocol compliance when in presence of a camera. This suggests that the reduction of negative encounters between police officers and citizens is led by mostly changes in police behavior rather than citizens changing their conduct when in presence of a camera.[8]

History of use-of-force Finally, we explore the extent to which the camera effects are higher in areas in which, historically, there has been a higher likelihood of use-of-force in dispatches. To do so, for each census tract we count the events with use-of-force in the 13 weeks before the experiment and split the areas by above and below the median for each municipality. The results, in Panel E of Table 1, interact the measure of baseline use-of-force with the treatment indicator and suggest that stronger treatment effects are observed in areas of the municipalities that at the baseline experience higher use-of-force, despite compliance being unchanged across the two areas. The negative interaction index suggests a reduction in use-of-force that is nearly five times larger in absolute terms in areas with a historically higher propensity to involve use-of-force compared to areas with lower historic use-of-force. This is consistent with cameras being effective devices especially in places and situations where use-of-force would be, counterfactually, more likely to unfold. This is indicative that BWCs may be particularly suitable to benefit citizen-police interactions in areas that historically involved a higher degree of use-of-force. We consider this to be particularly important when considering how the roll-out of such technology may need to be prioritized to areas with relatively strained citizen-police interactions.

Reporting margin Building on the observation made earlier, that BWCs improve the quality of police reporting, we now show that the presence of BWCs also affects the distribution of *type of crimes* that police officers report. In particular, we are interested in whether officers are more likely to record criminal charges that, counterfactually without the evidence provided by the camera footage, would have been hard to prosecute. For this exercise, we use the same specification as in equation (1), but study as outcome variables a set of indicators for the type of criminal charges recorded by police officers around an event. We estimate the probability of a dispatch resulting in reporting one of the five more recurrent crime

[8] Alternatively, one could argue that citizens show behavioral improvements and cooperation with the police in the presence of high-ranked officers, reducing the scope for BWC effects in events attended by them. We rule out this possibility by showing in Appendix Table A3 that BWC effects also appear in the presence of one high-ranked officer.

types (noise complaints, verbal attrition or threat, burglary, assault, and domestic violence), as well as being registered without information about the crime type.

In Table 2, we observe that the presence of a camera in an event decreases the probability of an event being closed as an "event with no information to register" by 2.7 percentage points, a decrease of 5.8%, significant at the 5% level with conventional standard errors, although the randomization inference p-value is greater than 10%. The presence of a camera also increases the probability that an event will be recorded as involving domestic violence by 69.2%, as a burglary by 17.6% and as an assault by 19.6%. There are no statistically significant effects on noise complaints, and verbal attrition or incidents involving threats. We interpret these findings as further evidence that BWCs improve reporting by police officers. This is particularly relevant in cases such as domestic violence or assault, which, without hard evidence, the prosecution is considerably more challenging and seen as a barrier to access to justice.[9]

Blackout specifications We next investigate whether it is the presence of BWC that defines treatment effects or whether treatment effects are due to potential behavioral changes induced by officers *assigned to wear cameras*. To do so, we study whether there are BWC treatment effects among events occurring on blackout days when officers that usually wear BWCs are not allowed to wear them. To do so, we leverage on randomized blackout shifts, mimicking a shift-level variation that is also commonly used in the literature (see Ariel et al., 2015 and Ariel et al., 2016a). The added advantage is that our design gives us treatment variation *across shifts*, as explained in Section 2. Therefore we can leverage blackout days to explore how events that would be usually attended by officer with a camera present would counterfactually unfold. We estimate the following equation:

$$\begin{aligned} y_{ibdw} &= \beta_1 \times \text{Treated Event}_i \times \text{Treated Shift}_d + \\ &+ \beta_2 \times \text{Treated Event}_i \times \text{Blackout Shift}_d + \\ &+ \eta_{bw} + \tau_d + z_{ibdw} + \sum_{j=1}^{n} \phi_{o_j(i)} + \epsilon_{ibdw} \end{aligned} \tag{2}$$

[9]Insufficient evidence is identified as one of the underlying causes of the high attrition rates on gender-based violence in the criminal justice process (UNODC, 2014, pp. 38).

where Blackout Shift$_d$ $=$ 1 if day d was randomly selected as a blackout day and η_{bw} is a precinct-week fixed effect. The disturbance term is clustered at the precinct-week level. Thus β_1 captures the treatment effect within regular days (delivering similar point estimates to those in Table 1 Panel A) and β_2 captures the effect within blackout days, which we would expect to be zero, on average, in absence of learning effects.

The results are presented in Table 3. Column (1) highlights that, on blackout days, hardly any event gets recorded due to the experimentally induced absence of cameras. We note that all BWC treatment effects on the outcomes measuring citizen and police interactions disappear. Yet changes in the reporting behavior persist. This could indicate that officers that were in the past exposed to the use of cameras behave differently, even in the absence of the camera. This can be consequential for alternative research designs as we discuss below. The learning effects primarily affect the reporting margin but do not appear to have an effect on use-of-force or other citizen-police interaction margins.

3.3 Robustness

No endogenous allocation of dispatches The observed decrease in the interaction margins could be confounded by a change in the pattern of policing rather than cameras improving dispatch officers' behavior when present. We now show that this hypothesis finds no support in our data. In Appendix Figure A3 we show that the spatial distribution of treated and control events remains unchanged, suggesting there is no selection in space. This indicates that police officers do not change their patrolling behavior as a function of wearing a camera, and that treated and control events occur in similar areas of the cities. We further test for the absence of endogenous sorting as a function of treatment in an econometric setting. We estimate Equation (1) with characteristics of the event as outcomes, and we test if there is any correlation between the treatment and characteristics of the event. The results are presented in Table 4. In column (1), we test if officers with cameras are avoiding locations with higher baseline use-of-force, measured as census tracts with any episode of use-of-force in the two preceding weeks before the experiment started[10]. The results show that cameras are not present in events as a function of the baseline level of use-of-force.[11] The results suggest that we have no reasons to believe that cameras are hindering officers from working in areas in which the citizen police interactions are more likely to escalate. Location wise, we regress treated events against measures of latitude and longitude in columns (2) and (3), finding that treated and control events are statistically similar with respect to latitude. The point coefficient for longitude is 0.002 of a degree. This distance is negligible representing roughly 200 m measured at the equator; further, while the point estimate appears significant using conventional inference methods, it is insignificant using randomization inference (p-value of 0.374).

Columns (4) and (5) test if the treatment affects measures such as time to dispatch and an indicator if time to dispatch is greater than five minutes. The interval between an incident being reported and the officer arriving to the scene is the same between treated and control events. Therefore, in summary, treated and control events occur in the same places, they have the same baseline level of use of force and treated officers do not take longer to get there.

A potential concern could be that cameras may change the way dispatches occur. The vast majority of dispatches are initiated by the central dispatch- and call handlers, who were blind to underlying treatment status. Nevertheless, there is a potential concern that officers carrying a camera may not initiate events at the same rate as officers without cameras: column (6) highlights that this does not appear to be the case. We can also test whether treatment status is uncorrelated with call-handler induced dispatches: column (7) confirms this. Finally, in column (8) we show that there is no differential assignment of officers with cameras to events based on their ex-ante risk level.

In summary, the results of Table 4 suggest that there is no selection in space as a function of the treatment and that officers' behavior with respect to patrolling, arriving at the event and working location do not seem to be altered as a function of the cameras.

[10]This follows the division of the sample between above and below median baseline use-of-force.

[11]The same results are present for alternative measures of baseline use-of-force and for the baseline crime frequency in each location. The additional regressions are available upon request.

No endogenous allocation of senior officers to riskier events One potential concern that may arise is if high-rank officers are more likely to be dispatched to higher-risk events which could correlated with a lower potential for de-escalation of conflicts. This concern may have been raised by the heterogenous treatment effects that were documented in Panel D of Table 1 showing that treatment effects are driven by events in which a lower ranked officer is carrying a BWC. Further, since we noted that treatment effects are driven by events classified as lower risk ex-ante, this could further raise concern about endogenous allocation of officers based on ranks, with senior officers more likely to be allocated to attend to higher risk events, thereby confounding the results. Again, our experimental protocol rules this out as dispatch operators are blind to the respective treatment status of any dispatch unit. Reassuringly, in Appendix Table A1, we show that the presence of a higher-ranked officer is not correlated with an event being classified ex-ante as high-risk.

Further, in Appendix Table A3 we compare the effects when dispatch units were composed of *only* low ranked soldiers with at least one officer above the soldier level, irrespective of who, in the dispatch unit, was actually wearing the camera. We show that, except for dispatch recording, the effect is not driven by the tenure of the dispatch unit – but, rather, who in the dispatch unit was wearing the camera.

Alternative sample composition In Appendix Table A4 we document that the results are robust to changes in the estimation sample. Panel A reproduces the main effects for reference. In Panel B, we include data from blackout days. Not surprisingly, the treatment effect is still present, but is smaller in size given that we included events that were attended by treated officers on randomly selected blackout days when those officers were not handed out cameras.

Panel C looks at dispatches with two officers, which is the modal dispatch size. The results show that when we restrict the sample to these events, the effect on use-of-force becomes statistically insignificant, even though it remains negative and sizable in magnitude. The effects on the negative interaction index and on adverse citizen behavior remain strong. Finally, Panel D excludes dispatches with more than 4 police officers and again the results remain virtually the same. Overall, our results remain qualitatively unchanged in this exercise.

Exploiting only observational variation In Section 4, we position the findings from this paper in the context of the much broader literature on BWCs, and we carry out another analysis that can be seen as a further robustness check. We estimate treatment-effects exploiting only observational variation, exploring the spatially explicit dimension of our intervention. Using the group of precincts that didn't participate in the experiment, we are able to assess how use-of-force evolves in the experimental precincts, with around 58% of treated events, vis-a-vis those in which no officer wears a camera. This can allay some concerns about potential unobservable within-precinct spillovers. As is discussed in the section on spatially explicit designs covered in section 4, we find very similar point estimates when replicating our analysis exploiting such a non-experimental research design in our data.

4 BWC effects and the literature null

Our results stand in significant contrast with much of the existing literature which has often failed to detect effects of BWCs on use-of-force.[12] Different findings could naturally have arisen due to the different settings in which the experiments were conducted. For instance, our study is among the first to evaluate the effects of BWCs in a middle-income, high-crime setting (compared to existing studies which are mostly conducted in the UK or the US). While we cannot rule out that context-specific effects may have played a role in explaining the differences in the estimated effect, we provide evidence that suggests that the predominant null results may be partially due to previous studies being plagued by methodological issues that lead to muted results.

To do so, we are able to replicate the evaluation designs used in most past studies *in our data*. This is feasible as our study directly nests shift- and officer-centric research designs. Doing so uncovers BWC treatment effects that are similarly inconclusive when comparing to those past studies; this naturally suggests that the

[12] See Appendix Section B and Appendix Table A5 for an overview of the literature and for the description of results and methodologies.

BWC literature null can be due to the empirical design rather than the absence of true effects. We argue that such collection of results is consistent with contamination and measurement error attenuating effects estimated at coarser levels of analysis.

We also investigate how the data aggregation might have affected the BWC effect estimates. Unsurprisingly, we find that more disaggregated data – as used in this study –, provide for increased precision by increasing sample sizes and enabling detailed controls, e.g. fixed effects at very fine levels. In other words, these findings are consistent with the interpretation that past studies that make use of aggregate data may suffer from power issues.[13] We finally compare our estimates from those relying on observational variation in a differences-in-differences framework, and suggest that both can recover relatively similar BWC effects. We next describe these exercises.

4.1 Unit of randomization and analysis

We first contrast the results from our design at the event-level with what we would obtain if we were to re-analyze the data at the officer or shift levels. The vast majority, at least 80%, of the experimental studies surveyed involved randomisation at either of these two levels.[14] The level of detail of our dataset and the two-layer design of our experiment allow us to replicate shift and officer-centric designs in our data and compare results when estimating treatment-effects at the event level – our unit of analysis. This allows us to investigate the extent to which the BWC camera effect estimates are sensitive to the experimental design.

In Panel A of Figure 2 we present the results of such a comparison. The point estimate displayed in red shows the 61.2% percent reduction in use-of-force between treated- and untreated events, which is estimated from the nominal effect size of Table 1, along with the 95% confidence interval.[15] The event-level estimate

[13]Appendix Table A5 overviews the main features of 33 papers we looked at in some detail that investigate the effects of BWCs. We classify the studies as shift-centric (7 papers), officer-centric (13) or spatially-explicit designs (13).

[14]Table A5 attempts to organize 33 papers that were surveyed. Out of those, 24 papers are of experimental nature. Panel A of Table A5 lists seven papers with shift-centric randomisation. Panel B lists 13 papers that make use of officer-level allocation.

[15]We normalize our coefficients in terms of percentage reductions relative to the baseline inci-

is also indicated with the red dashed horizontal line across all panels, for ease of comparison with other designs.

As we noted, officer and shift-centric designs make up the vast majority of experimental BWC studies, which have found muted or no effects of BWC. We explore how changing the randomization unit impacts the BWC treatment effect estimates using our data. We start with mimicking an *officer centric* study, which randomizes officers into treatment and control groups. As outcome variable y_{od}, we measure the share of incidents in which an officer o used force over a time period – say, a day d. We then explore the experimental variation in officer allocation to the treatment and control groups in the following specification:

$$y_{od} = \beta_{\text{officer}} \times \text{Treated Officer}_o + \eta_{bw} + \tau_d + \phi_o + \epsilon_{od} \tag{3}$$

excluding blackout days so we solely rely on the between-officer variation. As in our main specification, we include police precinct-by-week fixed effects (η_{bw}) along with day-of-the-week fixed effects (τ_d). We also include stratification bins fixed effects ϕ_o. The disturbance ϵ_{od} is clustered at the police precinct-by-week level. Treated Officer$_o$ is equal to 1 if the officer was assigned to wear a camera. We are interested in the estimated β_{officer}.

The results plotted in Panel A of Figure 2 suggest notable attenuation: the effect size is reduced from our event-level benchmark 61.2% to 26.5%. The estimated treatment effect size capturing a decline in use-of-force of 26.5% is 57% smaller compared to the estimated treatment effect when carrying out the analysis at the event-level. The attenuation is not surprising: in our design 1/3 of officers were randomly selected to wear a camera. Due to dispatches typically involving more than one officer, this indirectly resulted in around 58% of the events being treated with at least one camera present. Since a noticeable share of events attended by control group officers are, in fact, indirectly treated due to the presence of other experimental officers carrying cameras, this downward biases the treatment-effect estimate since a large share of events coded as being attended by control-group officers are in-fact treated. Such contamination-induced attenuation bias may af-

dence of use of force to render the estimates comparable across studies.

fect many of the existing studies designed at the officer-level, which use simple difference-in-means econometric frameworks. What is even more problematic is that almost all existing studies cannot directly test or measure contamination due to a lack of detailed event-level data.[16] Furthermore, the extent of contamination-induced attenuation bias is likely increasing in the share of officers that wear a camera. In Column (9) of Table A5, we see that virtually all officer-centric studies opted for a design with 50% of officers assigned to wear a BWC. Assuming a similar dispatch composition as in our context, this implies that 75% of all events are treated with at least one camera (see Appendix Figure A2), undermining power and downward-biasing the treatment effect estimate when considering officer-level data. Therefore, the attenuation of the results is consistent with spillovers effects since the analysis at the officer level does not account for the fact that control officers will sometimes mechanically tend to dispatches with treated officers.

The final estimate in Panel A of Figure 2 presents the treatment effect estimate implied in our data carrying out the analysis when we solely exploit treatment- and control variation across shifts. In this case, we collapse the data at the precinct-by-day level, and we exploit the fact that our research design allows us to contrast blackout and non-blackout days to give us treated- and untreated shifts. This is close to the experimental design of shift-centric papers because a day is approximately composed of two 12-hour consecutive police shifts. In the following specification, the outcome variable y_{bd} is the share of events in which force was used at police precinct b during day d,

$$y_{bd} \quad = \quad \beta_{\text{shift}} \times \text{Treated Shift}_d + \eta_{bw} + \tau_d + \epsilon_{bd}. \tag{4}$$

The fixed effects we control for are police precinct-by-week and day-of-the-week. The error term ϵ_{bd} is clustered at the police precinct-by-week level. The estimated effect sizes are around 16%, a substantial attenuation from the 61.2% reduction in use-of-force that was originally estimated from the event-level specification, and

[16]Of the 12 studies that opted for an officer-centric research design, we were able to identify from the papers whether officers are dispatched in teams for only six studies – out of those, 50% report that officers are dispatched in pairs or more officers. These studies may thus be vulnerable to such attenuation bias.

not significant statistically. This effect size is in fact comparable with studies that originally make use of variation at the shift level. For example, Ariel et al. (2016b) also uses data at the precinct-shift level and explores shift randomization, and is within our confidence interval.

4.2 Temporal resolution of outcome measurement

We document that accounting for unobserved time-effects may be important as well. We focus on the officer-level variation to uncover BWC effects, but aggregating the outcomes either at officer-by-day, officer-by-month, or pooling officer observations during the experimental period. Specifications at coarser levels may introduce a broad range of biases as it implies that we cannot control for the potential confounding effect of time fixed effects which are likely very relevant. Only a few studies take into account time fixed-effects as additional control variables in their respective econometric framework, with the majority of studies either ignoring time, reducing the time-dimension to before-and-after comparisons or simply estimating differences-in-means without control variables (see Appendix Table A5).

Panel B of Figure 2 documents what happens to our point estimates with various data aggregations. The first effect size – outcomes at the officer-day level – is replicated from Equation (3), the most granular aggregation of the event-level data when considering exploiting our experimental variation at this unit of analysis. The second model aggregates the data to officer-month level. In this case, the outcome variable is the share of the events with use-of-force by police officer o during month m. We estimates the following specification:

$$y_{om} \quad = \quad \beta_{\text{officer-month}} \times \text{Treated Officer}_o + \eta_{bm} + \phi_o + \epsilon_{om} \tag{5}$$

where index o refers to an officer, while index m indicates the month. We include police precinct-by-month and stratification bins fixed effects. Although the effect size does not change considerably, the precision decreases substantially. This can have two main reasons: first, naturally, we have a smaller sample size, which implies with conventional inference, the standard error estimates are less precise[17] . Further, a coarser design does not allow for the inclusion of other relevant controls, such as granular fixed-effects which, while being uncorrelated with the treatment, in our experimental setup would improve the precision of the point estimates.

We can further aggregate the data for each officer and consider the whole experimental period. Such simple group comparison are often found in the BWC literature, accounting for easily 1/3 of the existing experimental and non-experimental studies we surveyed. We only exploit cross-sectional variation arising from the randomisation of the treatment status. We refer to this as the "pooled" specification. The estimating equation is:

$$y_o \quad = \quad \beta_{\text{officer-pooled}} \times \text{Treated Officer}_o + \eta_b + \phi_o + \epsilon_o \tag{6}$$

where we include only precinct and stratification bin fixed effects. Standard errors are heteroskedastic robust. The effect size from this equation is smaller in magnitude and also statistically insignificant. As a reference from the literature, Yokum et al. (2019) also use data at the officer level pooled during their experimental period. The effect size they find for use-of-force is virtually zero, in both magnitude and statistical significance. Our pooled result is comparable to theirs and, again, their point estimates shown in the horizontal black dashed line fall within our confidence intervals.

4.3 Differences-in-Differences designs

We now shed light on the BWC treatment effects that we would estimate had we opted for a differences-in-differences (DiD) empirical evaluation framework. DiD empirical frameworks are very common in observational studies of BWC effects and typically come in two forms: either to study treated and untreated officers or by studying treated and untreated spatial units over time. In the officer-centric DiD evaluation framework, the main concerns that may cause biased estimates are spillovers from control officers working with treatment officers and measurement errorIn the spatially-explicit DiD design, the prime concern is statistical power

[17] Clustering the data at the precinct-month level would not be adequate due to the low number of groups that this combination provides, so we instead use heteroskedastic-robust standard errors.

especially in context of low compliance and coarse outcome measurement.

Officer-centric DiD design We first present point estimates that emerge in our data when employing a DiD design that compares the changes in outcomes associated with officers that are assigned to wear BWCs with those officers that never wear cameras. The presented point estimate is arrived at from estimation the following specification:

$$y_{obd} \quad = \quad \beta_{\text{officer-did}} \times \text{Treated Officer}_o \cdot \text{Post}_t + \eta_{bw} + \tau_d + \phi_o + \epsilon_{bdw} \qquad (7)$$

where b is the police precinct, d is the day and w is the week of intervention. We include police precinct-by-week (η_{bw}), day-of-the-week (τ_d) and stratification bins fixed effects(ϕ_o). The disturbance term is clustered at the precinct-week level.

The first estimate in Panel C of Figure 2 presents the treatment-effect estimate suggesting that among officers assigned to wear BWCs, use-of-force decreases by 32.5%, relative to untreated officers. This point estimate is still around 47% smaller in absolute magnitude compared to the point estimate obtained when carrying out the analysis at the event-level and consistent with the differences-in-means presented in Panel B. This is explained by the confounding effect of spillovers arising from treatment and control officers being dispatched together. We illustrate Panel C with the point estimate from Braga et al. (2018) as one of the few existing studies that opted for such an evaluation approach, and finds a much smaller but statistically significant treatment effects of BWC reducing use-of-force.

Spatially explicit design We then move to a spatially-explicit design where we compare outcomes of experimental against non-experimental police precincts. In the course of our study, we obtained data from non-experimental precincts that were included in a parallel study on the effects of community policing (see Blair et al., 2021; Barbosa et al., 2022). We can leverage the data from those study sites to estimate a DiD design using non-experimental precincts as the control group. Naturally, these estimates may also suffer from some attenuation due to the sparsity of the treatment: only 58% of the events in experimental municipalities in the post period were treated as per our randomization protocol. Such attenuation bias in treatment effect estimates would not arise if all officers in treated precincts were given BWCs as is common practice in some existing studies leveraging spatially explicit research designs. We aggregate our main outcome variables measured at the event-level to the precinct-by-day level by calculating the share of events in a given precinct-day that involved use-of-force, for instance. We estimate the following equation:

$$y_{bdw} = \beta_{\text{precinct-did}} \times \text{Treated Precinct}_b \cdot \text{Post}_t + \eta_b + \eta_w + \tau_d + \epsilon_{bdw} \qquad (8)$$

where b stands for police precinct, d is the day of the week, and w is the week of intervention. Standard errors ϵ_{bdw} are clustered by precinct-week.

The second point estimate in Panel C of Figure 2 presents the results. We find that treated precincts present a 0.17 percentage points reduction in use-of-force, which is equivalent to a 46.1% decline of average use-of-force.[18] This treatment effect estimate is imprecisely estimated, suggesting that the research design may struggle with power, measurement error introduced with the aggregation, and the fact that a large share of events in "treated" precincts are untreated. Nevertheless, the point estimate gets closest to the event-level estimate being just around 25% smaller in absolute value. Out of 13 spatially explicit studies, most often exploiting non-experimental variation, only Kim (2021) found a negative treatment effect suggesting that BWC may reduce use-of-force – albeit notably smaller than what we document here.

Furthermore, we can also consider this exercise as a robustness check to the treatment effect estimates, given they are obtained solely exploiting observational variation, which also serves as corroborating evidence for the estimates that were presented in Section 3.

5 Conclusion

Police violence is a worldwide concern and there is an urgent need to find ways to increase accountability. In this paper, we investigate the effects of body-worn cameras on police officer reporting behavior, on citizen misbehavior, on use-of-

[18] Appendix Figure A4 shows that pre-trends are absent for both DiD designs in this section.

force, and on use of handcuff and arrests. Through a large-scale experiment with an original design, we show evidence that body-worn cameras are effective to reduce use-of-force by police officers. The experiment took place in the state of Santa Catarina, Brazil, and five precincts were part of it with approximately 450 police officers taking part.

The results show that body-worn cameras are effective to improve the nature of police-citizen interaction – much to the contrast of the existing literature. Body worn cameras reduce use-of-force by the police by around 61.2% and improve the reporting accuracy of police officers. Moreover, we show that the decrease in use-of-force takes place in low seriousness events, as judged by a previous measure of risk assessment. The dispatch composition and the characteristics of the officer who is wearing the camera also matter for the treatment effect. Officers early in their careers, with a soldier rank, present higher reductions on the negative interaction index and on the filing of charges against citizens when carrying cameras.

The experiment has important policy implications. First, the results suggest that cameras are effective to curb police violence, which indicates that using them can increase the accountability of police officers. Moreover, the officer who wears the camera is also an important feature for compliance and for the results regarding use-of-force to exist. Officers early in their career are more likely to comply with the protocol and show improvements in reporting and interaction margins metrics. Implementing cameras can be an important step towards decreasing excessive use-of-force by the police, but to ensure that the cameras are efficient, it is important to consider the career incentives that exist for police officers that wear them. If officers are concerned about career progression, they are more likely to adjust their conduct to the protocol, fearing the possible repercussions. Put together with the results on blackout days, which show that treated officers in blackout days do not show a reduced use-of-force, the effects indicate that wearing a camera is important for inducing behavioral changes even for officers with career incentives, which are driving most of the treatment effect. Therefore, cameras are effective if police officers are concerned about the career implications of misbehaving.

Furthermore, our results speaks to a broader discussion on policy evaluation of police interventions. In such contexts where a natural unit of analysis does not exist, we document that focusing at the level in which the delivery of public goods take place (e.g. police-citizen interaction) helps to precisely identify treatment effects by allowing researchers to properly measure the phenomenon of interest as well as to control for correlated time effects.

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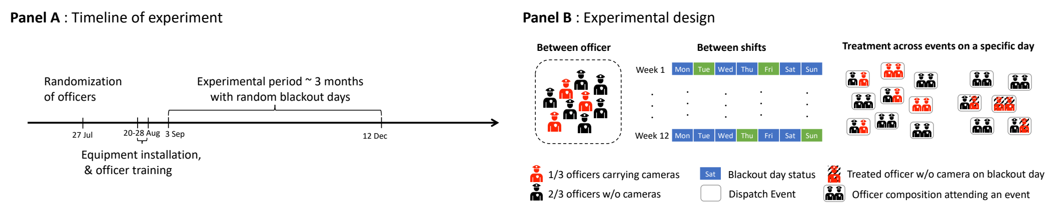
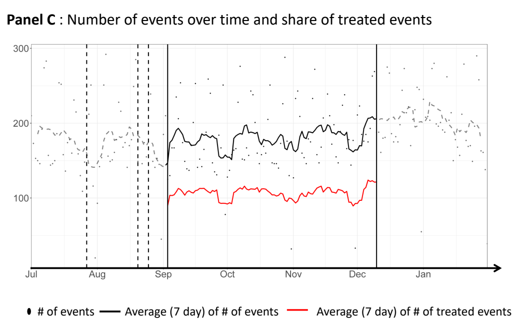
Tables and Figures

Figure 1: Timeline and experimental design



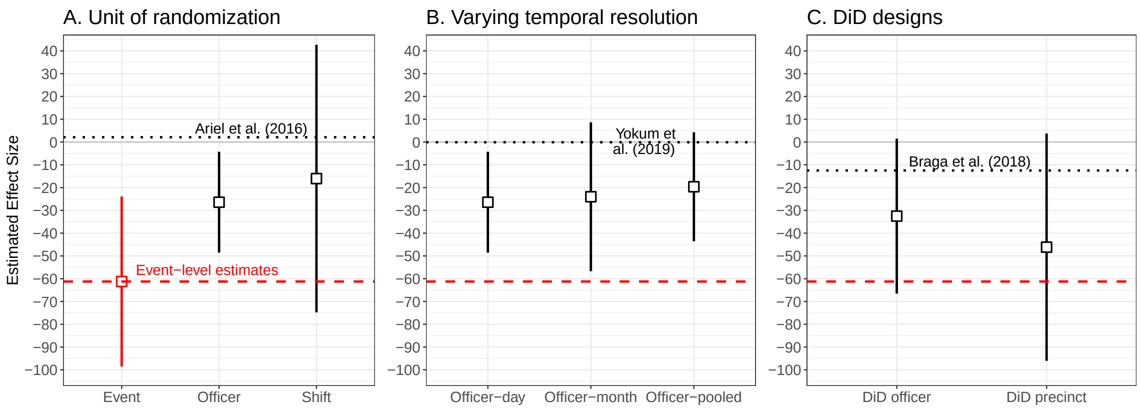


Panel D : Tabulation of # events by implied treatment status

		Days	
		Regular (5/7)	**Blackout (2/7)**
Officers	**T (1/3)**	7803	2646
	C (2/3)	5461	1751

Notes: The figure presents the experimental timeline. Panel A provides the timeline of the experiment that was conducted in 2018. Panel B illustrates the between and within officer variation that is randomly induced and how this can map into different treatment status at the individual event level. Panel C plots the time series of the number of events with a police dispatch per day across the experiment along with the seven day moving average of the number of treated- and overall number of events illustrating that, on average, 50% of events have an officer attending that is assigned to wear a camera. Panel D presents the tabulation of the overall number of experimental events by the treatment status.

Figure 2: Comparing the distribution of effects with different designs and the literature



Notes: The figure presents results on how the estimated treatment-effect sizes vary if we reanalyze the data using different commonly used evaluation strategies. Benchmark results from this paper exploit event-level variation and are presented in red. Estimates of effect sizes from reference studies in the literature using such designs are annotated as a horizontal dashed line and are closer to the most comparable estimate from our data. Panel A explores how changing the unit of randomization affects the results, exploring experimental variation between treated and control officers and between treated and control shifts. Panel B explores varying the temporal resolution in the aggregation of the outcome data, while keeping the experimental variation of officers constant. Panel C explores two differences-in-differences models, the first exploring experimental variation between officers and the second exploring the spatially explicit implementation of BWC.

Table 1: Effects of body worn cameras on accuracy of police reporting and citizen-police interactions

		Reporting Behavior		Interaction Margins			
	Dispatch Recorded	Police Report	Victims in report	Negative Interaction Index	Contempt, Resistance and/or Disobedience	Use-of-force	Handcuff and/or Arrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Main Effects							
Event treated	24.043***	3.101**	2.783***	-0.371**	-0.263	-0.425***	-0.320
	(1.873)	(1.190)	(0.805)	(0.149)	(0.196)	(0.157)	(0.471)
	p = 0.000	p = 0.060	p = 0.004	p = 0.030	p = 0.280	p = 0.009	p = 0.584
Panel B. Heterogeneity by Ex-ante Event Risk Assessment							
Event treated x Low Risk	24.052***	2.882**	2.349***	-0.403**	-0.381*	-0.414**	-0.433
	(1.857)	(1.274)	(0.775)	(0.160)	(0.216)	(0.161)	(0.501)
	p = 0.000	p = 0.091	p = 0.022	p = 0.010	p = 0.095	p = 0.004	p = 0.445
Event treated x High Risk	23.968***	5.417**	7.096***	-0.070	0.777	-0.489	0.718
	(2.605)	(2.449)	(2.395)	(0.640)	(0.811)	(0.703)	(1.474)
	p = 0.000	p = 0.060	p = 0.027	p = 0.928	p = 0.487	p = 0.452	p = 0.636
Panel C. Heterogeneity by Treatment Intensity							
Event treated by 1 Camera	22.430***	2.825**	2.485***	-0.330**	-0.207	-0.392***	-0.091
	(1.881)	(1.275)	(0.875)	(0.145)	(0.205)	(0.143)	(0.533)
	p = 0.000	p = 0.082	p = 0.014	p = 0.048	p = 0.390	p = 0.017	p = 0.884
Event treated by 2 or More Cameras	30.473***	4.201**	3.972***	-0.535**	-0.487	-0.554*	-1.233
	(2.632)	(1.599)	(1.184)	(0.265)	(0.357)	(0.298)	(0.804)
	p = 0.000	p = 0.098	p = 0.007	p = 0.111	p = 0.293	p = 0.083	p = 0.209
Panel D. Heterogeneity by Officer Rank							
Event Treated by Officer(s) with Soldier rank	24.974***	3.247**	3.240***	-0.444***	-0.390*	-0.471***	-0.294
	(2.040)	(1.289)	(0.871)	(0.161)	(0.219)	(0.166)	(0.504)
	p = 0.000	p = 0.049	p = 0.004	p = 0.015	p = 0.121	p = 0.007	p = 0.619
Event Treated by Officer(s) with higher than Soldier rank	18.906***	2.201	1.503	-0.174	0.095	-0.304	-0.520
	(2.244)	(2.077)	(1.349)	(0.311)	(0.413)	(0.325)	(1.156)
	p = 0.000	p = 0.609	p = 0.391	p = 0.606	p = 0.871	p = 0.311	p = 0.734
Event Treated by Officers of both types	24.788***	3.957	-0.108	0.154	0.604	-0.065	-0.001
	(2.678)	(3.217)	(1.880)	(0.502)	(0.641)	(0.541)	(1.707)
	p = 0.000	p = 0.394	p = 0.958	p = 0.813	p = 0.522	p = 0.909	p = 1.000
Panel E. Heterogeneity by Baseline Use-of-force							
Event treated x Below Median Use-of-Force	24.158***	2.874**	2.913***	-0.206*	-0.087	-0.265**	-0.265
	(1.911)	(1.227)	(0.868)	(0.123)	(0.170)	(0.133)	(0.517)
	p = 0.000	p = 0.095	p = 0.003	p = 0.189	p = 0.714	p = 0.079	p = 0.675
Event treated x Above Median Use-of-Force	23.584***	4.030*	2.270	-1.025**	-0.962*	-1.059**	-0.527
	(2.079)	(2.220)	(1.431)	(0.402)	(0.541)	(0.436)	(0.827)
	p = 0.000	p = 0.079	p = 0.161	p = 0.009	p = 0.092	p = 0.006	p = 0.590
Mean Dep. Var. Control Events	0.000	32.761	13.832	0.790	0.932	0.694	5.427
N	13274	13274	13274	13274	13274	13274	13274

Notes: Table presents results on the impact of a body worn camera being present at a police event. Panel A presents the main results capturing the average intent-to-treat effect. Panel B explores heterogeneity by the ex-ante risk level of the events, which characterizes an event as low risk if it has no weapons on the scene, if there are no injuries, if the suspect is not on site and if there is no material risk of general unrest. Panel C investigates treatment intensity heterogeneity, given by the number of officers wearing a camera in events. Panel D explores rank heterogeneity of who is wearing the camera. Panel E explores the heterogeneity by baseline use-of-force in areas of the municipalities. The dependent variables are "Dispatch recorded" indicating that the dispatch was partially or fully recorded using the body worn camera and hence represents the treatment being delivered. "Police Report" and "Victims in report" capture the extent to which officers formally report events, on which basis the Civil Police would proceed investigations. Interaction Margins comprises: (i) "Negative Interaction Index" is the standardized inverse-covariance weighted average of the three indicators in the group; (ii) "Contempt, Resist and/or Disobey " is an indicator if charges of contempt, disobedience or resistance towards the police were registered; (iii) "use-of-force " is an indicator if there was any deployment of physical, non-lethal (mechanical) or lethal force by the police, not considering use of handcuff or arrest; (iv) "Handcuff and/or Arrest"is an indicator if handcuffs were used or if any arrests made. All dependent variables are multiplied by 100. Specifications include police precinct-by-week, day of the week, number of officers and stratification bins fixed effects. Shifts without camera are excluded from the regression. Standard errors are clustered at the precinct-by-week level. *** $p<0.01$; ** $p<0.05$; * $p<0.1$. The randomization inference p-values are indicated below the standard errors.

Table 2: Reporting margin

	Event Registered with No Info	Noise Complaint	Verbal Attrition/ Threat	Burglary	Assault	Domestic Violence
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Event	-2.770**	0.126	0.237	0.842*	0.709*	1.138***
	(1.239)	(0.550)	(0.645)	(0.427)	(0.388)	(0.351)
	p = 0.164	p = 0.870	p = 0.774	p = 0.131	p = 0.096	p = 0.000
Mean Dep. Var. Control Event	47.268	8.661	9.940	4.787	3.618	1.644
N	13274	13274	13274	13274	13274	13274

Notes: Table documents the effects of BWC over the criminal typology contained in police reports. The first dependent variable measure if any criminal/fact type was reported, while the others are indicator of criminal typology reported at the end of the event. All dependent variables are multiplied by 100. Specifications include police precinct-by-week, day of the week, number of officers and stratification bins fixed effects. Shifts without camera are excluded from the regression, which follows specifications 1). Standard errors are clustered at the precinct-by-week level. *** p<0.01; ** p<0.05; * p<0.1. The randomization inference p-values are indicated below the standard errors.

Table 3: Exploiting within-shift variation: Effects of body worn cameras on accuracy of police reporting and citizen-police interactions

		Reporting Behavior		Interaction Margins			
	Dispatch Recorded	Police Report	Victims in report	Negative Interaction Index	Contempt, Resistance and/or Disobedience	Use-of-force	Handcuff and/or Arrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated Event in Treated Shifts	23.968***	2.786**	2.100***	-0.302**	-0.218	-0.343***	-0.301
	(1.773)	(1.182)	(0.728)	(0.126)	(0.180)	(0.125)	(0.461)
	p = 0.000	p = 0.097	p = 0.040	p = 0.085	p = 0.361	p = 0.052	p = 0.603
Treated Event in Control Shifts	3.748***	4.026***	2.419***	-0.169	-0.108	-0.202	0.213
	(0.946)	(1.213)	(0.823)	(0.151)	(0.252)	(0.194)	(0.573)
	p = 0.039	p = 0.059	p = 0.119	p = 0.459	p = 0.743	p = 0.395	p = 0.775
Mean Dep. Var. Control	0.000	33.158	14.652	0.751	0.901	0.652	5.420
N	17665	17665	17665	17665	17665	17665	17665

Notes: Table documents the comparison between treated and control events across randomly assigned blackout days, shifts in which treated officers do not wear cameras. Dependent variables defined as in Table 1. Specifications include police precinct-by-week, day of the week, number of officers and stratification bins fixed effects. Standard errors are clustered at the precinct-by-week level. *** p<0.01; ** p<0.05; * p<0.1. The randomization inference p-values are indicated below the standard errors.

Table 4: Testing for endogenous allocation of BWC to Events

	High Baseline Use of Force	Latitude	Longitude	Time to Dispatch (Minutes)	Time to Dispatch Greater than 5 min.	Active Policing	Telephone Initiated Dispatch	High Ex-Ante Risk
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated Event	-0.170	0.001	0.002**	-1.719	-1.348	-0.201	0.235	-0.341
	(0.896)	(0.001)	(0.001)	(1.269)	(1.100)	(0.573)	(0.574)	(0.587)
	p = 0.869	p = 0.708	p = 0.374	p = 0.223	p = 0.275	p = 0.838	p = 0.812	p = 0.586
Mean Dep. Var. Control Events	20.080	-27.468	-48.787	10.701	43.868	7.637	92.180	10.378
N	13274	13274	13274	13274	13274	13274	13274	13274

Notes: Table presents tests for the characteristics of the dispatch that could suggest endogenous allocation with respect to treatment assignment. Dependent variables are: (i) High Baseline Use-of-Force, which indicates whether the event happened in census tracts with above median baseline use-of-force; (ii) Latitude and (iii) Longitude, both measured in degrees; (iv) Time to Dispatch, a measure of the interval between communication and dispatch arrival in minutes and (v) a dummy to whether this interval was higher than 5 minutes; (vi) Active Policing, a dummy that indicates if the police self-initiated the event rather than being dispatched to it, (vii) is a dummy that indicates if the event was communicated through the telephone central and (viii) is a measure of ex-ante risk that characterizes an event as low risk if it has no weapons on the scene, if there are no injuries, if the suspect is not on site and if there is no material risk of general unrest. Sample includes all events in the experimental period and excluded blackout shifts. Specification includes include police precinct-by-week, day of the week, number of officers and stratification bins fixed effects. Standard errors are clustered at the precinct-by-week level. *** p<0.01; ** p<0.05; * p<0.1. The randomization inference p-values are indicated below the standard errors.

Online Appendix

"De-escalation technology: the impact of body-worn cameras on citizen-police interactions"

For Online Publication

Barbosa, Fetzer, Soto-Vieira and Souza

August 12, 2022

A Implementation Details

Preparations and randomization We obtained the full roster of citizen-facing police officers in the beginning of July 2018. Officers without citizen-facing duties, such as administrative roles, were not considered eligible for camera use. We used pre-intervention dispatch data to validate if the list that was sent to us had contained all citizen-facing police officers. We then confirmed that there was no selection of officers into the study sample.

During this period, all tests with the cameras and docking stations were conducted to ensure that the information necessary for the experiment was correct and to minimize technical issues during the experiment period. Prior to the start of the experiment, all officers were briefed in how to use the equipment, and how to adjust the standard operating procedures allowing for the use of camera (in particular, it was made clear that officers were required to verbally communicate to citizens that the events were being recorded). Importantly, *all* officers were briefed – irrespective of the treatment status – to avoid the briefing itself confounding the BWC treatment effects.

The implementation timeline is depicted in Appendix Figure 1. We randomized officers and blackout days on July 7th 2018. Shift-level treatment allocation was randomized before the start of the experiment, but we only communicated to the police precincts in the preceding evening through dedicated WhatsApp groups established for this purpose. This was supposed to avoid the potential for the endogenous selection of any aspect of the policing activity with respect to the anticipation of blackout days to begin with. Importantly, the blackout applies to officers starting their shifts. That is, officers already on-duty at midnight of the start of the blackout would continue to use their cameras until the end of their shift; conversely, any shift that starts during blackout that spans after its end would not be recorded. This feature was necessary for logistic reasons: the police deemed it not practical or desirable to interfere in the apparatus of the dispatch units after they had left the precinct headquarters.

Intervention step-by-step Once the experiment period started, the intervention would happen as follows. At the start of their shifts, treated officers would obtain their camera, along with other equipment, from the armory section of the police precincts – from where they obtained their gun, radio, and other equipment of regular and special use. The armory sections are usually very secluded and considered to be of high-security environment – due to the nature of the material that is stored therein – and only a few high-ranked officers have access to those rooms. Importantly, the docking stations, which both downloaded the videos at the end of every shift and recharged the cameras, were located inside the armory rooms. This ensured that not only the equipment was maintained, regularly inspected, and kept to a good working order throughout the experiment, but also ensured that docking stations and cameras themselves were not interfered with or violated during the experiment.

The docking stations were remotely accessible from the PMSC headquarters. Videos were stored locally for 30 days and pulled to the central HQ on demand due to bandwidth issues. The research team established routines to consolidate the camera automatic logs in a central database. In this way, it is possible to observe if a given dispatch generated a video recording, as well as the corresponding docking station and filenames. After finishing their shift, police officers would hand back cameras to the armory section, which would then be docked in the station and readied for the next use. This recycling process usually took between 4 and 6 hours for a full battery charge that lasted at least eight hours in continuous regular use.

On the preceding night before control shifts, the research team would message the officers responsible for the armory sections in each police precinct telling them to **not** give cameras to treated officers. So all the officers that would start their shifts in the blackout day would receive from the armors all the equipment but the cameras. On any given day, dispatch units would be composed by on average two officers. If any of those was assigned to wear a camera at the officer level randomization, this dispatch (as well as the event they tended to) is classified as a treatment one. Thus, the average treatment effect of BWC implementation over police activity and police-citizen interactions is identified by comparing events attended by dispatches with at least one officer assigned to wear a camera with events with none.

As for blackout shifts, all treated officers would not be allowed to wear cameras.

Therefore, we can compare events attended by treated dispatches with events attended by control dispatches in days in which no treatment officer is allowed to wear cameras, allowing us to identify if the effects would persist were the treatment technology not present. Importantly, the dispatch operators were blind to whether dispatch units were manned with officers wearing a body-worn camera. This prevented the endogenous allocation of dispatch calls to be recorded (or, conversely, to avoid recoding).

B The BWC literature and use-of-force

Experiments on the effects of BWC on use-of-force do not consistently show that cameras effectively work to decrease excessive use-of-force, and mixed evidence across studies as shown by Lum et al. (2019). Appendix Table A5 lists the main BWC papers in the literature.[1] We include their main features, e.g. the number of citations, country, sample size, share of treated units, and whether any effect of use-of-force is detected. As it can be seen, the literature is not conclusive on the BWC camera effects on use-of-force. We argue below that most papers were plagued with methodological issues that attenuated the camera effects.

We start in Panel A with the studies that allocated cameras on the basis of shifts. Those papers, whether providing experimental evidence or not, allocate cameras to treatment and control shifts. We argue that this design is potentially problematic as a single given officer may be allocated to both a treatment and a control shift. This may be an important SUTVA assumption violation if, for example, officers alter their behavior after using a camera, e.g. through learning, or if there are across-officers spillover effects (Ariel et al., 2017). Out of the seven studies that use shift analysis, five have use of force as an outcome and only one finds statistically significant results (at the 5% level) that suggest that BWC affect use-of-force. Ariel et al. (2015) conducted the first experiment on BWC and it is by far the most cited paper in the literature. The shifts were randomized to be conducted with and without cameras, and the results suggest that BWC reduce

[1] This is not intended as a literature review, but selective and partial read on the studies that we found to be most prominent in the literature.

use-of-force by the police. However, these effects are barely significant at the 10% level. Following that, Ariel et al. (2016b) repeated the same design across multiple sites, and the results show null effects of BWC on use-of-force. Ariel et al. (2016a) suggest that one potential explanation for muted results comes from compliance with the protocol. They show that use-of-force rates were higher in sites where the compliance with the protocol was lower, and vice-versa. Magaloni (2019) does not find any effects of BWC in use-of-force, and the experiment faced issues with low compliance as well. With an experiment in the UK, Henstock and Ariel (2017) used shift randomization and find that BWC were effective to reduce use-of-force, in particular physical restraint and non-compliant handcuffing.

We move to officer-centric designs in Panel B. The literature shifted to officer-level allocation to ensure officers were always in the same assigned group throughout the duration of the experiment. This design also presents its challenges. First, contamination is a substantial concern: among the officer-centric papers we could identify, half had routinely more than one officer per dispatch, which can mechanically result in contamination between officers if both a treated and a control officer are in the same dispatch. Moreover, all officer-level studies treat half of the police officers, which results in a much higher share of treated events – if an event is considered as treated if one or more cameras were present –, given that most dispatches are tended by more than one police officer. In our data, simulations show that treating half of the officers would imply on around 75% of treated events (see Figure A2), leading to a considerably smaller control group and potentially undermining power. A corroborating evidence from Braga et al. (2020), who use officer-level randomization combined with spatial selection of districts, indeed shows evidence of large contamination from treated officers to control officers. Finally, some papers only included in the experiment officers that volunteered to wear a BWC (Jennings et al., 2015; Ready and Young, 2015; White et al., 2017; Headley et al., 2017; Braga et al., 2017, 2018). This can introduce self-selection bias and compromise the identification of the effects. Taken together, these design characteristics can result in muted estimated effects of BWC on police operations.

Finally, in Panel C we list papers that make use of spatially explicit empirical designs. Out of 13 studies, only three look at use-of-force as an outcome and only Kim (2021) find evidence of the impact of BWC. They use a DiD empirical strategy and take advantage of the variation in the timing of the adoption across US agencies to assess the effects of BWC on a national level. While this strategy does not have to deal with the spillover that can occur between officers, it relies on the strong identifying assumption that adoption timing is independent of agency characteristics. Similarly, Miller and Chillar (2021) explores the staggered adoption of BWC to study the effects on fatalities that arise from citizen-police interactions. Bollman (2021) studies the effects of BWC on court outcomes, also using a spatially explicit differences-in-differences. She finds a significant reduction in new case fillings for offenses initiated during a citizen-police interaction, suggesting an improvement of these encounters.

Overall, some papers in this panel do not follow rigorous program evaluation techniques and some do not even perform statistical inference methods. Nonetheless, meta-analysis with the existing studies have found no statistically significant effect of BWC on use-of-force, even though the point estimate is negative (Lum et al., 2020; Williams et al., 2021).

C PAP Registration

Submission history. Our initial study design was pre-registered on the "Evidence in Governance and Politics" (EGAP) repository as part of a broader project on the Metaketa IV round of funding that analysed the effects community policing. The EGAP repository was later in 2020 fully migrated with the OSF repository and can now be accessed through the link `https://osf.io/yzpva/`. File dates in the OSF system refer to migration date, not the original date we submitted to the EGAP registry. The Pre-Analysis Plan (PAP) associated with this project was registered in November 2018, before we had access to most of the experimental data.[2] We had access to the majority of the data with substantial delays in December 2019. We registered an update to the PAP in January 2020.[3] The updates from the first version are not relevant to this project as they mostly pertains to the parallel study on the effects of community policing program. We further amended the

[2] `https://osf.io/j2p5y/`

[3] Available at `https://osf.io/j2p5y/`.

PAP including a Specification Appendix specifically for this project, and before we undertook any data analysis, in June of 2020.[4] The analysis was also registered at the AEA Registry with `AEARCTR-0007785`. We did so when we decided to submit the paper to AEA journals as mandated by editorial guidelines[5].

Hypotheses. In the November 2018 PAP we registered the hypotheses to be tested that we reproduce here in Appendix Table A6. Our understanding at that point was that we would be able to distinguish which officer took specific actions within the dispatch. For example, we would obtain data regarding which officer in the dispatch was responsible for use of force, and who conducted arrests. We postulated the hypotheses based on this understanding. We later learned that it was not possible to distinguish which officer in a dispatch had been responsible for each outcome. Our outcome data is instead at the level of the *dispatch*, not the officer within the dispatch. Following on the example above, we observe if there was use of force, or arrests at the level of the dispatch, but not the specific officer who undertook those actions. This made it impossible to test `H1-a` and `-b`, `H2-a` and `-b`, `H3-a` and `-b`, and `H4-a` and `-b`. Moreover, the low quality of the Civilian Complaints dataset proved it impossible to test `H2` altogether. Our definition of the treatment follows the hypotheses `H1-c`, `H3-c`, and extrapolates the same definition to be able to test `H4`.

Outcomes. The use-of-force outcomes were registered in the PAP. Outcomes of the reporting margin – whether cameras increase the probability that an event lead to the filling of a police report or the incident that is reported on it –, are understood as part of our own exploratory analysis. Nonetheless, given the effect sizes detected (see Table 2), we believe them to be of high importance for the understanding of the impact of the policy. The fact that the increase in reporting of certain types of crime is larger for domestic violence, and to a lesser extent, burglary and assault are important findings, and for this reason we opted to include along the main tables.

[4] Available at https://osf.io/f923e/.

[5] From January 2018, the submission policy to AEA Journals makes mandatory registration in the AEA RCT registry.

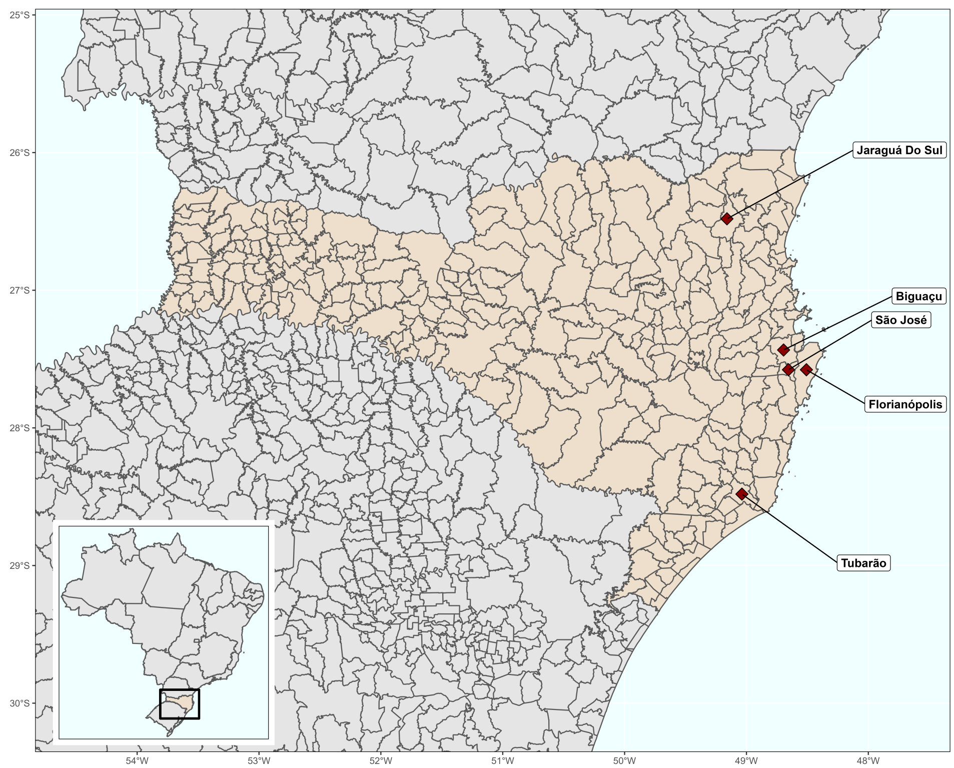
Specifications. The specifications we use in the paper are largely consistent with those that were registered in the PAP. Below we detail and explain the reasoning behind minor differences in some specifications. In all cases, the versions in the paper and as registered in the PAP produce very similar results. The latter are available upon request. We reproduce Equation (1) from the PAP below

$$y_{ibdw} = \beta \times \text{Treated Event}_i + \eta_{bw} + \tau_t + z_{ibdw} + \epsilon_{ibdw} \tag{9}$$

where η_{bw} precinct-by-week fixed effect, τ_d is day-of-the-week fixed effect, and z_{ibdw} is the number of officer fixed effects. For convenience, the notation and subscripts were harmonised with those used elsewhere in the paper. In the main specification, Equation (1), we added stratification bins to account for the stratified random assignment of cameras to officers. This is necessary to account for the fact that the camera assignment is random conditional on the stratification bin. Results are shown in Panel A of Table 1. The inclusion of officer stratification bin fixed effects was also reproduced in the blackout specification, corresponding to the PAP Equation (4). The event risk pre-assessment by the police was also pre-registered within the paragraph for heterogeneities. In line with the PAP, we also explore the effects of treatment intensity in Panel C. Other specifications registered in the PAP pertain to the an event-study design, and variations of the main equation using less granular variation aggregated at the officer and precinct levels. We use those specifications in Section 4 and Appendix Section B as they allow us to compare the results with previous studies in the literature which implemented similar designs. Once more, we replicated all the analysis with the exact versions in the PAP and found results that are very consistent with those reported in the paper, and are available upon request.

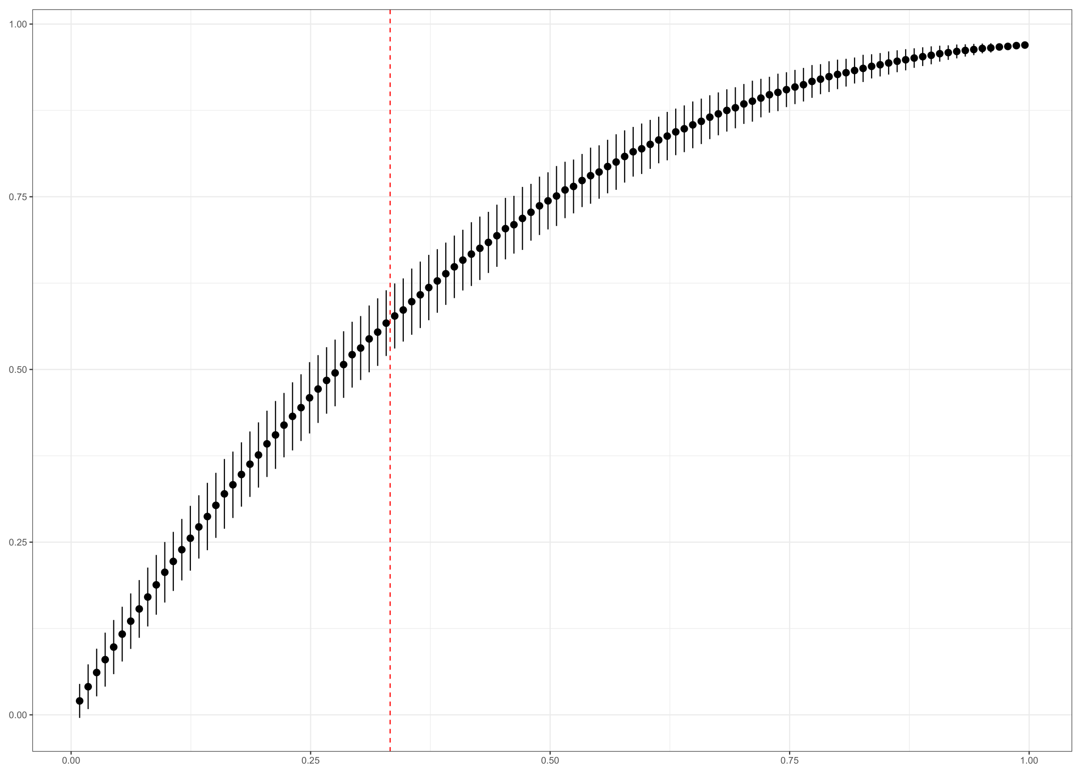
Appendix Tables and Figures

Figure A1: State of Santa Catarina and the experimental sites where the BWC intervention was implemented



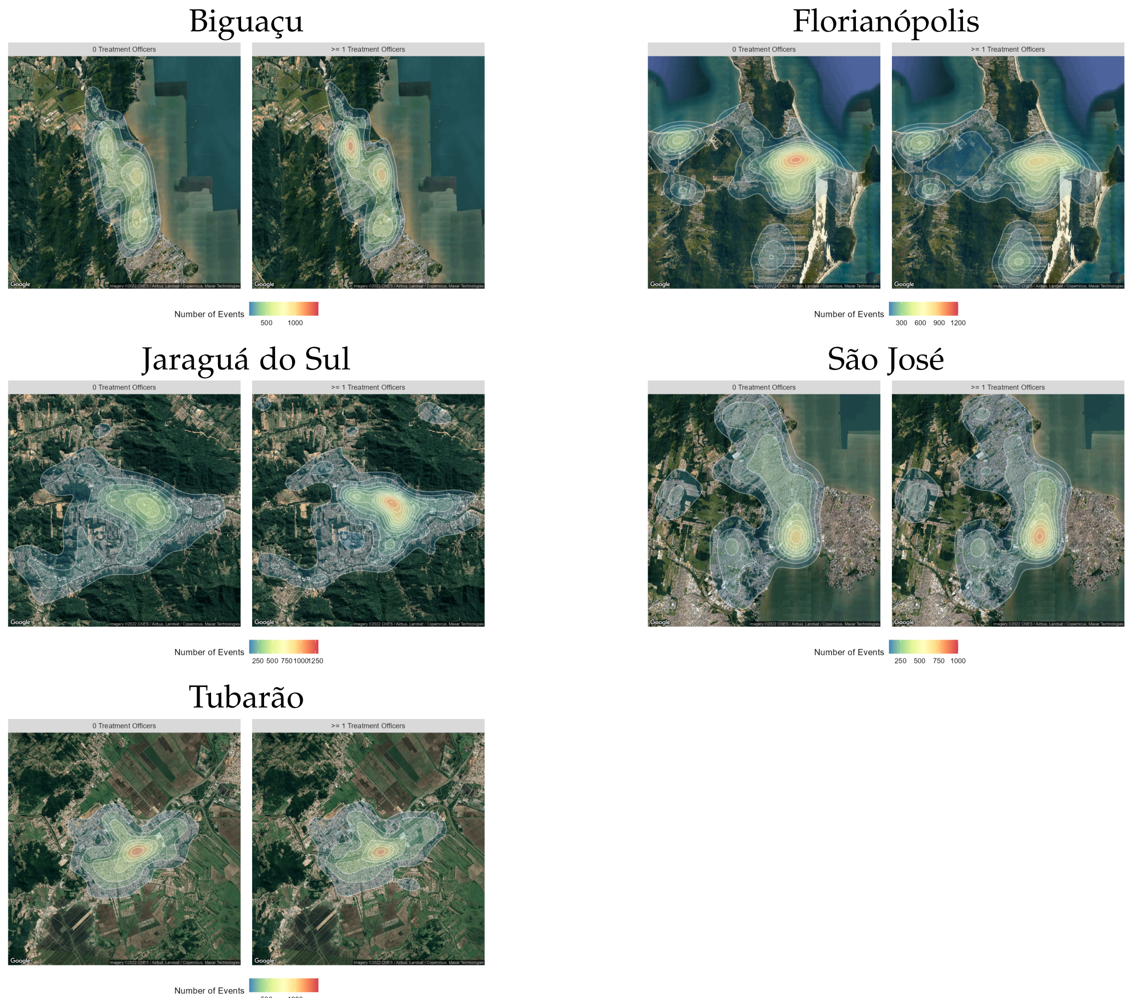
Notes: The figure presents the experimental sites on which the experiment took place. These are the catchment areas of the 24th Police Precinct in Biguaçu, 21st in Florianópolis, 14th in Jaraguá do Sul, 7th in São José and 5th in Tubarão.

Figure A2: Induced treatment allocation at the event-level from officer-level camera randomization



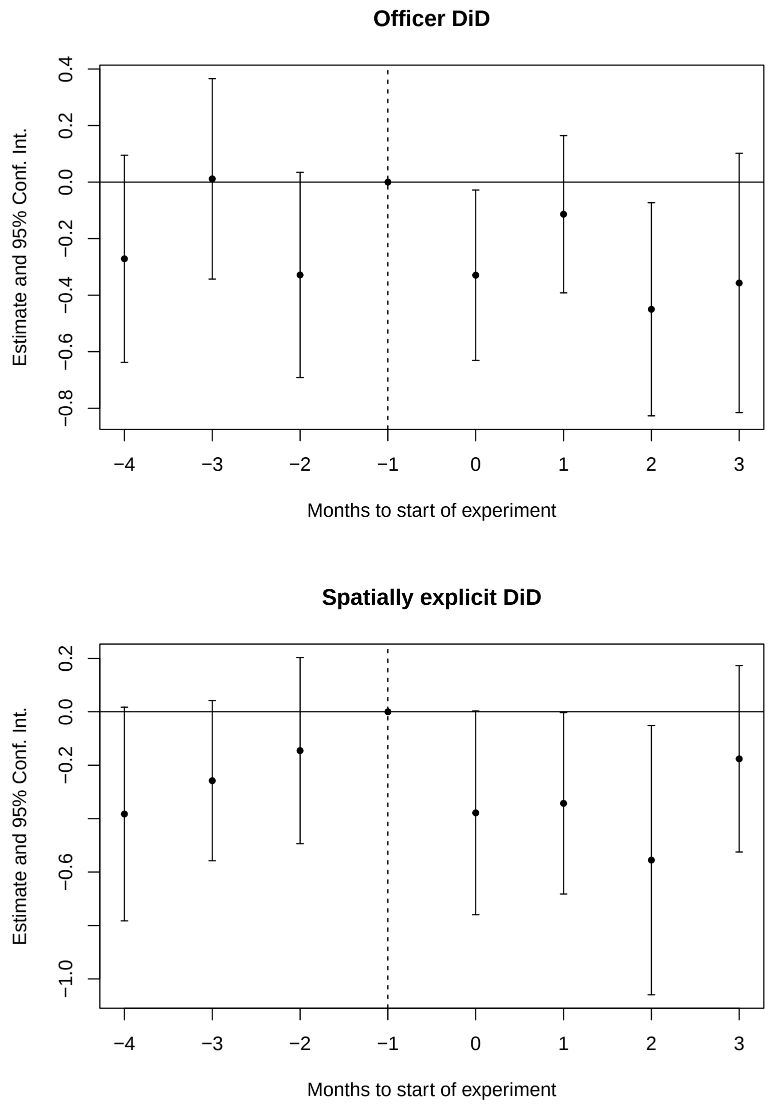
Notes: Figure displays simulation results that were used to identify the optimal share of treated officers to ensure 50% of the dispatches would be treated by virtue of having at least one camera attending the dispatch. The horizontal axis captures the share of officers assigned to receive treatment while the vertical axis plots the share of events that are treated by at least one camera at the dispatch. The vertical dashed line indicates the experimental design chosen whereby 1/3 of the officers are assigned to wear a camera while 2/3 are control group officers that never wear a camera themselves.

Figure A3: Spatial Distribution of Treatment and Control Dispatches



Notes: The figure presents kernel density estimates of the spatial distribution of treatment and control events across the 5 cities that were part of the experiment. It highlights that the spatial distribution of both treatment and control event dispatches is very similar throughout and highlights the different topographies of the study area.

Figure A4: The effects of BWC on use-of-force: two event-studies



Notes: The figures show event-study estimates of the effects of BWC. The first explores variation between treated and control officers and the second variation between treated and control precincts. The point estimates are the coefficient of the treatment unit interact with period. The officer-level DiD regression uses officer, precinct, week and weekday fixed effects and standard errors are clustered at the precinct-week level. The spatially explicit DiD uses precinct, week, and weekday fixed effects and the standard errors are clustered at the precinct-week level.

Table A1: Correlation between event characteristics and officer rank

	High Risk
	(1)
Event with High-rank Officer	0.939
	(1.069)
Mean Dep. Var. Events Only With Soldiers	9.820
N	13274

Notes: The table shows the correlation between the presence of a high-rank officer in an event and the ex-ante level of risk of an event. High-risk is the ex-ante risk assessment indicator used in Table 1. Specifications include police precinct-by-week, day of the week, number of officers and stratification bins fixed effects. Standard errors are clustered at the precinct-by-week level. *** p<0.01; ** p<0.05; * p<0.1.

Table A2: Summary Statistics of study sites

	Biguaçu	Florianópolis	Jaraguá Do Sul	São José	Tubarão	SC average
Panel A. Socioeconomic Characteristics						
Population	58,206	421,240	143,123	209,804	97,235	18,468
						(42,990)
Urban (%)	0.904	0.964	0.932	0.989	0.907	0.599
						(0.231)
Income	1,208.22	2,578.28	1,586.99	1,692.74	1566.36	1,127.35
						(236.72)
White (%)	0.836	0.846	0.864	0.844	0.908	0.829
						(0.103)
Primary school or less (%)	0.292	0.623	0.594	0.574	0.656	0.571
						(0.082)
High school or less (%)	0.797	0.959	0.941	0.937	0.965	0.940
						(0.025)
Water access (%)	0.995	0.999	0.996	0.999	0.995	0.987
						(0.023)
Computer (%)	0.490	0.727	0.585	0.661	0.569	0.365
						(0.110)
Internet (%)	0.391	0.650	0.427	0.564	0.462	0.248
						(0.101)
Panel B. Violence and Use-of-Force Incidence						
Use-of-Force Incidents	23	52	34	62	22	-
Crime Events	739	2135	2622	3097	1309	-
Homicide Rate per 100k	22.9	17.16	5.38	16.9	9.65	-
Use-of-Force - Yearly Rate per 100k	106.90	33.39	64.27	79.95	61.21	-
Crime Events - Yearly Rate per 100k	3,435.05	1,371.276	4,956.55	3,993.78	3,642.28	-

Notes: Socio-demographic characteristics and baseline violence across the five study sites and the average in Santa Catarina state. Sociodemographic data from 2010 IBGE Census, Homicide Rate from the 2016 IPEA Atlas da Violência and use-of-force and crime events incidence from author's calculations using PMSC data from March to July, 14th 2018. Income in Brazilian Reais per month. Standard errors in parenthesis.

Table A3: Dispatch Composition: Officer Rank

		Reporting Behaviour		Interaction Margins			
	Dispatch Recorded	Police Report	Victims in report	Negative Interaction Index	Contempt, Resistance and/or Disobedience	Use-of-force	Handcuff and/or Arrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated Event x All Soldiers	26.373***	2.910**	2.780***	-0.377**	-0.494**	-0.320**	-0.247
	(2.110)	(1.337)	(0.959)	(0.156)	(0.235)	(0.153)	(0.591)
Treated Event x At Least 1 Above Soldier Rank	18.526***	3.638**	2.838**	-0.357	0.285	-0.670**	-0.469
	(1.832)	(1.558)	(1.224)	(0.278)	(0.305)	(0.319)	(0.823)
Mean Dep. Var. Control Event	0.000	32.761	13.832	0.790	0.932	0.694	5.427
N	13274	13274	13274	13274	13274	13274	13274

Notes: Intention-to-treat specifications. Unit of observation is a police event. Dependent variables defined as in Table 1. Specifications include police precinct-by-week, day of the week, number of officers and stratification bins fixed effects. Shifts without camera are excluded from the regression. Standard errors are clustered at the precinct-by-week level. *** p<0.01; ** p<0.05; * p<0.1.

Table A4: Sample Robustness

		Reporting Behaviour		Interaction Margins			
	Dispatch Recorded	Police Report	Victims in report	Negative Interaction Index	Contempt, Resistance and/or Disobedience	Use-of-force	Handcuff and/or Arrest
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Main Result							
Event treated	24.043***	3.101**	2.783***	-0.371**	-0.263	-0.425***	-0.320
	(1.873)	(1.190)	(0.805)	(0.149)	(0.196)	(0.157)	(0.471)
Mean Dep. Var. Control Events	0.000	32.761	13.832	0.790	0.932	0.694	5.427
N	13274	13274	13274	13274	13274	13274	13274
Panel B. Including Blackout Days							
Treated Event	18.852***	3.100***	2.180***	-0.268**	-0.190	-0.307**	-0.171
	(1.521)	(1.097)	(0.652)	(0.113)	(0.172)	(0.120)	(0.429)
Mean Dep. Var. Control Events	0.000	33.158	14.652	0.751	0.901	0.652	5.420
N	17665	17665	17665	17665	17665	17665	17665
Panel C. Two Officers - Modal Dispatch Size							
Treated Event	24.140***	4.036***	3.365***	-0.271**	-0.371*	-0.222*	-0.158
	(1.930)	(1.328)	(0.897)	(0.125)	(0.205)	(0.114)	(0.474)
Mean Dep. Var. Control Events	0.000	30.569	13.173	0.557	0.810	0.416	3.807
N	9928	9928	9928	9928	9928	9928	9928
Panel D. At Most Four Officers							
Treated Event	24.061***	3.196**	2.947***	-0.325**	-0.285	-0.344***	-0.427
	(1.859)	(1.236)	(0.848)	(0.125)	(0.177)	(0.128)	(0.433)
Mean Dep. Var. Control Events	0.000	32.335	13.600	0.698	0.856	0.595	5.153
N	12546	12546	12546	12546	12546	12546	12546

Notes: Intention-to-treat specifications. Unit of observation is a police event. Dependent variables defined as in Table 1. Specifications include police precinct-by-week, day of the week, number of officers and stratification bins fixed effects. Shifts without camera are excluded from the regression. Standard errors are clustered at the precinct-by-week level. *** p<0.01; ** p<0.05; * p<0.1.

Table A5: Characteristics of Notable BWC Studies in the Literature

Paper	**Year** (1)	**# Citations** (2)	**Country** (3)	**(Quasi) Experiment** (4)	**Unit of (quasi) randomization** (5)	**N** (6)	**T** (7)	**C** (8)	**Share of Treated Units** (9)	**Avg # of officers per dispatch** (10)	**Analysis Unit** (11)	**Time dimension** (12)	**UoF as outcome?** (13)	**Effects on UoF** (14)	**Empirical strategy** (15)
Panel A: Shift centric studies (7 studies)															
Ariel et al. (2015)	2014	633	US	Yes	Shift	988	489	499	0.495	1	Shift	None	Yes	Decrease	Poisson regression
Ariel et al. (2016a)	2016	178	US	Yes	Shift	4,915	2,447	2,468	0.498	-	Shift	None	Yes	Null	Means test
Ariel et al. (2016b)	2016	221	US	Yes	Shift	4,915	2,447	2,468	0.498	-	Shift	None	Yes	Null	Means test
Ariel et al. (2017)	2017	187	US	Yes	Shift	3,882	1,908	1,974	0.491	-	Shift	None	No	-	Means test
Henstock and Ariel (2017)	2017	78	UK	Yes	Shift	430	215	215	0.500	1	Shift	None	Yes	Decrease	Odds-Ratio
Ariel et al. (2018)	2018	48	US	Yes	Shift	4,915	2,447	2,468	0.498	-	Shift	None	No	-	Odds-Ratio
Magaloni (2019)	2019	-	BR	Yes	Unit-shift	21,472	16,390	18,642	0.468	1+	Officer-shift	Shift	Yes	Null	OLS
Panel B: Officer centric studies (13 studies)															
Jennings et al. (2015)	2015	243	US	Yes*	Officer	89	46	43	0.517	1+	Officer	None	Yes	Decrease (check)	% change
Ready and Young (2015)	2015	239	US	Yes*	Officer	3,698	50	50	0.500	1+	Contact report	None	No	-	HGLM
White et al. (2017)	2017	84	US	Yes*	Officer	298	82	67	0.550	-	Officer	Pre-post	Yes	Null	DiD
Jennings et al. (2017)	2017	62	US	Yes	Officer	120	60	60	0.500	-	Officer	Pre-post	Yes	Decrease	% change **
Headley et al. (2017)	2017	89	US	Yes*	Officer	103	26	25	0.510	-	Officer	Pre-post	Yes	Null	% change
Braga et al. (2017)	2017	62	US	Yes*	Officer	832	218	198	0.524	1	Officer	Pre-post	Yes	Decrease	DiD
Braga et al. (2018)	2018	107	US	Yes*	Officer	832	218	198	0.524	1	Officer	Pre-post	Yes	Decrease	DiD
Peterson et al. (2018)	2018	-	US	Yes	Officer	504	252	252	0.500	-	Officer	Pre-post	Yes	Null	DiD
Wallace et al. (2018)	2018	64	US	Yes	Officer	228,220	82	67	0.550	1+	Call-officer	None	No	-	DiD
Yokum et al. (2019)	2019	32	US	Yes	Officer	1,922	1,189	1,035	0.535	-	Officer	None	Yes	Null	OLS between officer
Koslicki et al. (2020)	2020	10	US	No	-	-	-	-	-	-	Officer * Month	-	Yes	Null	Time series analysis
Braga et al. (2020)	2020	9	US	Yes	Officer + District	562	140	141	0.498	1	Officer	Pre-post	Yes	Decrease	DiD
Braga et al. (2020)	2020	-	US	Yes	Officer + Precinct	7,778	1,991	1,898	0.512	-	Officer	Pre-post	No	-	DiD
Panel C: Spatially explicit designs (13 studies)															
Katz et al. (2014)	2014	170	US	No	Area	2	1	1	0.500	-	Area	None	No	-	Means test
Grossmith et al. (2015)	2015	107	UK	Yes	Team	2,060	814	1246	0.395	1+	Officer	None	No	-	Means test
Morrow et al. (2016)	2016	104	US	No	Area	4	1	1	0.500	-	Area	Pre-post	No	-	Means test **
Ariel (2016a)	2016	79	US	No	District	17,726	1	5	0.167	2	Street segment	Pre-post	No	-	Means test
Ariel (2016b)	2016	92	US	No	Area	924,457	1	5	0.167	-	Call	Pre-post	Yes	Null	Odds-Ratio
Hedberg et al. (2017)	2017	160	US	No	-	44,380	22,660	22,720	0.499	1+	Incident	None	No	-	GLM
Mitchell et al. (2018)	2018	10	UY	No	Region	38	5	14	0.263	-	Region	Pre-post	No	-	Means test
Owens and Finn (2018)	2017	32	UK	Yes	Team	-	814	1,246	0.395	1+	-	-	No	-	-
Bennett et al. (2019)	2019	-	US	No	Squad areas	2	1	1	0.500	-	Squad * Week	Week	Yes	Null	Diff. in trends test
Stolzenberg et al. (2019)	2019	6	US	No	-	1	-	-	-	-	Monthly rates	Month	No	-	None **
Kim (2021)	2021	-	US	Yes	Agencies	96	21	75	-	-	Agency * Month	Month	Yes	Decrease	DiD
Miller and Chillar (2021)	2021	2	US	Yes	Agencies	2,376	1,346	1,030	-	-	Agency * Year	Year	No	-	DiD
Bollman (2021)	2021	-	US	Yes	Courts	103	70	33	-	-	Court * Quarter	Quarter	No	-	DiD

Notes: Table provides a non-exhaustive overview of some of the existing empirical literature on BWC. The overview does not claim to be comprehensive but has aimed to include all empirical studies evaluating BWCs across a broad range of fields from criminology to economics. In case a randomization unit is indicated with * next to a Yes it means that the officers included are partially self-selected into the experiment implying that caution needs to be put on detected effects as these could be quite specific LATE estimates. The table focuses on the respective randomization design, the outcome measurement approach, empirical strategy employed and whether effects on use-of-force (UoF) have been identified. Empirical strategies chosen often do not follow more rigorous program evaluation techniques, and the studies that do not perform statistical inference have **. Not in all cases was it possible to infer all required input and only two papers have replication data available.

Table A6: Registered Hypotheses in the Nov 2018 PAP

	Main Hypothesis	Sub-hypothesis
H1	BWC reduces use-of-force incidents	by a) officers wearing a camera; b) officers in the same patrol group as those wearing a camera; c) officers attending an event where at least one officer was wearing a camera.
H2	BWC reduce civilian complaints against officers	by a) officers wearing a camera; b) officers in the same patrol group as those wearing a camera; c) officers attending an event where at least one officer was wearing a camera.
H3	BWC reduce use-of-force incidents by police officers that had in the past	a) worn a camera; b) patrolled with officer that had worn a camera; c) attended an event where one officer was wearing a camera.
H4	BWC reduce dispatch time	a) wearing a camera; b) patrolling with an officer wearing a camera.

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